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The Institution is not, as a body, responsible for the opinions expressed by individual authors or speakers.

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Hundred and Forty-fourth

ERRATA.

Messrs. Morris and Lister's paper, "On Transformers and Transformer Iron," Vol. 37, page 273.

On line 5 *for* $i = \frac{w_1}{e_1} \cos \phi$ *read* $i = \frac{w_1}{e_1 \cos \phi}$

On line 10 *for* $i = \frac{w_1}{\mathrm{E}} \cos \phi$ *read* $i = \frac{w_1}{\mathrm{E} \cos \phi}$

approved by the Council :—

TRANSFERS.

From the class of Associate Members to that of Members—

David O. Evans. | Frederick W. Mills.
F. A. Wilkinson.

From the class of Associates to that of Associate Members—

Francis G. C. Baldwin.
Wm. H. Barker.
Harry U. Collins.
Arthur R. Cooper.
Claud Crompton.
John T. Irwin.
Frederic D. Latimer.
H. H. S. Marsh.
Frederick K. M. Nicholl.
W. G. Royal-Dawson.
Hugh Sharman.
Edward A. Short.
Jas. A. Walker.
Wm. B. Walker.

From the class of Students to that of Associate Members—

J. D. K. Restler. | Arthur B. Scorer.
Arthur A. Watkins.

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Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. 38. 1907. No. 181.

Proceedings of the Four Hundred and Forty-fourth Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, November 8, 1906—Mr. JOHN GAVEY, C.B., President, in the chair.

The minutes of the Annual General Meeting held on May 24, 1906, were taken as read, and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

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From the class of Associate Members to that of Members—

David O. Evans.
Frederick W. Mills.
F. A. Wilkinson.

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Jas. A. Walker.
Wm. B. Walker.

From the class of Students to that of Associate Members—

J. D. K. Restler.
Arthur B. Scorer.
Arthur A. Watkins.

The PRESIDENT: Before opening the proceedings I have a pleasant announcement to make. As most of you are aware, I have just spent some four or five weeks in Berlin, and whilst there I was invited to attend a meeting of the Electrotechnischer Verein, at which a paper was read by Mr. Poulsen, the well-known inventor of the telegraphone, on A New Method of Wireless Telegraphy, which I am glad to say was founded on some original researches made by one of our members, Mr. Duddell. His Excellency von Sydow, the President of that Institution, taking advantage of my presence there, made a statement, in which he expressed the gratification of the German members at the hospitality which was extended to them during their recent visit to this country; and he desired to thank the Institution, through me, for that hospitality. It was a gratifying incident, which preceded an interesting address.

Mr. JOHN GAVEY then presented the Premiums and Scholarships referred to in the Annual Report for the year 1905–6.

Mr. Gavey then vacated the chair, which was taken by Dr. R. T. Glazebrook, F.R.S., the new President.

The PRESIDENT (Dr. R. T. Glazebrook): Gentlemen, my first duty as President is to ask Mr. Siemens to be good enough to propose a Resolution.

Mr. ALEXANDER SIEMENS (Past President): Gentlemen, it is my pleasing duty to propose, "That the best thanks of the members of the Institution of Electrical Engineers be given to Mr. John Gavey, C.B., for the very able manner in which he has filled the office of President during the past twelve months." It is hardly necessary for me to say many words in proposing this resolution. Mr. Gavey, I have no doubt, has during his presidency experienced the usual troubles which all Presidents have; but we must bear in mind the fact that, in addition, he has been a very able and efficient host to the foreign Societies which we invited to visit this country in June of this year. He has just told us how the German Society thanked him when he was in Berlin for the way in which the members of that Society were received, and I am sure you all know how very much Mr. Gavey personally contributed to the success of that gathering. I have all the more pleasure in proposing a vote of thanks to him for the efficient manner in which he performed the duties of his office because some of you may remember that I had the honour of introducing him to you last year. I have very much pleasure in moving the resolution.

Mr. W. M. MORDEY (Vice-President): Sir, the duty devolves upon me—and I am very glad it does—of seconding the proposal that has just been made. We must not forget, in speeding our parting President, that this Institution began as the Institution of Telegraph Engineers. This year in particular it has been very fitting that our President should have been a distinguished member of the oldest branch of our profession. England is the home of submarine telegraphy, which was developed in an endeavour to keep in touch with our foreign friends. This year we have tried to make that connection still closer, as you

know, by conducting our foreign guests round our country, under the guidance of our President, and showing them some of the developments of electrical engineering here. I am sure we all wish to thank him now, at the end of his year of office, for the way in which he has carried out his unusually onerous duties, for the kindliness and the dignity with which he has conducted all our meetings, and for the way in which he has represented this Institution at home and abroad. I have great pleasure in seconding the proposal of Mr. Siemens.

Mr. J. GAVEY: Gentlemen, I thank you very heartily for the kind manner in which you have received the proposal of Mr. Siemens and Mr. Mordey. I am very proud of having had the honour of presiding over this great Institution during the past Session. If I have not wholly failed in conducting the work of the Institution, I owe what little success I may have achieved very largely to the hearty co-operation and goodwill of my friends and colleagues on the Council, and, in the second place, to the consideration you have been good enough to show me in conducting the debates of the Institution. Again, gentlemen, I thank you.

The PRESIDENT then delivered his Inaugural Address.

INAUGURAL ADDRESS.

By Dr. R. T. Glazebrook, F.R.S., President.

Gentlemen,—Let me commence my address by thanking you very sincerely for placing me in this chair. The post to which you have called me is a most honourable one. I value the distinction highly, and accept the responsibility it entails with the full intention of doing all I can to promote the best interest of the Institution, and with the sure hope that in this endeavour I shall have the support of its members.

Since the last Presidential Address we have lost some of our members by the hand of death; among these I would specially remind you of the name and work of Carl Heinrich von Siemens. Born in 1829, he was the sixth in a family of eleven sons, and his work for engineering science is closely bound up with that of his three brothers, William, Werner, and Friedrich. After representing the firm of Siemens and Halske for some time in St. Petersburg, he settled in London in 1869, and took an active share in the business of Messrs. Siemens Bros. He took charge of the new cable ship *Faraday* on her first voyage, and was the first to succeed in recovering from the Atlantic a broken cable. After eleven years in England he returned to Russia, but in 1892 settled in Berlin as head of the firm of Messrs. Siemens and Halske. He was a Vice-President of our Institution, having been a member from the time of its foundation.

During this year our progress has been marked; the Annual Report of the Council gives full statistics. Our members have increased and now reach 5,800. The pages of the *Journal* contain evidence of the value of our work and of the wide scope of the interests with which we are concerned; while among important matters which are not yet completed I should like to refer to the work of the Wiring Rules Committee, of which Mr. C. P. Sparks, Vice-President, is the Chairman, and to the reorganisation of the Library, which is being carried through at the cost of much personal labour by Mr. Duddell, Chairman of the Library Committee, Professor Thompson, and Mr. Mather.

During the autumn of the past year a very successful Electrical Exhibition was held at Olympia. The Council were glad to accept the position of patrons to the Exhibition, and gratefully acknowledge the kindness of the Organising Committee in handing a large donation from their balance to the Benevolent Fund of the Institution.

Nor is it only in London that our activities have shown themselves. Throughout the country the number of our members is becoming

larger, and the papers which are contributed to their meetings are growing in importance and in value.

Those of us who were fortunate enough to take part in the tour last July were gratified and pleased at the strength and vigour of our Local Sections in the great towns we visited, and at the positions which prominent members of those Sections had won for themselves.

But this same growth brings its own difficulties. The question how best to utilise the various forces of the Institution for the benefit of the whole is ever with the Council, and for its solution needs careful thought. Mr. Gray referred to it in his address three years ago. I mention it now, not to discuss it, but merely to express my own view of the importance of securing to each Section liberty of action in its own affairs, while at the same time giving to the central body the strength which it can only possess through gaining the full confidence of the Sections, and which is necessary for the welfare of the whole.

It is impossible, I know, for the Chairmen of our Local Sections to attend meetings of the Council or its Committees with the regularity of London members. May I take this opportunity of telling them that they are always very welcome and of assuring them on behalf of their colleagues that their experience of local needs is most valuable to the Council, and that suggestions which they may make for adding to the usefulness of the Institution will be most carefully considered?

And in reviewing very briefly the events of the past year I should like to thank the authors of the various papers which have been contributed to our *Proceedings*. It is no light task for busy men like those whose names appear in this volume to write papers such as we have listened to. Their labours have added to the sum of our knowledge, and have advanced both the principles and practice of electrical engineering. The large attendance at our meetings, the number of speakers, and the numerous points raised in the discussions, all testify to the interest and importance of their work.

But while the ordinary path of the Institution has been full of interest, the visit of our kindred societies from other lands stands out as the marked feature of the year. The story of that visit is well known to you all; still, it seems right to put on record some few facts relating to it. The desirability of inviting to England many friends whose hospitality we had ourselves enjoyed, and by whose experience we had so greatly benefited, was realised as soon as the question was mooted, and the generous response made to the first suggestion of the Council quickly brought the matter within the bounds of practical politics. The invitation of the Institution was accepted by some two hundred guests,* and their visit was, we may fairly claim, a great success. So far as the country tour was concerned, that success was due in no small measure to our Local Sections, and to the support and assistance they received

* The invitation was issued to and accepted by the following associations: The American Institute of Electrical Engineers, The Canadian Electrical Association, The Société Internationale des Electriciens, The Elektrotechnischer Verein, The Verband Deutscher Elektrotechniker, The Associazione Elettrotecnica Italiana, The Schweizerischer Elektrotechnischer Verein.

from the municipal authorities in the various towns which we visited. This support changed the whole character of the tour. Everywhere it became a public event, and the enthusiasm with which the local authorities took the matter up, the trouble they gave themselves to make it a success, and the generous hospitality they offered to our guests, were preponderating factors in determining the results of the visit. As our President said at Glasgow, the courtesies we received converted the private hospitality of the Institution into an act of international courtesy.

The pleasure of our visit to Glasgow was enhanced in no small measure by the fact that Lord Kelvin received us as President of the Local Section, and the presentation which the Italian delegates made to him was an act of homage which gave the deepest satisfaction to us all.

Formal thanks have gone from the Council to those who entertained us. I feel, however, that you would wish me to express publicly to our kind hosts acknowledgments of their help.

The Council, too, at its meeting in July passed a very cordial vote of thanks to our ex-President and to Mrs. Gavey for all they did to contribute to the success of the visit; to Mr. Gray, whose ready tact and courtesy smoothed over every difficulty, and whose only thought seemed to be how to make all around as contented as himself; and to the officials of the Institution, who throughout interpreted in the most admirable manner the wishes of the Council, and devoted themselves in the most whole-hearted way to giving those wishes effect. You, I am sure, whom I am now addressing, will confirm these votes and express to all whom I have mentioned your hearty thanks for their services to our Institution.

My own scientific work for many years past has been concerned so largely with units of measurement, standards, and standardisation, that you will pardon me, I trust, if I ask your attention for a brief time to the early history and to some of the present aspects of these and allied problems.

I hope that no apology is needed for the choice. Standards and exact measurement do not appeal to all. Some weeks ago a distinguished visitor was looking in my laboratory at a string of figures which represented the results of a long series of measurements. The figures were given to five places, and his remark was, "If you really wish to see what the differences between your individual experiments are, you will have to calculate another place of decimals." I fear that many present will hardly appreciate fully the pleasure those words gave to his listeners; and yet the history of standardisation is most interesting, and the account of the progress whereby the present high state of accuracy has been reached, from the days when a cow or sheep was the unit of value, or a barleycorn a measure of length, through those in which the yard was determined by the stretch of the sovereign's arm, up to our own times, is a fascinating one. On the general subject reference may be made to a recent work—"The Evolution of Weights and Measures," by Hallock and Wade.

In our own subject the story is comparatively short. The first Report of the Electrical Standards Committee of the British Association* is dated October 3, 1862. It deals with the value of the unit of resistance and incidentally establishes the absolute system of electrical units.

The entire connection between the various units, the Report says, may be summed up as follows:—

"A battery or rheomotor of unit electromotive force will generate a current of unit strength in a circuit of unit resistance, and in the unit of time will convey a unit quantity of electricity through this circuit and do a unit of work or its equivalent."

The words sound very simple and elementary in 1906; it is only forty-four years since they were penned and electrical measurements began.

An absolute determination of the unit of resistance had already been made by Weber; the Committee proposed to make a fresh determination, and they state: "If Professor Weber's results accord within 1 per cent. with these new determinations, it is proposed that provisional standards shall be made of German silver wire in the usual way, and that they should at once be issued to all interested in the subject without waiting for the construction of the final material standard." We are now striving to multiply this accuracy one thousand times.

Next year Joule and Maxwell appear as members of the Committee, and the Reports of 1863 and 1864 contain accounts of the first experiments for the determination of the ohm in absolute measure carried out by Maxwell and Fleeming Jenkin in the laboratory at King's College; while in Appendix C to the Report of 1863 is to be found "a full explanation of the meaning of absolute measurement, and of the principles by which absolute electrical units are determined," which the Committee thought fit "should form part of the present Report, especially as the only information on the subject now extant is scattered in detached papers by Weber, Thomson, Helmholtz, and others, requiring considerable labour to collect and understand." This Appendix on the Elementary Relations between Electrical Measurements is by Clerk Maxwell and Fleeming Jenkin, and is a model of what such an account should be.

In 1865, we read, the Committee has the pleasure of reporting that the object for which they were first appointed has now been accomplished. The unit of electrical resistance has been chosen and determined by fresh experiments; the standards have been prepared, and copies of these standards have been made with the same care as was employed in adjusting these standards themselves. Of these copies, ten made of various alloys were prepared with great care and deposited at the Kew Observatory. It is interesting to note that after various journeyings they have now come back, in a sense, to their original destination: they are in my charge at the National Physical Laboratory.

* The members of the Committee were Professor Williamson, Professor Wheatstone, Professor W. Thomson, Professor Miller, Dr. Matthiessen, and Mr. F. Jenkin.

Fleeming Jenkin, in his Report to the Royal Society on the new unit of resistance proposed and issued by the Committee on Electrical Standards appointed in 1861 by the British Association, gives a history of the whole subject, starting from the days of Lenz, who in 1833 stated that 1 foot of No. 11 copper wire was his unit of resistance, and Wheatstone, who in 1843 proposed 1 foot of copper wire weighing 100 grains, not only as a unit but as a standard of resistance, until the time when in England the unit was the mile of No. 16 copper wire, in Germany the mile of No. 8 iron wire, and in France the kilometre of iron wire 4 millimetres in diameter, down to the mercury unit of Werner von Siemens, to which in our recent legislation we have so nearly returned.

The question of electrical standards in its present phase was last brought before us by Mr. Duddell in his very able Report on the proceedings at the St. Louis Conference, and occupied us for a large part of two evenings in April of last year. You will remember that in the course of Mr. Duddell's Report he referred to two important resolutions passed at St. Louis, and the question of giving effect to these was considered. The first resolution was as follows :—

"It appears from papers laid before the International Electrical Congress, and from the discussion, that there are considerable discrepancies between the laws relating to electric units, or their interpretation, in the various countries represented, which in the opinion of this Chamber require consideration with a view to securing practical uniformity. Other questions bearing on nomenclature and the determination of units and standards have also been raised on which, in the opinion of this Chamber, it is desirable to have international agreement.

"The Chamber of Delegates considers that these and similar questions could best be dealt with by an International Commission representing the Governments concerned. Such a Commission might in the first instance be appointed by those countries in which legislation on electric units has been adopted, and consist of (say) two members from each country. Provision should be made for securing the adhesion of other countries prepared to adopt the conclusions of the Committee."

At the discussion I was able to announce that His Majesty's Government were prepared to assist by nominating delegates to such a Conference, and possibly by more active aid than that.

Since that date the matter has progressed very considerably. In October, 1905, a Conference of representatives of standardising laboratories was, by the very kind invitation of Professor Warburg, President of the Reichsanstalt, held at Charlottenburg, to consider the position of the subject and to discuss the questions which might come before such a Congress if it were held.

Representatives were present from America, Austria, Belgium, France, Germany, and Great Britain, and Professor Mascart, of Paris, was unanimously invited to take the chair. After very careful discussion the following resolution was adopted :—

"In view of the fact that the laws of the different countries in

relation to electrical units are not in complete agreement, the Conference holds it desirable that an official Conference should be held in the course of a year with the object of bringing about this agreement."

This result was reported to the Government, and the Foreign Office issued invitations to a Conference for this purpose, to be held, probably, in October,* 1907.

These invitations have been very cordially received, and there is little doubt but that about a year hence this important Congress will be held in London.

And now as to some of the matters which will come before the Congress when it is held, and which were considered at Berlin last year.

Of the three units of resistance, current, and electromotive force, two only are independent, and it will be remembered that at St. Louis, while it was agreed that the unit of resistance must be one of these, there was some difference of opinion as to whether the volt or the ampere should be the second. This question was considered at Berlin. Professor Carhart strongly pressed the view that the volt—defined for practical purposes by the E.M.F. of some standard cell—should be the second, but in this he found little support, and in the end the two following decisions were come to:—

1. That only two electrical units shall be chosen as fundamental units.

2. The international ohm, defined by the resistance of a column of mercury, and the international ampere, defined by the deposition of silver, are to be taken as the fundamental electrical units.†

Accepting, then, these two resolutions, a definition of the international volt is required, and that is given by a third resolution.

3. The international volt is that electromotive force which produces an electric current of one international ampere in a conductor whose resistance is one international ohm.

But this resolution by itself does not enable us to realise the volt in a concrete manner except by the measurement of a current and a resistance; some reference to a standard cell is required, and that is afforded by Resolution 4.

4. The Weston cadmium cell shall be adopted as the standard cell.

It will be noted, however, that these resolutions by themselves do not define the relation between these international units and the absolute C.G.S. units. It is understood that the international ohm, ampere, and volt represent, at any rate with sufficient exactness for practical purposes, the ohm, the ampere, and the volt of the C.G.S. system, or 10^9 C.G.S. units of resistance, 10^{-1} C.G.S. units of current, and 10^8 C.G.S. units of electromotive force respectively. But for this purpose the dimensions and temperature of the mercury column, the weight of silver

* The date October, 1906, was first suggested, but consideration showed that it was too soon to enable the necessary preliminary work to be done.

† These decisions, it should be noted, are in harmony with a resolution of the Electrical Standards Committee of the British Association passed on October 19, 1905, to the effect "That two units should be defined independently, and that these two should be the unit of resistance and the unit of current."

deposited under definite conditions in a given time, and the E.M.F. in C.G.S. units of the standard cell must all be found.

These matters were brought under consideration in turn. In the case of the ohm, while there was no proposal to modify the statement of the dimension of the mercury column formulated at Edinburgh in 1892, some important conclusions were reached as to the best method of constructing practical standards having a resistance of 1 ohm.

With regard to the ampere and the volt, it was felt that though recent work had made uncertain the numbers generally accepted for the weight of silver deposited per second by 10^{-1} C.G.S. units and the E.M.F. of the Clark or Weston cell, there was not sufficient evidence available at that time to settle these questions, and the opinion was therefore expressed by the Conference—

1. That the information before it is not sufficient to enable it to propose any alteration in the formerly accepted value for the ampere.

2. That the information before it is not sufficient to enable it to lay down exact directions in respect to the silver voltameter and the standard cell.

While, finally, the opinion was expressed by the Conference that on such matters as the exact value of the electro-chemical equivalent of silver or the procedure to be followed with regard to the use of the silver voltameter or the construction of the standard cell, it was important that agreement should be reached between the Standardising Institutions concerned before a formal Conference was asked to decide on them, and that if such agreement was not reached by correspondence a new preliminary Conference should be held.

Accordingly, since October last experiments have been going on in various countries to decide the questions left open.

Professor Ayrton described during the discussion already referred to, the balance, the electrical part of which has been constructed at the National Physical Laboratory from the designs of himself and Mr. Mather as given in working drawings made by Mr. Gregory at the Central Technical College. Since that date a very large number of results have been obtained by the balance, both for the mass of silver deposited and the E.M.F. of a Weston cell. The material is being prepared for publication, and although some points still need further elucidation, I may say that the agreement reached is most gratifying, and the results will enable us to state both the value of the international ampere and the E.M.F. of the cell in C.G.S. units to a very high degree of accuracy, a degree which, at any rate, is far beyond anything that will be required for commercial purposes for years to come.

At the same time it must be remembered that we require to know the value of our standards to a much higher order of accuracy than that to which they are used in order to be certain of our results. Much of the rapid progress of the electrical industry is due to the fact that the quantities dealt with are capable of very exact measurement, and I trust that we shall never underrate the importance of great exactness in all fundamental measurements.

And now let me turn to another aspect of standardisation. It appears that it is not only necessary that the fundamental units of our science should be the same throughout the world; of recent years the conviction has grown stronger that the welfare of the nations may be promoted by the extension of this principle, and International Standardisation has become a question of the first importance.

Much of the work of the Engineering Standards Committee is known to all members of the Institution. I propose to return later to some of its aspects. Two years ago Colonel Crompton, the enthusiastic Chairman of one of the sub-committees, brought the question of standardisation before the St. Louis Congress, and that act has had a wide-reaching result, for the Chamber of Delegates at St. Louis took up the question and adopted unanimously the following resolution: "That steps should be taken to secure the co-operation of the technical societies of the world by the appointment of a representative Commission to consider the questions of the standardisation of the nomenclature and ratings of electrical apparatus and machinery."

It was further agreed: "That the delegates report the resolution of the Chamber to their respective technical societies, with the request that the societies take such action as they may deem best to give effect to this resolution; and that the delegates be requested to communicate the result of such action to Colonel R. E. B. Crompton, of Chelmsford, England, and to the President of the American Institute of Electrical Engineers, New York City."

In accordance with this, Colonel Crompton communicated the desire of the Congress to the Institution of Civil Engineers, and after due consideration it was proposed that our Institution should take up the matter by appointing an Executive Committee to consider and report upon a scheme for the constitution of an International Commission, in accordance with the resolution passed by the Chamber of Delegates at the International Congress of St. Louis in 1904, and to pursue any course which might seem desirable to them to enable them to submit a satisfactory report.

Colonel Crompton had already stated that in response to preliminary inquiries, replies favourable to the constitution of such a Commission had been received from the Electrical Societies of France, Hungary, the United States, Italy, Germany, Denmark, Sweden, Norway, and Canada.

This step was willingly taken by your Council, and arrangements were made for communicating with the technical societies of the various countries and organising a preliminary meeting of delegates for the purpose of constituting the proposed International Commission.

The fact that the Institution was welcoming its foreign guests in June last made that time a convenient one, and meetings of the delegates were held on June 26th and 27th under the chairmanship of our ex-President, Mr. Siemens, who had acted as President of the Executive Committee.

The following countries were *represented*, and the list shows how

keen was the interest in the work: America, Austria, Belgium, Canada, France, Germany, Great Britain, Holland, Hungary, Italy, Japan, Switzerland, and Spain; while it was stated that Denmark, Norway and Sweden had accepted the invitation, but that their delegates had not been appointed.

The Chairman explained that the first business of the meeting was to constitute the Commission by adopting a set of rules. A draft which had been provisionally prepared and circulated previously to the delegates was then referred to a sub-committee for detailed consideration, and at an adjourned meeting on June 27th the proposals of the Committee, after further consideration and amendment, were adopted, subject to ratification by the authorities by whom the delegates were appointed.

According to the scheme as thus settled, the Commission is to be known as the International Electrotechnical Commission for the standardisation of nomenclature and ratings of electrical apparatus and machinery; it is formed for the purpose of carrying out the resolution of the Chamber of Government Delegates at the International Electrical Congress of St. Louis in September, 1904, which has already been quoted.

Any self-governing country desiring to join the Commission may form a local committee. These committees are to be formed one for each country, by the technical societies of each country. In a country having no such technical societies, the Government may appoint a committee.

Each committee is to send delegates to the Commission. Each country is entitled to one vote only, whatever the number of delegates which it may send. Only such decisions may be published as those of the International Electrotechnical Commission which have been passed unanimously by the Commission. All decisions passed by a divided vote may be published only when the names of the countries voting for and against are given.

The Central Offices of the Commission are for the present in London, at the office of the Institution of Electrical Engineers. The methods of carrying out the objects of the Commission are in the hands of a Council consisting of (*a*) the President of the Commission, (*b*) the Presidents of the local committees, (*c*) one delegate from each local committee, (*d*) the Honorary Secretary.

In general, the business of the Commission will be conducted by correspondence, but the President may summon a meeting of the Council or of the Commission when he sees fit. A meeting is also to be held if three of the local committees request that this should be done. These meetings are to take place in London, or in such other places as the majority of the Commission determine. Each local committee is to find funds for its own expenses, and to contribute an equal share to the expenses of the Central Office.

A local committee may fix its own rules as it thinks fit, provided these rules are not incompatible with those of the Commission.

At the meeting on June 27th, Lord Kelvin was appointed the first President of the Commission, and Colonel Crompton the first Honorary Secretary.*

In accordance with the provision in the rules requiring that they should be ratified by the technical society representing each country, they were brought before the Council of the Institution in July last, and it was unanimously agreed that "the Council give its adherence to the scheme as drawn up and approved by the Commission." By this means the first British Local Committee was constituted.†

The task before the Commission is a large one. Nomenclature alone may well occupy its attention for a long period, and if we may judge by the labour expended on the work of the Electrical Committees of the Engineering Standards Committee, progress in dealing with the rating of machinery must be but slow. And as to the result, *cui bono?* A more detailed consideration of some of the work of the Engineering Standards Committee will, I believe, afford an answer to this inquiry, and will justify the statement that the advantages to all of a well considered and judicious scheme of standardisation are immense. As to nomenclature there can be no doubt, and though the task is a difficult one it is not, I think, impossible. As to rating machinery, while it must not be forgotten that the conditions of various countries differ widely, and that for this reason, if for no other, any attempt to force on engineers and manufacturers a cast-iron specification with which they must comply is bound to failure, yet we must recognise that these considerations were before those who started this movement. Let me quote from Colonel Crompton's paper as to the extent to which electrical standardisation can usefully be carried. He says:—

"I think we must all agree that electrical standardisation must bear a different meaning to standardisation of the far older and more crystallised types of machinery used by mechanical engineers. It is highly undesirable that any types, patterns, or sizes should be standardised if these are likely in any way to hinder the future development of design; but all who have looked into the matter know how much useful electrical standardising can be carried out in such matters as the settling on correct nomenclature and definitions of certain terms, hitherto used in a somewhat loose way in text-books or in trade lists, in settling standard test conditions, in determining a satisfactory method of measuring the rise of temperature in the parts of electrical machinery that are affected by temperature rise. In addition to this, we all feel that some attempt must be made to standardise sizes in

* I owe this brief abstract of the rules to the kindness of the Secretary.

† The following are the members:—President, Mr. J. Gavey, C.B., President I.E.E. Vice-Presidents, Sir W. H. Preece, K.C.B.; Mr. Alexander Siemens. Members, Lord Kelvin, President of the Commission, Lord Rayleigh, Professor Callendar, Dr. R. T. Glazebrook, Dr. S. P. Thompson, Mr. A. C. Eborall, Mr. M. B. Field, Colonel H. C. L. Holden, Mr. A. P. Trotter, Mr. C. H. Wordingham, Mr. G. H. Baillie, Mr. W. Duddell, Mr. R. K. Gray, Mr. C. H. Merz, Mr. C. P. Sparks, Mr. E. B. Vignoles, Mr. R. Hammond, Hon. Treasurer, I.E.E., and Colonel Crompton, Hon. Sec. to the Commission.

order, if possible, to reduce the number of patterns that now must be kept by manufacturers, and many of which are felt by them to be wholly unnecessary, and which are only demanded because some manufacturers have produced them for special purposes."

Or again: "We wish to standardise nomenclature, frequency, voltage, test conditions, and similar matters, and, if possible, to standardise ratings, so as to minimise, as far as possible, the number of types which the responsible consumer or the consulting engineer can order. By such a standardisation of ratings we can eliminate the special types which the common sense of manufacturers and users has decided are not necessary as standard types. We do this in the interests of manufacturers as well as of users. We do not wish manufacturers to be burdened by an increased number of patterns, which we believe are practically unnecessary, and it is evident that by so limiting the number of patterns we facilitate the rapidity and economy of manufacture."

Contrast this with a story told by Mr. C. F. Scott in the same discussion. He said: "As a foreign engineer expressed it to me, the engineer in his country was somewhat of an artist. He wanted to build up a new plant according to his own ideas. If he simply followed somebody else there was nothing individual in his effort, any more than an artist would deserve merit if he simply copied some one else's pictures. He wanted to build up a new system, and if he could find a voltage or a frequency that was a little better adapted to his particular plan, according to his opinion, why that was undoubtedly the thing to use. These are quite different ideas to what we have here, and I think ours are well worthy of due consideration."

It is this belief that has led to the establishment of the International Commission.

Returning now, however, to our own country, let us consider what advance we have made in this matter. Its dangers and its advantages are equally clear. Have we avoided the one and utilised the other?

A most interesting book has recently been published by Dr. Shadwell, on "Industrial Efficiency." In it he examines in fullest detail the conditions under which industries are carried out in the three leading industrial countries, England, Germany, and the United States; and, to quote from an article in the *Spectator*, "he has made the resultant book as readable as a novel, and considerably more interesting than the average novel, because it is literally a transcript of life."

His general conclusions are not altogether in our favour. After pointing out our debt to the physical advantages consisting in the adjacent deposits of coal and iron first developed in this country, he sums up the effect of the human element involved thus:—

"The American method of work in the industrial sphere is distinguished by the following features: enterprise, audacity, push, restlessness, eagerness for novelty, inventiveness, emulation, cupidity.

The manufacturer aims at extending his business. He takes up novelties, encourages invention, studies the market, tries devices to increase output and diminish cost. Hence, for instance, the standardisation of products, the organisation of the workshops, the demand for highly-educated officers, and the alert control exercised by large combinations. . . . The employed are eager to earn as much as possible and to better themselves. Both are absorbed in their occupation and bend all their energies to it." . . . "The industrial expansion of Germany," he says, "presents another picture. It has been achieved by equally hard work; but the adventurous audacity and restless search for novelty of America have been replaced by steady and watchful effort. . . . Ordered regulation is accepted and applied with infinite pains by the legislative Government departments and private citizens. . . . So the edifice has been built up four square and buttressed about on either side. It is a wonderful achievement, in which every unit has played a part, and the spirit which has brought it about is the spirit of duty and work."

"England is like a composite photograph in which two likenesses are blurred into one. It shows traces of American enterprise and of German order, but the enterprise is faded and the order muddled. . . . We are a nation at play."

Dr. Shadwell develops this theme at considerable length. It must be the task, at any rate, of the English Committee of the International Standardisation Commission to remove this general reproach, which may be in some cases a merited one, from the industry with which we are connected, if we cannot do more. And at the same time we must remember that standardisation has its dangers. These are well brought out in an article by Dr. Louis Bell in the September number of the *Engineering Magazine*, entitled, "What of America? Do her manufacturing methods imperil her trade?"

"Broadly," as Dr. Bell points out, "competition is a struggle of intelligence, of quick perception and adaptability, of far-sighted appreciation of the world's wants. But unless the knowledge thus gained is diligently applied, it will sooner or later fail in results. Just at this point lies," he thinks, "the chief danger to American industry. The foreign peril lies not in foreign acuteness, but in the painstaking avoidance of our mistakes. Our real danger is not from without but from within—the danger that comes from overhaste and lack of thoroughness."

"These things are just as characteristic of American industry as is the marvellous alertness that has been its motive power. In the mechanical arts, for instance, American methods and workmen produce average results of remarkable excellence. But if one wants a bit of work done with the utmost thoroughness and precision, nineteen times out of twenty he will find that the workman who has finished it is a German, or Swede, or Englishman. . . . The primal intent" of the American industrial system "is to produce at the lowest possible cost the largest possible quantity of marketable goods. . . . The funda-

mental principle of its method is to reduce manufacture to operations by automatic machinery, using human labour only where it cannot be avoided, and constituting a manufacturing plant as a species of enormously complicated machine tool, of which the workmen are merely belts, wheels, and oilcans. The only real artisans are in the drawing office, the pattern shop, the tool shop, and at the inspection benches. . . . Hence, average quality is determined not by adherence to any ideals, but by imperfection of the working mechanism. It is mere justice to say that under favourable circumstances this average may be extremely high." And the conclusion drawn is of great importance :—

"Standardisation, however desirable from a pecuniary standpoint, in the last resort means the cessation of active improvement. In relatively fixed arts standard products may hold their own for many years, as in the case of certain textiles. In the mechanical arts, however, there is constant change, by which the world has marched onwards, and even a brief halt may mean falling out of the ranks."

Of recent years standardisation has advanced with us. Let us see what has been done to put it on a sound basis, to profit by the advantages pointed out by Dr. Shadwell on the one hand, to avoid the pitfalls to which Dr. Bell has called attention on the other.

The Engineering Standards Committee is doing no small work, and doing it in a way that is thorough and complete. As an example of its methods I propose to consider in some brief detail a recent report of the Screw Threads and Limit Gauges Sub-Committee, trusting that by this course we may discover how standardisation may be properly used to advance our national welfare. The Committee was originally appointed to deal with the interchangeability of screw threads, but at its first meeting it appeared that, in order to secure limit gauging for screw threads, if such were possible, it was necessary to collect additional information regarding the limit gauging of cylindrical surfaces.

A first step was to have clear understanding as to the meaning of terms employed. Words such as tolerance, allowance, clearance, and the like are used somewhat vaguely in practice, without very definite meanings. Accordingly the following definitions were agreed upon :—

TOLERANCE : A difference in dimensions prescribed in order to tolerate unavoidable imperfections of workmanship.

ALLOWANCE : A difference in dimensions prescribed in order to allow of various qualities of fit.

CLEARANCE : A difference in dimensions or in the shape of the surface prescribed in order that two surfaces or parts of surfaces may be clear of one another.

Thus in endeavouring to institute a system of limit gauges it was desirable to determine for various classes and sizes of work what difference in dimensions must be permitted in order to tolerate imperfections in workmanship, and what allowances must be prescribed in order to secure various classes of fit—sliding fits, running fits, and the like.

Various existing systems of limit gauges were brought before the

Committee, and much valuable information was put at their disposal in various interviews and conferences with leading engineers and manufacturers; but the results, important though they were, were felt to be insufficient as a basis for an authoritative system, and it was decided, in order to complete the information at the disposal of the Committee, to invite the co-operation of the manufacturers and to measure up a large quantity of work; the results would show the tolerances and allowances actually found in some typical shops. Eleven firms co-operated in a most cordial manner, and the results were drawn up in a report* issued by the Committee in March last.

Measurements on over five hundred pieces are recorded in the tables which sum up the work, and many of these have reference to a shaft and the hole it is intended to fit.

In such a case the table for the shaft gives in a series of columns the nominal dimensions, the nature of the work, ground or turned, the nature of the material, the length of the part measured, the number of measurements, the amount of taper, if any, and the amount of ellipticity. The same is repeated for the hole. Then come the tolerances as permitted to the workman, and the error in workmanship actually measured. These are followed by the allowances prescribed by the drawings and the difference in diameter between shaft and hole as actually measured.

The results are summed up in a number of diagrams, and, by the permission of the Committee, I am able to reproduce one or two of these, which may be of special interest to the Institution.

Fig. 1 shows the errors in workmanship in the shafts and holes for high-speed engines and dynamos.

In the Report itself numbers at the head of the columns give the numbers of the specimens; thus details of the various specimens can be obtained from the tables. The ordinates give in thousandths of an inch the error of the piece from its nominal size. Each mean result is marked by a dot or circle, and a short vertical line through any circle indicates the range of error found in the various measurements on that piece. The actual measured size is not given exactly, but the numbers at the bottom of the diagram indicate in inches these sizes approximately.

The dotted lines running nearly horizontally across the diagram give values of the tolerance for the corresponding sizes of the work which had been suggested to the Committee by one of its members, and which were ultimately practically adopted.

It will be noticed that with some few exceptions the tolerances measured lie within these limits, and, indeed, in the cases in which the limits were exceeded the nature of the work often afforded the reason for the excess. Some of the specimens with a large negative allowance were crank shafts of a large size, where a specially easy fit was required, while others, in which the allowance is positive, were crank pins.

* "Report on Errors in Workmanship," Engineering Standards Committee's Report, No. 23.

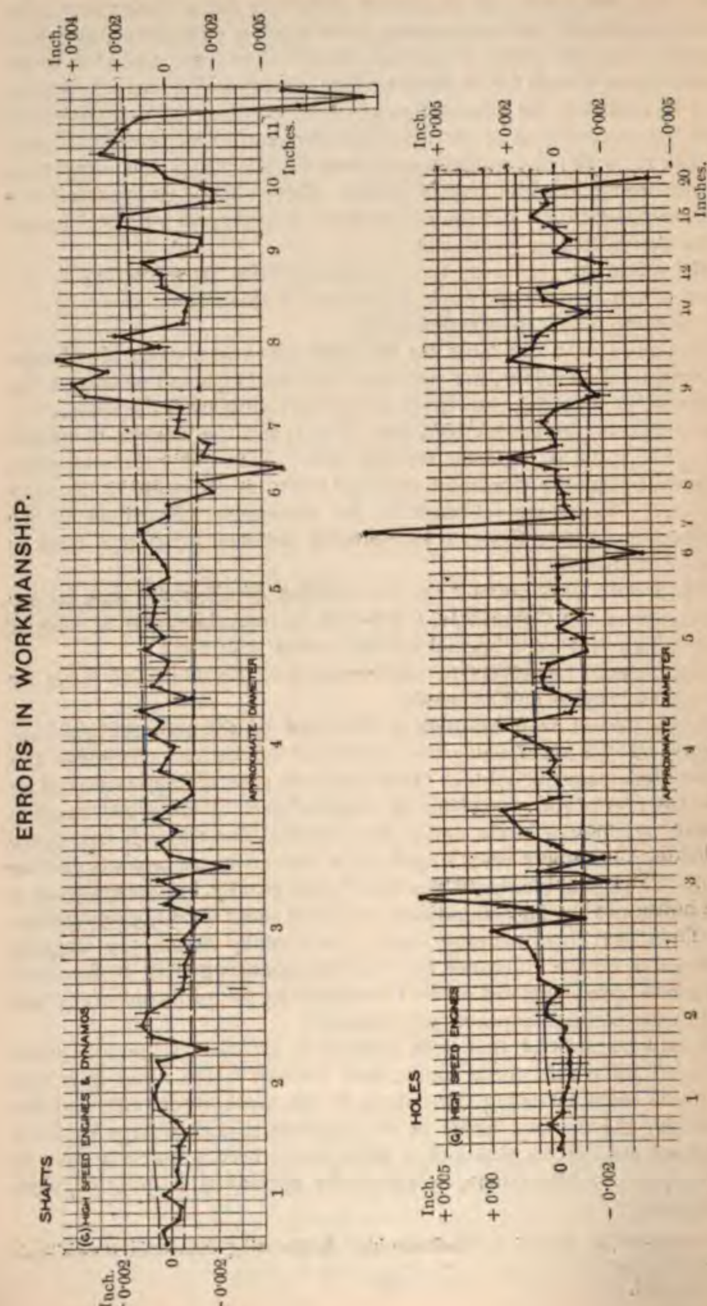

ERRORS IN WORKMANSHIP.
SHAFTS
(6) HIGH SPEED ENGINES & DYNAMOS
Inch. + 0·002
0
− 0·002
Inch. + 0·004
+ 0·002
− 0·005
APPROXIMATE DIAMETER
Inches.
HOLES
(6) HIGH SPEED ENGINES
Inch. + 0·005
+ 0·00
− 0·002
Inch. + 0·005
+ 0·002
− 0·005
APPROXIMATE DIAMETER
Inches.

FIG. 1.

In this case a large percentage of the cases measured came within the suggested tolerances, while the average error found for the shafts was 0·0011 inch and for the holes 0·0010 inch. The next slide (Fig. 2) shows the similar results for small tools, and in this class a very high degree of accuracy is reached. In two specimens there is a large accidental error. Even including these a very large number fall within the limits suggested; the average error is only 0·0005 inch. Of course in other classes of work, such as locomotives, gas engines, and large tools, the errors exceeded the above, but it is clear, I think, that the work was of a very high order of accuracy, while at the same time it was average representative work, for it was selected by Mr. Attwell, to whom the measurements are due, from such work as happened to be passing through the shops at the time of his visit in each case.

With this mass of information before them the Committee were in a position to standardise limits of workmanship, with the full confidence that the limits suggested would be practicable, being, in fact, those which are actually being worked to in our leading shops.

Their conclusions are given in a further Report on "British Standard Systems for Limit Gauges (Running Fits),"* being summed up in a number of tables and in a diagram, which is here reproduced (Fig. 3).

In dealing with the question of the allowance to be prescribed for a running fit between a shaft and a hole, it was necessary to determine whether the allowance should be made on the shaft or on the hole. After much discussion, the Committee recommended that the shaft should be the element more nearly approaching the true dimensions, and allowance be made on the hole according to the class of fit required. The tolerances on the shaft are negative, in order that it may never exceed its true dimensions. A minimum allowance is specified between the two elements, and this cannot be encroached upon by either element. Above this allowance a positive tolerance is laid down for the hole.

The Committee recognised a first, second, and third quality of work, while for the smaller sizes they standardised also an extra fine quality with very small tolerances and allowances. Moreover, they recommended, with a view to reducing the number of gauges required, that the maximum diameter of the hole for first quality work should be the minimum diameter for second quality, and so on.

Thus, for the shaft, the maximum diameter is always the same as the nominal diameter; the minimum diameter is given for the three classes by the three curves below the zero line, while corresponding curves above the zero line give the dimensions for the corresponding holes.

It is, of course, too soon to say how far this system is likely to establish itself. It is gratifying to learn from the Secretary of the Committee that there is already a large demand for the Report, and it should also be remembered that as, by the constitution of the Committee, the reports come up for consideration from year to year, the

* *Engineering Standards Committee*, Report No. 27.

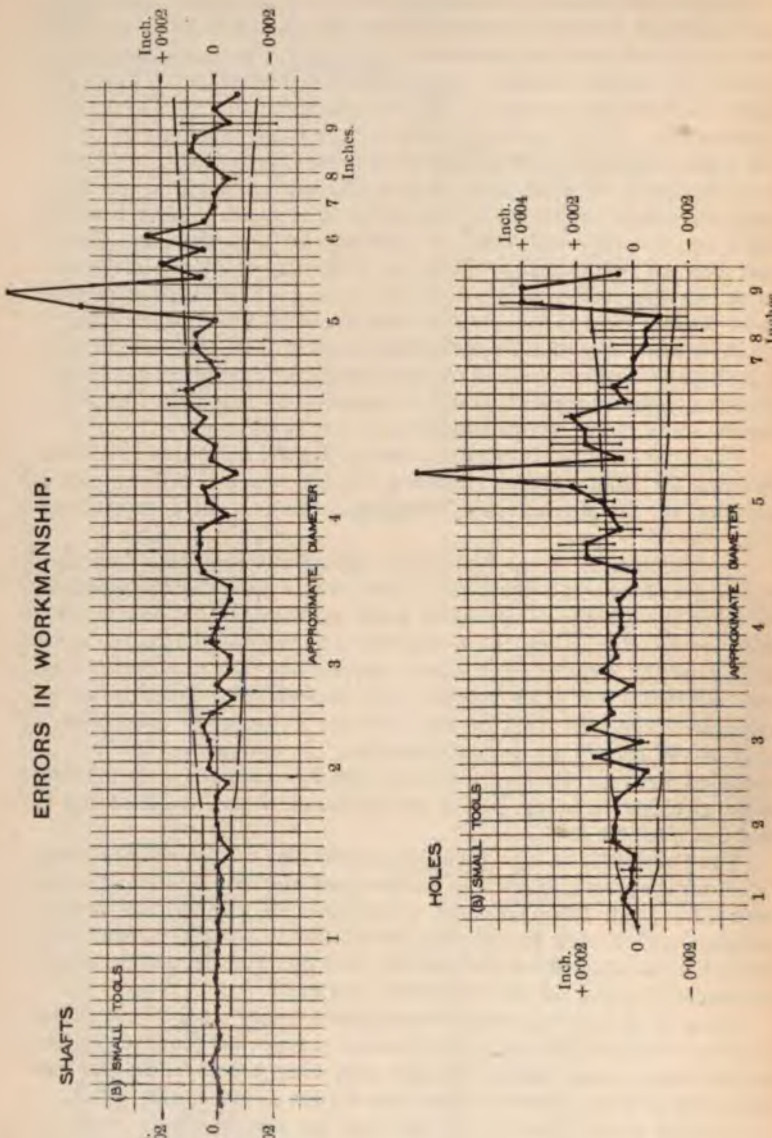

ERRORS IN WORKMANSHIP.
SHAFTS
(B) SMALL TOOLS
Inch.
+ 0·002
0
− 0·002
1
2
3
4
5
6
7
8
9
Inches.
APPROXIMATE DIAMETER
HOLES
(B) SMALL TOOLS
Inch.
+ 0·004
+ 0·002
0
− 0·002
1
2
3
4
5
7
8
9
Inches.
APPROXIMATE DIAMETER

FIG. 2.

chance of inflicting permanent injury by want of flexibility is greatly reduced.

The general interest of our Institution in engineering work must be

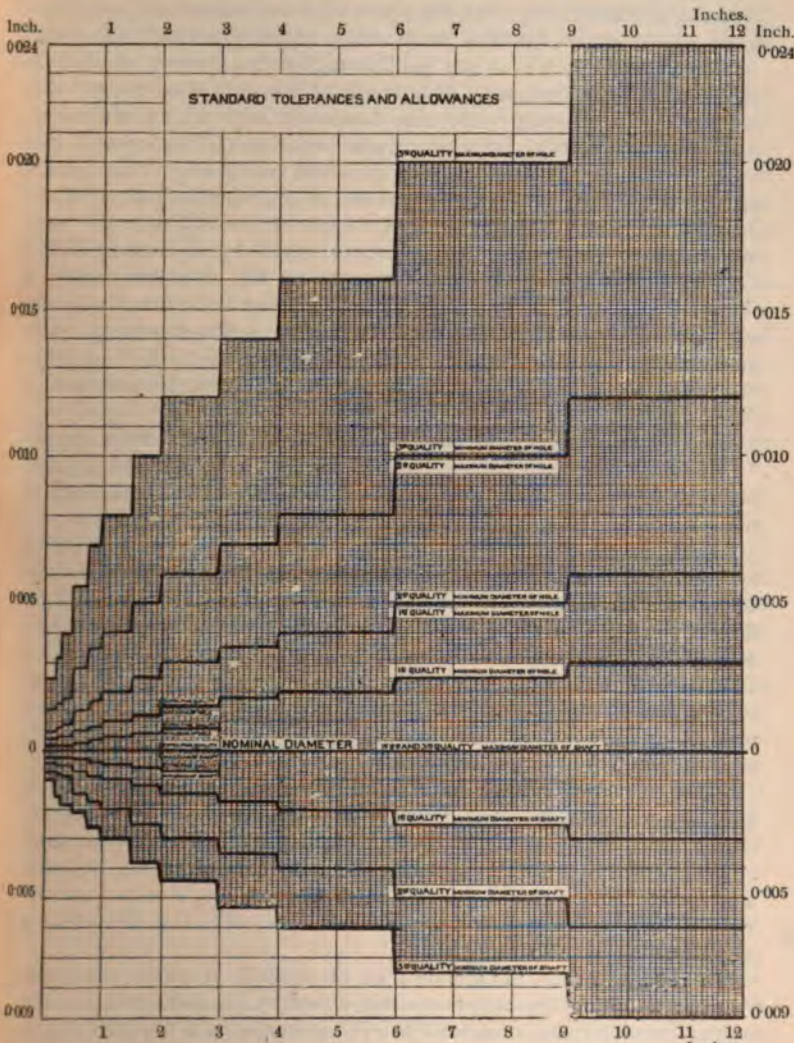


FIG. 3.

my excuse for occupying so much of the time with a question of mechanical engineering and workmanship. Let me turn now to the work of some of the Electrical Sub-Committees of the Engineering

Standards Committee. There are a number of such; it is, perhaps, sufficient to mention the names of some of the Chairmen to show how closely they are linked to our Institution. Among these we find our Past-Presidents—Sir Wm. Preece, Colonel Crompton, Mr. Gray, Mr. Siemens, and Mr. Gavey. The range of subjects covered by these Committees is enormous, and this is the reason why the work lasts so long.

Some reports, however, are published, and the method of work in all cases has been the same: discussions at Committee meetings, communication by writing with manufacturers and others interested, conferences in London with the trade, experimental work where that seemed necessary to elucidate the question, and finally a draft report, usually circulated before its final issue to the representatives of the manufacturers and others interested. Some of the experimental work initiated in order to reply to questions raised by the work of the Committee has already been before you in Mr. Rayner's paper on "Insulation Materials"; arrangements are being made for continuing and extending part of that work to higher voltages and to other materials, for investigating, in particular, the ageing effect of long-continued electric stress, and for examining the consequences of dielectric hysteresis.

Another report which I hope will be ready immediately is one on "Carbon Incandescent Lamps," and I trust it may be possible to bring before your notice during the present session a very large amount of work which has been carried out for the purposes of the report.

Still there remains much to be done. Many of the questions touched on by the Sectional Committee on Electrical Plant or by the Sub-Committee on Motors, Generators, and Transformers are somewhat difficult, and it is only by the mutual forbearance of conflicting interests and by the cordial endeavours of all to find a reasonable solution that advance can be made. Yet for all this we have advanced. Standardisation has been placed on a sound basis, and the work has shown that its importance is thoroughly appreciated by leading English manufacturers.

As to the actual use which is being made of the specifications, it is not easy to get very exact information. The Secretary of the Committee, however, tells me that, in regard to ship and boiler specifications, he is informed that the three Registry Societies controlling the shipping world, viz., Lloyd's Registry, the British Corporation, and the Bureau Veritas, have adopted them, while he believes that the Government Departments are also using them.

In regard to rails, the members of the Railway Engineers' Association are adopting the standards laid down by the Committee as and when opportunity to do so occurs. The tramway rails have also been very largely adopted. The chairman of the Tramway Rails Sub-Committee writes: "With but few exceptions every new tramway system in this country, as well as a number of Colonial systems under construction during the past year, have been, or are being, provided with these standard rails. Among these systems are Aston Manor, Belfast,

Barking, Burton and Ashby, Chesterfield, Colchester, Dartford, Derby, Erith, Ilford, Keighley, King's Norton, Lincoln, Lowestoft, Middlesex County Council, Northampton, Rochester, South Shields, Swindon, Walthamstow, and Wellington (New Zealand), while upon other systems already partly constructed the rails first adopted have given place to British Standard Sections in London, Birmingham, Leeds, Chatham, Croydon, and Newport.

Rolling mills for constructional steel work are largely adopting the specifications of the Committee. Sir John Wolfe Barry, Chairman of the Committee, in his interesting paper read before the British Association at York, stated that orders have already been received for nearly 100,000 tons of these standard rails, while it is estimated that the saving to British manufacturers by the standardisation of iron and steel sections alone will amount to some millions sterling; and in illustration he quoted the striking testimony of a large steel maker in Scotland, who stated that since the introduction of standard sizes his firm has been able to break up some hundreds of tons of rolls, and also that by no means the least advantage gained is that in his works the process of manufacture is no longer interrupted, as it used to be, by the frequent changing of the rolls to produce in smaller quantities the many special sizes asked for without any corresponding advantage to the consumer.

Enough perhaps has been said to show that standardisation, based on rational principles, is now open to the British engineering industry, and in concluding this part of my subject I should like again to emphasise the fact that, while there is no compulsion to adopt these British Standard specifications, they have in all cases been worked out by competent men on a practical basis, and that the general advantages to the community which will follow from their adoption will almost always outweigh the private benefit which in some special case might accrue from some trivial but tiresome variation. Standardisation is a large subject. You, gentlemen, know its magnitude and its importance far better than I; but I feel strongly that a due appreciation of the advantages which standardisation can offer, and a readiness to apply a standard system in the large number of cases in which it is applicable, may do much for English industry in its endeavours to retain that premier position which has belonged to it hitherto.

Sir WILLIAM PREECE, K.C.B. (Past-President): Gentlemen, it is my pleasure to propose, "That the best thanks of the Institution be accorded to Dr. R. T. Glazebrook for his interesting and instructive Presidential Address, and that, with his permission, the address be printed in the *Journal* of the Proceedings of the Institution." I have had free access to this room for forty-seven years. In 1859 I joined the Institution of Civil Engineers as an Associate Member, and I think, during those forty-seven years, I have heard, perhaps, forty-five Presidential Addresses. I was also one of the original members who constituted the Institution of Telegraphic Engineers in the year 1871, thirty-five

Sir William Preece.

Sir William Preece.

years ago, and at both of those two institutions I have myself given more than one Address. I am bound to confess that I never listened to my own Addresses without vexation; but of all the Addresses I have heard connected with these two Institutions in this room I am bound to say I have not heard one that has given me greater pleasure than the Address that has been given to us by Dr. Glazebrook to-night. I do not know how long it is since Dr. Glazebrook took up his position as Secretary of the Committee of Standards attached to the British Association; I think he was Secretary when I was appointed to it in the year 1878. From that period Dr. Glazebrook and I have worked hand and glove in carrying out the work of the Committee. I am delighted to think that, after a life spent in endeavouring to introduce exact measurement, the motto of his Address, I have been allowed to remain long enough to hear such an Address read. I never did feel more inclined to be humorous, if I could, than to-night, except for this, that I should be sorry to deprive this Address of the effect of its deep inwardness on those who take it up and read it with that earnestness that you all possess. At one time I felt some qualms of conscience at assisting the members of the purely scientific world to come amongst us engineers, but I am bound to say that things have changed very much within my experience, and now we welcome with all our hearts the assistance of those scientific men who can bring their physics to bear upon the practical processes and tools of our profession. Gentlemen, I have very great pleasure in moving the vote of thanks to Dr. Glazebrook.

Lieut.-Col. Crompton.

Lieut.-Colonel R. E. B. Crompton, C.B. (Past President): I rise to second Sir William Preece's proposition, which I heartily agree with. I have listened on three consecutive days to three consecutive Presidential Addresses, but as immediately on my left I see the President of the Institution of Civil Engineers, who delivered one of those Addresses, I will only state that it is extraordinary to find that our new President should have in the short space of two days been able to take advantage of the important suggestions and advice made by Sir Alexander Kennedy, by giving us the instructive and practical address that we have listened to this evening. I therefore beg to second this proposition in the most heartfelt manner.

The resolution was put to the meeting and carried by acclamation.

The President.

The President (Dr. Glazebrook): Sir William Preece, Colonel Crompton, and gentlemen, allow me to thank you very cordially for the way in which you have listened to my Address, and for the manner in which you have received the proposal of thanks which Sir William Preece has put before you. I would again say it has been a great pleasure to me to prepare the Address and give it here, and I trust I may be able, during my year of office, to promote still further the great advantages that I am clear will arise from a well thought out and proper scheme of standardisation.

The meeting adjourned at 9.25 p.m.

Proceedings of the Four Hundred and Forty-Fifth Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, November 22, 1906—Dr. R. T. GLAZEBROOK, F.R.S., President, in the chair.

The minutes of the Ordinary General Meeting held on November 8, 1906, were taken as read and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council:—

TRANSFERS.

From the class of Associate Members to that of Members:—

Edgar Walford Marchant.

From the class of Associates to that of Associate Members:—

Sidney H. Barber.	Harry Henderson.
Edgar Loam.	Frederick J. Lowe.

From the class of Students to that of Associate Members:—

Samuel Utting.	Francis E. Wilkinson.

Messrs. L. T. Healy and F. Pooley were appointed scrutineers of the ballot for the election of new members, and, at the end of the meeting, the following were declared to have been duly elected:—

ELECTIONS.

As Associate Members.

John Edward Allen.
Henry Sidney Barker.
Randall Birch Canning.
Nigel Henry Cohen.
Robert Dudley Coulthard.
Bertie Croft.
Ernest Daniels.
Ernest William Dean.
Harry Dixon.
Edwin Fulford.
Edgar Darnell Illingworth.
David Cunningham McIlleron.
James Mackersie.
Arthur Harold Pashby.
Archibald James Grant Simpson.
Thomas Reginald Herbert Tetley.
Gottlieb Wüthrich.

As Students.

Sidney Aitken.
Lewis Barber.
Horace Carl Beken.
Lionel M. Boddam-Whetham.
Leonard Bertram Boilard.
Alfonso Castello-Sosa.
John Edward Catt.
Thomas Harold Edwards.
Clarence Hollander.
Arno Clarence Huskinson.
Alexander Kinnes, B.Sc.
Frederic Auber Menzies.
Thomas David Morgan.
George Frederick O'Dell.
Alec. Stewart Paterson.
John J. Richardson.
Ernest Francis Turton.

Donations to the *Library* were announced as having been received since the annual general meeting on May 24th from Messrs. Alabaster, Gatehouse & Co., Ltd., B. J. Arnold, E. Anthony, Prof. W. E. Ayrton, C. Barnes, R. Bauch, W. W. Beaumont, A. R. Bennett, E. Bennis & Co., Ltd., Biggs & Sons, M. Bonghi, The Brush Electrical Engineering Co., Ltd., The Cambridge Scientific Instrument Co., Ltd., The Central South African Railways, W. C. Clinton, S. Cowper-Coles, E. H. Crapper, Prof. F. B. Crocker and S. S. Wheeler, W. Crudeli, P. Dawson, E. C. de Segundo, Dick, Kerr & Co., Ltd., D. Dorda, J. E. Dowson and A. T. Larter, M. T. Edelmann, The "Electrical Times," The Electrician Publishing Co., The Elektrotechnische Verein, The Engineering Standards Committee, Executors of the late Maj.-Gen. F. C. Cotton, G. Finzi, Prof. J. A. Fleming, A. L. Foley, V. A. Fynn, The Griffiths-Bedell Surface Contact Co., Ltd., Gauthier Villars, W. Geipel, Gent & Co., C. C. Garrard, R. Hammond, A. Hartleben, A. Hay, Hirschfield Bros., Ltd., H.M. Patent Office Library, W. R. P. Hobbs, The Institute of Chemistry, The Indian Telegraph Department, W. Jaeger and H. von Steinwehr, W. H. N. James and D. L. Sands, Prof. G. Kapp, M. Kohl, Lawrence Scott & Co., Ltd., The Liverpool Corporation Tramways, L. Lombardi, Macmillan & Co., Ltd., G. Marconi, A. W. Marshall, T. C. Martin, Maschinenfabrik Oerlikon, F. J. Moffett, The National Physical Laboratory, C. E. S. Phillips,

Physikalisch-Technische Reichsanstalt, G. Quincke, F. C. Raphael, Rentell & Co., Ltd., E. B. Rosa, The Royal Observatory, Greenwich, The Royal Society, J. C. Sager, Sir David Salomons, Bart., E. Scott & Mountain, Ltd., J. N. Shoolbred, Siemens Bros. & Co., Ltd., Prof. A. Slaby, K. Sosnowski, E. & F. N. Spon, Ltd., A. Still, J. Swinburne, F. Tandy, J. Thin, Prof. S. P. Thompson, Prof. E. Thomson, Sir Charles Todd, A. P. Trotter, W. C. Unwin, The Verband Deutscher Elektrotechniker, The Wagner Electric Manufacturing Co., L. H. Walter, E. Warburg and G. Leithäuser, Whittaker & Co., F. W. Wilcox; to the *Building Fund* from Prof. Epstein, W. von Siemens, A. H. Unwin, F. H. Webb, H. Wragg; and to the *Benevolent Fund* from S. E. Britton, A. F. R. Curteis, H. M. Middleton, T. H. Minshall, S. Paterson, E. H. Rayner, H. S. Russell, C. H. Shanan, C. P. Sparks, F. H. Webb, to all of whom the thanks of the meeting were unanimously accorded.

The following paper was read and discussed, and the meeting adjourned at 9.45 p.m.

TESTING OF ELECTRIC MACHINERY AND OF MATERIALS FOR ITS CONSTRUCTION.

By Professor J. EPSTEIN (Frankfort), Foreign Member.

(*Paper read November 22, 1906.*)

It will readily be conceded that the selection and testing of all materials necessary for the construction of electric plant and machinery as well as the testing of the complete machine or apparatus before putting into regular service is of the utmost importance, both to the user and manufacturer.

As a rule the purchaser of the machinery is only able to carry out tests with the finished article, while the manufacturer, on the other hand, is not only in a position to test it step by step during its construction, but also to exercise control over the quality of materials used, either accepting or rejecting them according to their suitability or otherwise for the purpose in view. The latter is therefore well able to guarantee that the machine when complete will fulfil the specified requirements.

In the first place the selection of material requires the exercise of the greatest judgment on the part of the manufacturer. It is not always the most expensive qualities which will best answer his purpose, as it may happen that these are unsuitable for the special work in view, especially when one reflects that the supplier of the material for the construction of electric machinery is usually ignorant of the working conditions and of the particular stresses to which it will afterwards be subjected, these being generally known to the user only.

The material which plays the most important part in the construction of electrical machinery is copper, which must be used in its purest state in order to obtain a good conductivity. Provided therefore that copper possesses the requisite conductivity, its suitability for electrical purposes is assured.

In measuring the resistance of copper the temperature of the sample to be tested must first be allowed to assimilate itself to that of the laboratory, and no readings should be taken until the sample has acquired the temperature of its surroundings.

The cross section of round wire is generally obtained by means of a micrometer. In cases where the diameter is very small, weighing may be resorted to. With wire of a section other than circular, allowance must be made for the rounded edges. From a large number of readings taken in the author's laboratory it was found that for the

usual sizes of copper strip the cross section may be taken as equal to $(h - 0{\cdot}35) \times b$, where h and b represent the greater and smaller axes of the strip in millimetres.

It has been proposed to specify only the resistance per metre, and, if the specific resistance proved to be above normal, to allow the contractor to compensate for this by increasing the cross section of the material under consideration. Such an arrangement might answer in the case of cables, where the price is fixed by the length, but it cannot be adopted in the case of machinery for the reason that plain copper is usually bought by weight, consequently the worse the quality of the copper the more costly it would be for a given length. Moreover, the greatest accuracy of gauge is required in building a slot-wound armature and in the construction of field coils. Depth and width of the slot are calculated to a tenth of a millimetre, and any irregularities in the dimensions of the copper strips and insulation would make it impossible to fit them in the slots as intended. The speed of shunt-wound motors also depends upon the diameter of the wire on the magnet poles. It is, therefore, absolutely essential that there should be no appreciable variation in the dimensions of the wire.

Fig. 1 represents the results which have been compiled from tests carried out in the Felten & Guilleaume laboratory, and it shows the percentage of deviation of the diameter of the wire from that specified by the makers. It will be observed that the sample indicated by ○ is in general more reliable in this respect than the sample indicated by ●.

The conductivity of copper being dependent on its purity, and the manufacture of pure copper having long been practicable, it follows that no great improvement in high conductivity drawn copper is to be anticipated in the future. Therefore, although copper wire and bars are the most important materials in dynamo construction, the determination of their qualities presents little difficulty in the laboratory, as but few tests are required for this purpose.

The specific conductivity of cast copper may, on the other hand, vary from about 40 per cent. to 85 per cent. in the Matthiessen scale. When a young laboratory assistant finds the conductivity of a piece of cast copper to be 85 per cent. he would be likely to consider it excellent, but if the experienced workman detects porosity he will rightly pronounce it bad; in such a case, as in many others, one is apt to be misled by a purely physical test made in the laboratory.

Resistance materials, like German silver, manganin, etc., show even greater variations than copper, the specific resistances varying from 0·3 to 0·8 unit. Whether a higher or lower specific resistance is more suitable for the work is a matter of calculation. When the resistance has been decided upon, special tests and experiments are needed to discover what alloy will give the best results as regards stiffness, elasticity, ageing, etc. The proper function of the laboratory is to ascertain if the dimensions and specific resistance are in accordance with the specification.

The variation in the specific resistance above or below normal can be compensated for by increasing proportionally the weight of the material, that is, with a less than normal specific resistance the length must be increased, while with a higher than normal specific resistance the cross section must be increased. In reality the risk is less with a specific resistance above the normal, since, in the majority of cases, rheostats are provided which must be capable of regulation down to zero, hence the higher specific resistance will not affect the regulation.

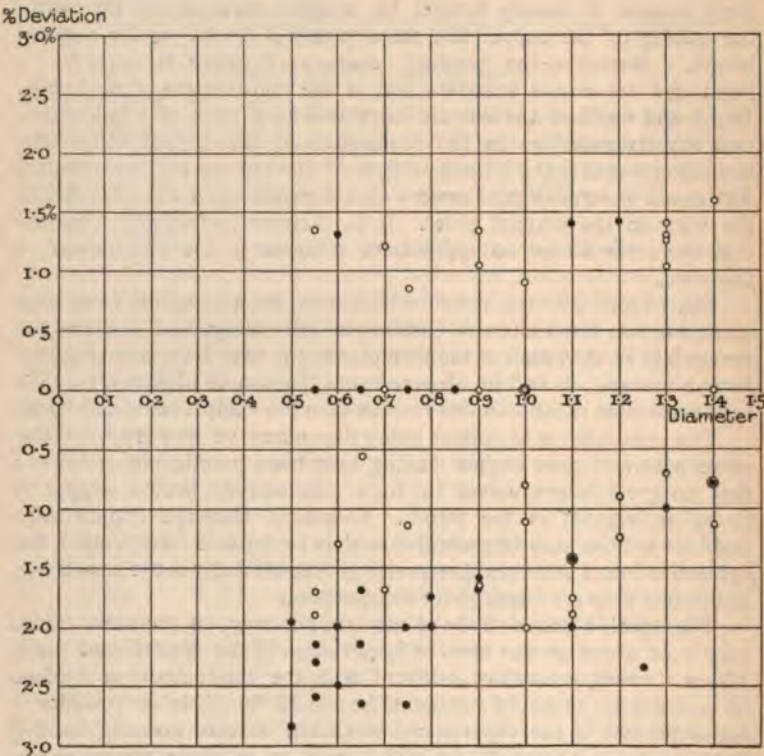


FIG. 1.—Percentage Deviation of Wire from Specified Diameter.

It is only very rarely necessary to take into consideration the possibility of an overload beyond the limit of thermal capacity.

In contrast with copper, where the purest is the best, the quality of iron is by no means always depreciated by the introduction of foreign elements. On the contrary there is great scope for improvement by introducing other substances, such as carbon, silicon, aluminium, etc. There is another point which makes the question of iron more complex than that of copper; with copper we are interested only in its conductivity, whereas in the case of iron there are three magnetic and electric

qualities of importance, namely, magnetic permeability, magnetic hysteresis, and electrical conductivity.

As long as the magnetic flux is constant, permeability alone is of importance. Although there are some kinds of cast iron or cast steel which are practically non-magnetic, one can as a rule obtain from every good foundry cast iron suitable for electric machines. The higher the permeability and the higher the point of saturation, the better of course the iron. In designing a machine for a given flux, both iron and copper will be saved and efficiency will be gained by using an iron of good permeability and having a high degree of saturation. If, however, after construction the iron proves superior in these respects than was expected, it will be too late to take advantage of this, as the structural details of the machine will have been settled. The speed of motors will be less than the required speed, which will have to be compensated for by introducing an additional resistance, while with dynamos it will be necessary to vary the field resistance. Therefore, to use iron of better magnetic quality than one ordinarily expects only leads to trouble and expense.

To a certain extent the quality of the delivered material will necessarily vary, and accordingly we must calculate the ampere-turns for the worst quality expected, and make allowance by constructing the variable field resistance for the best quality. What is therefore required and appreciated is an iron of high constant quality.

Fig. 2 shows the magnetisation curve for a good and for a bad quality of cast iron, and the same for special cast steel, as observed in the author's laboratory among the samples received for regular test. The difference between the curves seems very small with the cast steel; the magnetisation, for instance, for 50 ampere-turns per centimetre varies by only a few per cent. But the chief point of interest is the number of ampere-turns required for a given B, which, for instance, for $B = 17{,}000$ lines per square centimetre, differs by 40 per cent. for the two samples of steel.

It is important in comparing the materials and factors of safety to refer to the right factor. In investigating the permeability of ordinary cast iron, the author would recommend, as a characteristic figure, to take the number of ampere-turns per centimetre required for $B = 9{,}000$, and, as regards special steel, those required for $B = 17{,}000$. For measuring the permeability of cast iron and steel the ballistic method appears the most convenient. The best results would be obtained by using large rings, having a radius approximately equal to five times the thickness of the actual ring coil, but for practical reasons the yoke arrangement is to be preferred. As remanence is not of much importance, the magnetisation curve obtained by reversing the magnetising current should be noted. This curve has the advantage over the hysteresis curve obtained step by step, that each point is in itself absolutely correct and does not suffer from cumulative errors.

The question of remanent magnetism in field frames is generally of

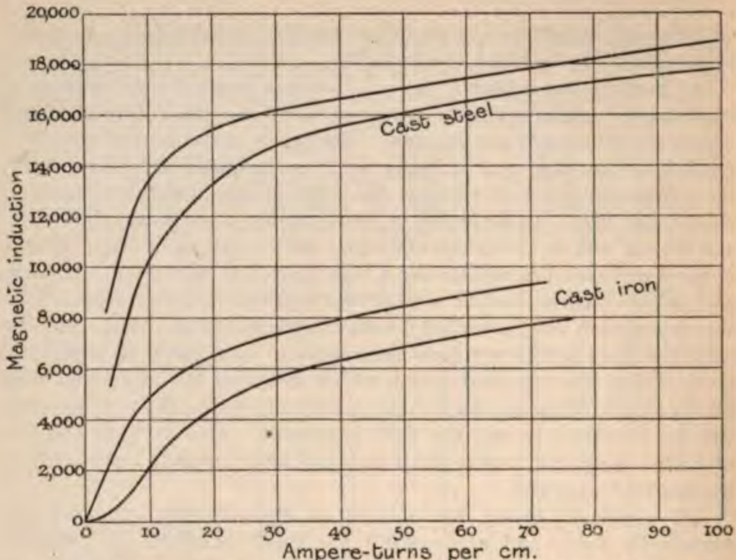


FIG. 2.—Magnetisation Curves for Cast Iron and Cast Steel of Different Qualities.

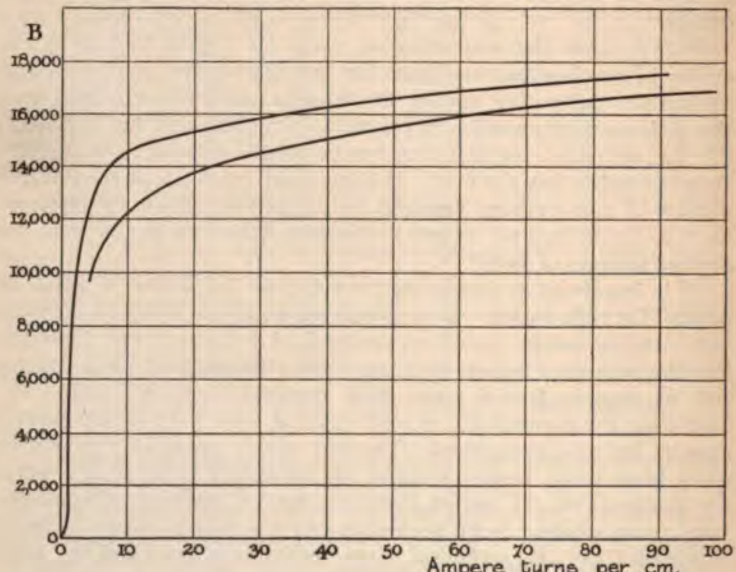


FIG. 3.—Magnetisation Curves of Iron Sheets. The upper curve represents an ordinary stamped sheet, the lower one is that of a modern sheet of special alloy.

secondary importance, as consideration of this only becomes necessary when dealing with some self-regulating machines, such as boosters, etc.

Besides the physical laboratory tests, the mechanical inspection of cast steel is very important. For instance, holes in the steel may increase the magnetic resistance of the poles or yoke and cause dissymmetry in the magnetic circuits, which in the case of a continuous-current dynamo will produce sparking or heating in the armature.

Although every magnetic and electric property of the iron sheets used in the construction of armature or transformer cores plays its part, yet the permeability and the saturation are not of such importance, as it is necessary to work with a low saturation of the sheets in order to avoid losses and heating. Extra high saturation will be found in the teeth of continuous-current dynamos. As some special stampings give less hysteresis and eddy current losses, they may be worked at a higher degree of saturation. Unfortunately they possess a poorer permeability than the old class of stampings. In Fig. 3 the upper curve is the magnetisation curve found for an ordinary stamped sheet, while the lower curve is that for a modern sheet of special alloy.

The hysteresis can be measured by taking the magnetisation curve through a complete cycle, and by measuring the specific resistance of the material directly by a Wheatstone bridge; but the modern and better practice is to measure the total losses due to hysteresis and eddy currents together, the sample being tested under working conditions.

By varying the frequency the well-known straight line curves are obtained which discriminate between the losses due to hysteresis and eddy currents at constant induction. The losses themselves are measured with a wattmeter, special care being taken that the instrument is correct for low power factors. As the amount of energy to be measured is very small, precaution must be taken against the occurrence of losses in the instruments.

The apparatus used by the author for such researches was constructed eight years ago. It was the first standard apparatus for testing iron sheets approved of by the Verband Deutscher Electrotechniker at their meeting at Dresden, where they had the pleasure of meeting their English friends. To get a good commercial average value the apparatus is constructed to take a 10 kg. sample, which according to the rules of the German Committee must be made up from at least four different sheets. Small samples taken from different sheets, or from different parts of the same sheet, differ sometimes by as much as 20 per cent. The form of the apparatus is shown in Fig. 4. There are four coils for magnetising the four cores of sheets under test, each insulated by thin paper in the same manner as in transformer cores, and at each corner, where the cores meet, a layer of paper, 0·2 millimetre thick, is interposed. This thickness does not affect the exactness of the results, as even when a layer up to one millimetre was tried, there was no appreciable difference in the readings.

Naturally, in order to obtain the best readings the magnetising current should be adjusted to as low a value as possible.

Some years ago ironworks were not accustomed to test iron sheets

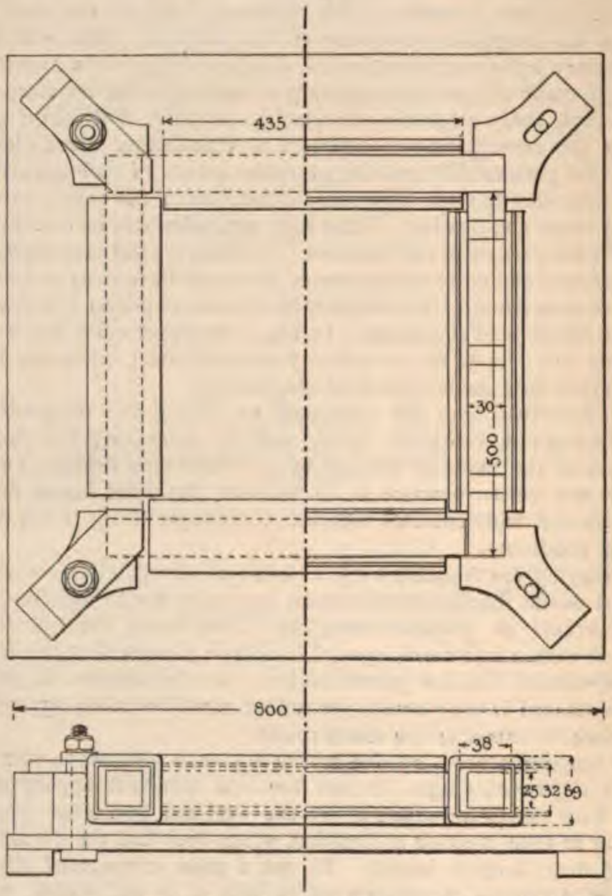


FIG. 4.—Apparatus for Investigation of Magnetic Properties of Iron.

Total No. of Coils	600
No. of Coils per Core	150
Length of Iron Sheets...	500 mm.
Width " "	30 "
Weight of Iron Sample to be investigated	10 kg.
Cross Section of Wire, 2 Wires parallel...	2 × 3·5 mm.
Resistance of the Apparatus	0·184 ohm.

for dynamo work. When they tried to do so, they found that their results rarely agreed with those of the dynamo manufacturer, on account of the variety of methods and of their complication and inaccuracy. It was therefore important that the manufacturer and con-

sumer should mutually agree upon a method of testing which should be as simple as possible. To settle this the Verband Deutscher Elektrotechniker appointed a committee which decided to adopt the wattmeter method. When samples of iron were sent to laboratories of different firms, it was found that the losses measured for one induction and one frequency agreed fairly well, but there was great trouble as soon as an attempt was made to draw the straight line curves for separating hysteresis and eddy currents. Therefore, although we knew that in order to obtain the exact characteristic of iron it was necessary

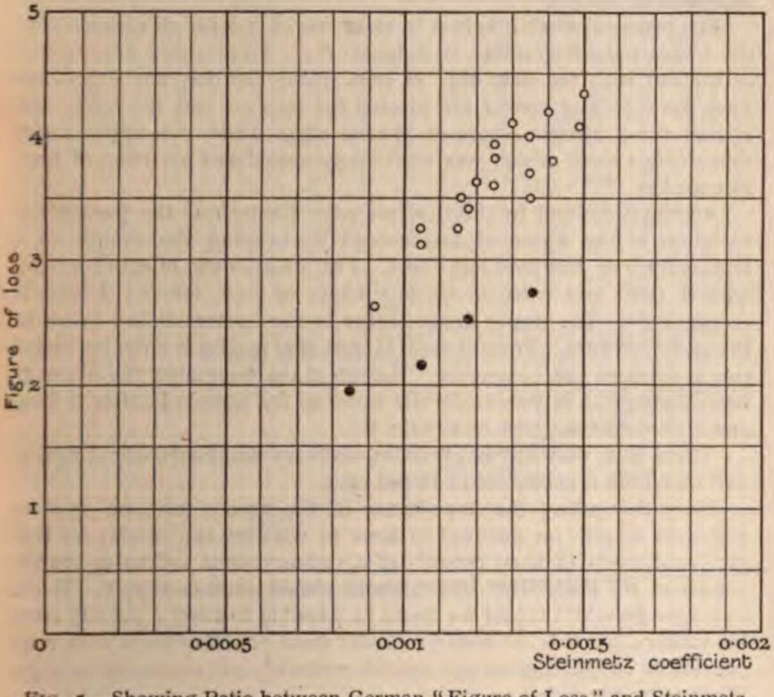


FIG. 5.—Showing Ratio between German "Figure of Loss" and Steinmetz Coefficient, for Iron Sheets of two Different Compositions.

to test it at least in two particulars, namely for hysteresis and eddy currents, we abandoned the attempt and decided instead to take as a characteristic the loss measured with B = 10,000 lines per square centimetre and 50 ∼. In the German rules this value is called the "figure of loss" (Verlustziffer). The adoption of this standard has proved very useful during the last few years. Since the eddy currents decrease with rising temperature the figure of loss was determined for a standard temperature of 30° C. The commercial 0·5 millimetre sheets made the last few years changed by about 0·2 per cent. for each

degree C., while the modern sheets show practically no variation. In comparing different kinds of iron the electrical engineer naturally examines them under varying inductions and frequencies, and the manufacturer will now also do the same, as he gradually gets accustomed to making magnetic tests. In the Felten-Guilleaume-Lahmeyer laboratory every wagon load of iron sheets is tested and is rejected if the "figure of loss" is too high. Only with irons of analogous composition, the conductivities of which are almost equal, is there any direct proportion between the above figure of loss and the "Steinmetz" coefficient. Fig. 5 shows this ratio as found for sheet iron of two different alloys.

For practical work it is best to make use of a diagram which shows the losses per kilogramme at different $\sim$. To present a comparison of the old with the new kind of iron sheets, the diagram, Fig. 6, has been drawn. The curves are plotted for 50 $\sim$, and the lower one is that for a modern sheet of special alloy, while the upper curve represents a sheet which was until lately considered as being of first-rate quality.

Ageing is defined by the German rules for iron as the percentage variation of the figure of loss caused by keeping the sample at a temperature of 100° over 600 hours. Fig. 7 shows the effect of ageing with a good and with an inferior sheet of iron, marked A and B respectively. The upper curves refer to the hysteresis, the lower to the eddy currents. From these it is seen that ageing is only due to the rise in value of the hysteresis. Though at the beginning the figure of loss of sample A is practically the same as for sample B, after a long run it exceeds the figure of loss for B.

There is no difficulty in obtaining ordinary stampings, whose ageing will be within a maximum of 15 per cent.

Notwithstanding the importance of the ageing test, the dynamo maker is unable on account of time to wait for the result; all that can be done is to keep records of his observations and to utilise the results of his tests when he wishes to obtain another supply. If the iron ages greatly it would no doubt be possible to exact a penalty from the makers, but it is far better to order from manufacturers who have previously supplied good and reliable material, and are therefore more likely to be depended on.

The testing of the resistance of insulating materials used in the construction of dynamos is not so important as might appear. The old-fashioned axiom that high resistance insulation was a guarantee for good quality, which formerly proved right for special cases, does not now hold good. Insulation difficulties are scarcely ever due to low insulation resistance, but more often to surface leakage, which may be avoided by using only dry materials and by suitable design.

It is necessary, however, to test all insulating materials for disruptive strength, and this constitutes, for high-tension work especially, the most important electrical test. Nevertheless, the more experienced one becomes in the practical testing and theory of disruptive strength,

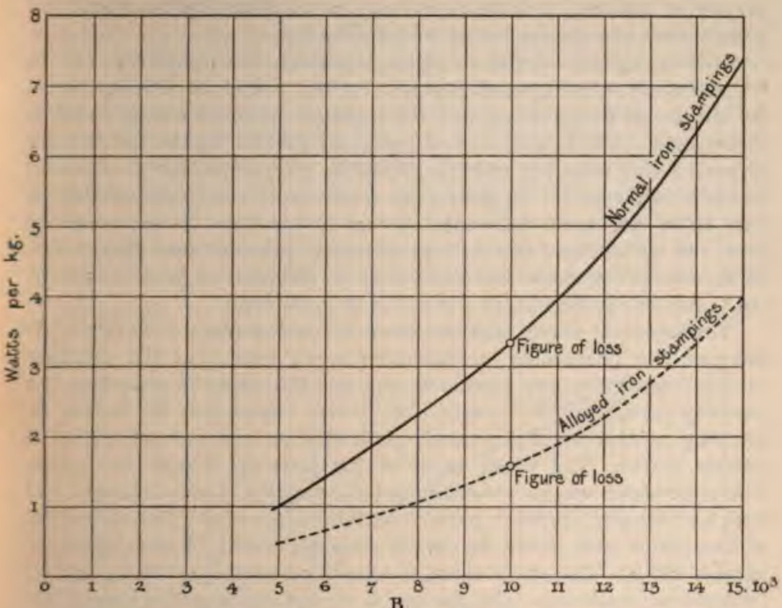
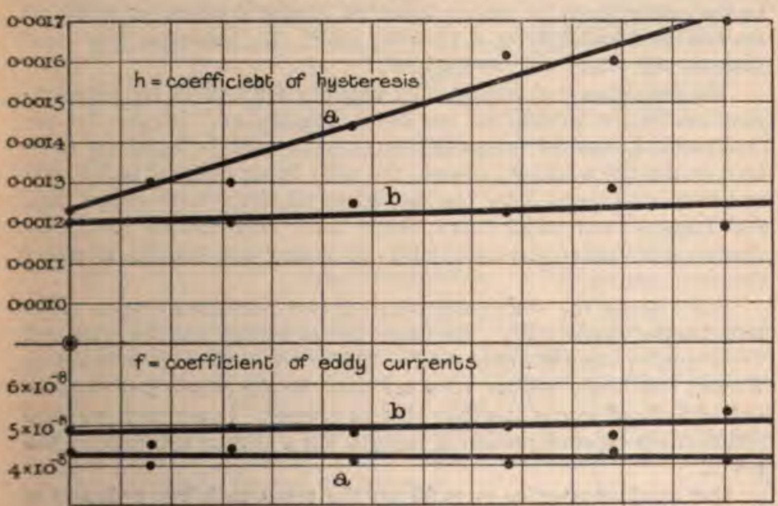


FIG. 6.—Curve of Losses for an Ordinary (Normal) Iron Sheet and for an Iron Sheet of Special Composition.



$$\text{Watts/kg} = (h \times B^{1\cdot 6} \times n + f \times B^2 \times n^2) \times \frac{10^{-4}}{sp.\ gr.}$$

FIG. 7.—Ageing of Dynamo Sheets.

the more difficult it appears to obtain accurate figures which are comparable with those of other investigators.

Most of the high-tension insulating materials, like micanite products, varnish cloth, impregnated paper, etc., are not at all homogeneous. By means of the piercing test the strength at the weakest point is ascertained. The larger the test piece, the greater will be the number of weak points covered, and the greater is the probability of obtaining specially low values. To prove this the author tested two samples, A and B, of the same material. A was tested with an electrode of $50 \times 3 = 150$ cm.^{2} and the average piercing pressure was 7,600 volts. Of B, different samples were tested with an electrode of $50 \times 62 = 3,100$ cm.^{2}, and the piercing point was found at 4,000 volts.

The effect of increasing the area of the test-piece is not only to increase the probability of detecting weak spots, but the increase in the capacity of the electrode also has the effect of lowering the piercing pressure. We proved this to our satisfaction by testing to piercing point a sheet of varnish cloth with an electrode of 4 cm.^{2} in sixteen places. The mean value of the piercing voltage was 9,900. The same sheet was then further tested, using the same electrode, but with a condenser placed in parallel with the electrodes. This consisted of two plates each about 64 cm.^{2} in area, separated by two layers of varnish cloth. The mean value of twenty readings gave the piercing pressure as 7,250 volts. On placing a second condenser in circuit the piercing pressure sank still further to 7,100 volts.

In the light of these experiments a standard electrode was adopted by the author's firm for testing dielectric strength, which for practical reasons was taken at $25 \times 25 = 625$ cm.^{2} To minimise the local stresses, the edges are rounded off.

The importance of testing most carefully mica tubes for high-tension machinery cannot be too much emphasised. In the Felten-Guilleaume-Lahmeyer laboratory every mica tube is tested up to at least double the working pressure, the tube being covered with tinfoil and filled with metal rods. As the factors of safety taken are so high that failures even under more severe tests very seldom occur, the electric phenomena are controlled by noting the heating effect of dielectric hysteresis.

Fig. 8 shows the difference between two 2-millimetre mica tubes tested under 15,000 volts. The conditions of testing must be identical. For example, when the mica tube is filled with iron rods it becomes warmer than when copper rods are used, for the reason that the heat conductivity of iron is less than that of copper. In the same way the length of the exposed portion of the tube has a distinct influence on the result.

Our standard practice is to fill up the tubes with iron rods and to cover them in such a way that the exposed portion of the tubes equals 20 per cent. of the length under test. About 30 tubes are then laid one against the other. Mica tubes, however, which may be able to resist this particular test will still be unsuitable for the work if they show inferior mechanical strength or deformation at higher temperatures.

Much has been said about the danger arising from the generation of nitric acid in mica tubes. This phenomenon has also been observed in our laboratory while testing mica tubes filled with cotton-insulated wires under extra high pressure. At first, experiments were made with cotton-covered materials impregnated with different kinds of shellac or paraffin, and the effect in question was soon noticed, even when using the purest materials. Both the thickness of the tubes and the testing pressure were then varied, and tubes of different manufacture were also used. Tests carried out during the last three years have shown that for our practice at pressures up to 10,000 volts, the limits of thickness and of quality to be kept in order to avoid generation of

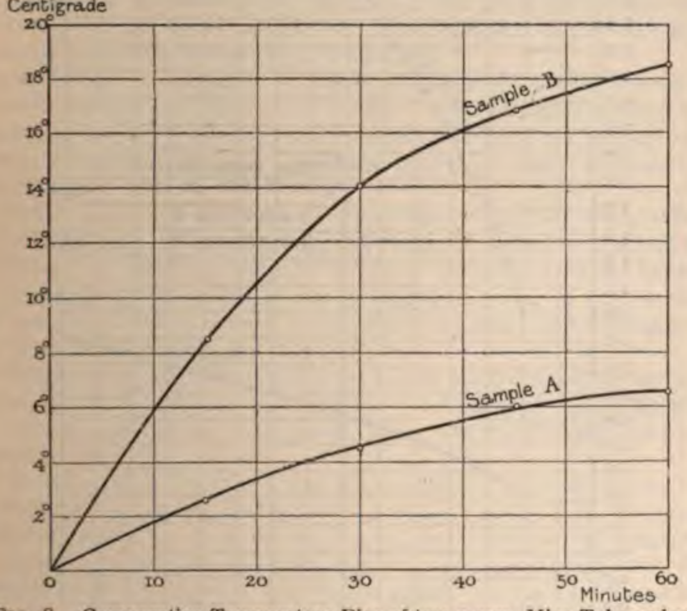


FIG. 8.—Comparative Temperature Rise of two 2-mm. Mica Tubes when tested at 15,000 Volts.

nitric acid lie within the limits which had been adopted by us from other electrical and mechanical reasons. Although there are methods by which this phenomenon may be overcome, the author has not found it necessary within his own practice to use them, but he quite agrees that when stresses are higher, owing either to higher potentials against earth or to the nature or dimensions of the materials used, special precautions should be adopted.

In testing carbon brushes the most important details to be considered are durability, absence of any vapours developed through heating, smoothness of running, low friction and low wear and tear and also the homogeneity of the carbon.

As to electrical features, a high surface resistance may be an advantage in a high-voltage machine, but the reverse in a low-voltage. Fig. 9 illustrates the voltage required for pairs of carbons at different density of current, and the watts lost by the friction per square centimetre at a speed of one metre per second. The figures were taken with a specific pressure of 100 grammes per cm.[2] Further experiments showed that on a well polished commutator the A quality carried about

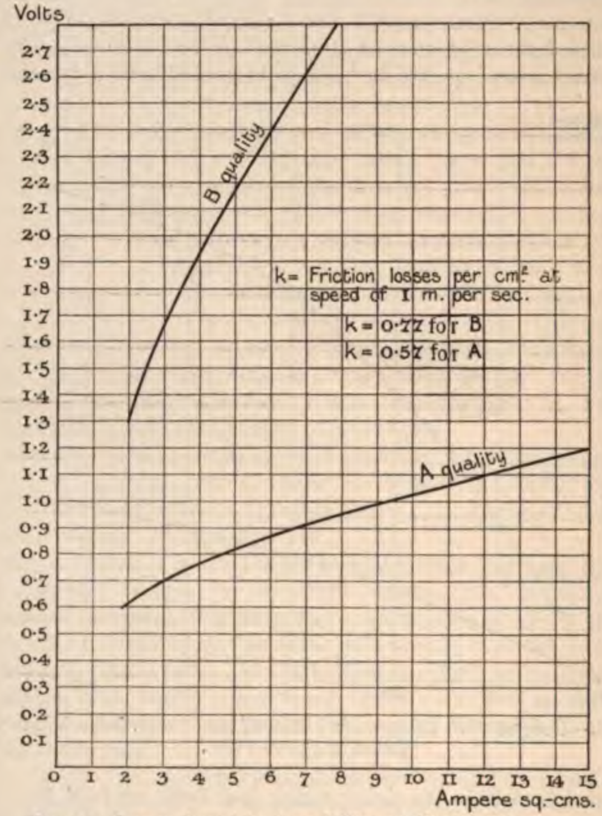


FIG. 9.—Losses found for two Different Kinds of Carbon.

50 amperes per cm.[2] without glowing, and the B quality carried about 10 amperes, but to be on the safe side we do not work at a higher current density than 9 amperes per cm.[2] with the A quality, and 4·5 with B. For instance, to collect 180 amperes with A quality, 2 × 20 = 40 cm.[2] would be required for both poles, and the losses would be 150 watts by drop of voltage and, at a speed of 10 metres per second, 240 watts by friction. With the B quality the brushes would require to have 2 × 40 = 80 cm.[2] brushes, and with these the losses would be 380

watts by drop and 640 by friction. From these data it appears at once that the A quality is superior to B, and though the cost per $cm.^3$ is much higher, A would be more economical both in initial cost and in running efficiency. For high-tension work, however, it was found necessary to use carbons of the B quality, as low resistance carbons gave bad commutation, the brushes being apt to heat, due to parasitic currents, and to glow at apparently normal current density. As this phenomenon is not due to machine voltage itself, but to the voltage of reactance, the difficulty will diminish with the voltage of commutation. To allow the use of low-resistance brushes for machines with high commutation voltage a sort of laminated brush has been proposed with exceedingly low resistance in the direction of the main current and high resistance in the cross section. Excellent carbons of this sort have been placed on the market by a British firm.

To obtain comparative values, experiments on carbons should be carried out with commutators or collector rings of faultless polish and accuracy, but as we do not desire to make show tests, our practice is to test the carbon brushes on machines under the most unfavourable conditions.

Some years ago the author's firm found a carbon brush which gave excellent results in the laboratory and for several months on the machines in their own works. When it was placed on the market, however, though it worked satisfactorily in 99 cases out of 100, yet with certain plants and on certain machines it gave trouble, and consequently the firm abandoned the use of it altogether.

The chief details to be considered in the electrical testing of complete electrical machines are insulation, temperature rise, efficiency, regulation, commutation and power factor, both for different loads and for overload.

In dynamo practice as a rule the result of poor insulation is not the loss of energy but the production of an "earth," with the possibility of shocks to those in attendance, the disturbance of telephone apparatus, etc., and the risk that in time it may cause a breakdown due either to local heating or to electrolytic action. In the latter respect, a 2000-ohm leakage on a 500-k.w., 240-volt dynamo would be altogether harmless if equally distributed, while the 29 watts would be dangerous to the insulating material if the whole of the leakage were concentrated at one spot. To a certain extent, therefore, the interest is not so much in the megohms but in the question as to whether there is any local leakage. That is to say, suspicion would arise if the total amount were greater than might reasonably be expected, all other factors being normal.

A great improvement was effected by using fibrous materials, such as leatheroid, in connection with low-tension and especially with continuous-current dynamos, on account of their elastic and mechanical strength. Mica insulation, which is indispensable for high-tension machines, is excellent when used in thick layers, but on account of its inflexibility and brittleness it is not at all suitable for use in thin layers,

especially when it is necessary to bend it. The purely electrical test of a newly erected machine is likely to give better results with a mica insulated continuous-current armature than with an armature insulated with leatheroid; but with mica there is more risk of local faults, which occur as a rule at the corners and are chiefly due to the brittleness caused by the continual heating and cooling.

What is really wanted is reliability, which can only be guaranteed by practical experience on the part of the manufacturer, and not by a simple physical test alone. A machine, even when built with the most suitable material, has to undergo on completion an insulation test with a higher voltage than its normal, in order to ensure a reasonable factor of safety. It is desirable that a machine be able to stand any surges that may possibly arise under working conditions, but to insulate a machine in such a manner as to safeguard it against lightning discharges would not, as a rule, prove advantageous or commercial.

The probability of the occurrence of surges in a plant of given output increases with an increase in voltage. Although from this point of view the factors of safety to be taken ought to increase with the voltage, in practice the opposite is actually found to be the case, this being due to mechanical and electrical reasons. A certain thickness of insulation is necessary in order to gain mechanical strength and stiffness, and it is not good practice to use mica tubes thinner than 2 millimetres, otherwise they will not stand the mechanical stresses.

Thus at lower pressures the factors of safety are unnecessarily high, simply from mechanica reasons. The ratio between the thickness and the piercing pressure increases with the former. Consequently, to increase the factor of safety to a given amount will affect the construction of a 10,000-volt machine much more than that of a machine of 5,000, not to mention the difficulties which arise owing to the excessive space in the active zone taken up by insulation of great thickness. We require for our mica tubes a safety factor of 5 or 10, in order to ensure safety against accidental weakness of the material. For this reason an additional thickness must be adopted, which, as will be easily understood, will not increase the factor of safety of thicker tubes to the same extent as that of thinner tubes.

Having to take account of the fact that single layers may be faulty, it is necessary to increase the number of layers in proportion as the total thickness of the insulation is diminished. Even if it were desired to increase the thickness of the mica tubes proportionately, the dielectric strength of the tube will still not increase in the same ratio as its thickness. This is shown by curves of dielectric strength in relation to thickness, which, as is well known, droop at higher values.

Again the weak point of an extra high tension machine is often not to be found in the insulating tubes themselves, but at the ends outside the slot. This space for a given pattern is about the same in a 1,000 as in a 10,000 volt machine, therefore the factor of safety here decreases as the voltage increases.

Another weak point which becomes apparent with the rise in voltage

is the electrical stress between different layers of the single coils; the number of coils being usually fixed by the number of poles, the stresses inside the coils increase with the voltage, and therefore the factor of safety also diminishes in this direction as the voltage increases. Accordingly the test pressures ought not to be in the same ratio for all voltages, and the following rules, drawn up by the Verband Deutscher Elektrotechniker for the testing of insulation of machines and transformers, appear to be of the best practical value:

Working pressure = P.

For $P <$	5,000,	test pressure applied for $\frac{1}{2}$ hr.	$= 2\,P$
„ $5{,}000 < P <$	10,000	„ „ „ „	$= P + 5{,}000$
„ $P >$	10,000	„ „ „ „	$= 1\frac{1}{2}\,P$

In first-class works, however, the finished machines are tested at a higher pressure than is provided for by those rules, and the single parts are tested, before being fitted, at a still higher pressure.

As regards the risk to the machines in carrying out high-tension tests, nothing need be said in reference to the damage which might happen through setting up an arc and burning both conductors and insulation, but discussions have arisen as to whether the risk of a breakdown is increased by applying a high electric stress during a certain time. As far as the author's own experience goes, he has never held the view that a high-pressure test weakens a dielectric in the same manner as excessive mechanical stresses weaken an elastic metal. It often happens that a material stands a few minutes' test and then suddenly breaks down. This is generally due to a puncturing, or in some cases to a carbonising, effect. On account of the variation of the dielectric factor of the materials employed and of the geometrical form of the electrodes, the stresses of the dielectric vary, and if there are any points where these exceed the disruptive strength, the dielectric punctures and carbonises. This increases the stress in adjacent parts, and the average stress per centimetre, consequently other parts become punctured in turn. The phenomenon was exceedingly clearly observed during a 60,000-volt laboratory experiment made on a tube. A few minutes after applying the current bright brush discharges were noticed burning and carbonising the surface of the tube, which progressed at the rate of 15 centimetres in twenty minutes. It is therefore advisable not to exaggerate testing pressures, but rather to seek safety in well selected and tested materials and in careful and thorough construction. The object of putting the finished machine under a pressure test before starting up would then be to detect any accidental fault which might possibly have arisen during transport or erection.

In all cases the conscientious building up of the machine on the part of the designer is a better guarantee than the voltage test made on a newly erected machine. It must be borne in mind that this latter takes place under the most favourable circumstances, while on the other hand an experienced designer will consider the unfavourable con-

ditions which may arise in actual practice. To demonstrate the effect of such conditions, experiments were made in our laboratory which are depicted by the following curves, shown in Fig. 10.

Curve A shows the relationship between the creeping distance in millimetres and the pressure for clean mica tubes.

Curve B the same for mica tubes sprinkled with dry carbon dust.

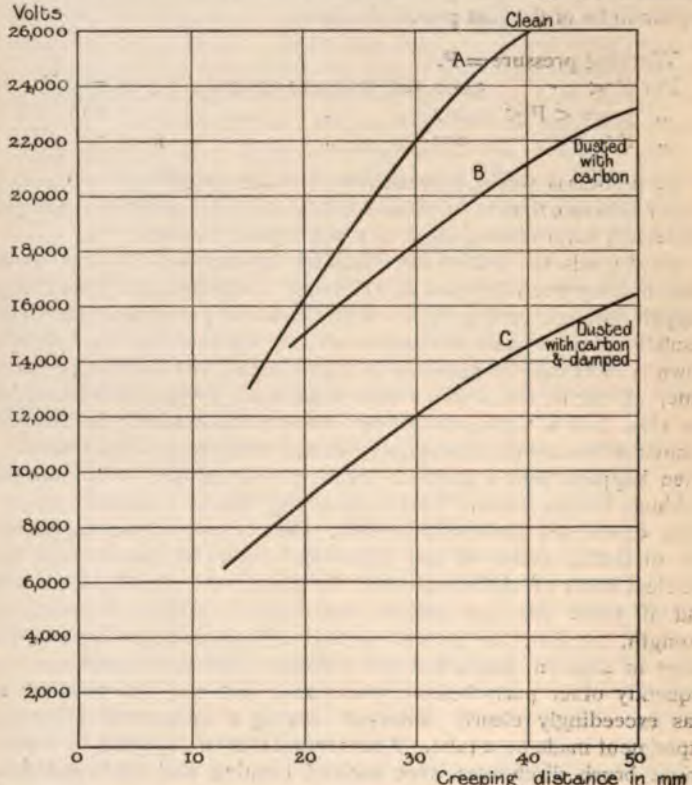


FIG. 10.—Curves showing Relationship between Creeping Distance in mm and Pressure, with Clean and Dusted Mica Insulation.

Curve C that for mica tubes sprinkled with carbon dust and damped with steam.

Although two machines, constructed with different creeping distances, may stand the same normal pressure test, it may be that one is much better suited for practical work than the other. This cannot be ascertained by increasing the voltage of the pressure test. It may happen that the weakest point of the clean machine is the piercing point of the insulation. This would in no way be altered by increasing the creeping distance, but the machine of ample design

would puncture at the same pressure as the inferior machine, given that both were clean. With a well designed machine the factors of safety are of course *not* taken against pressures which may arise in actual practice under ideal circumstances, but against pressures which may be met with in actual practice under unfavourable circumstances, and would differently affect the various parts of the machine. Above all the factors of safety are chosen to prevent troubles which might

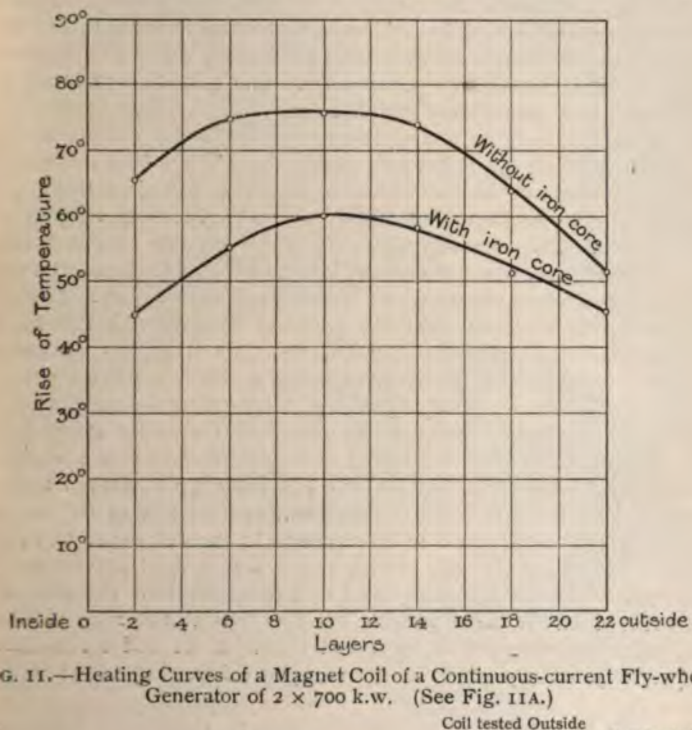


FIG. 11.—Heating Curves of a Magnet Coil of a Continuous-current Fly-wheel Generator of 2 × 700 k.w. (See Fig. 11A.)

	Coil tested Outside: Without Pole.	Coil tested Outside: Fixed Pole.	Coil Tested on Machine Running.
(a) Maximum inside temp.	77°	60°	—
(b) Average measured by thermocouple	70°	54°	—
(c) Average measured by variation of resistance ...	72°	55°	35°
(d) Outside temp. measured by thermometer ...	48°	40°	9°
(e) Ratio $\frac{c}{d}$	1·5	1·4	4

lower the insulation value, such as dust in central stations, moisture in mines, and deposits in rooms with very moist atmosphere, etc.

Ten years ago good machines used to run without any appreciable rise of temperature, due to the fact that, experience in those days as regards commutation, regulation, materials, etc., being limited, disproportionately high factors of safety were usual. The machine was considered faulty if any part of it heated up beyond 30° or 40° C. At

the present day the only reason why we are interested in the temperature is the harm it may ultimately do to the machine, and especially to the insulating material. From experiments made on samples of cotton-covered wire by the National Physical Laboratory of Great Britain, 125° C. was taken as an approximate safe limit. With an engine-room temperature of 35° C. there would thus be left a margin for heating of about 90° C.

The temperature of the outside of the machine is easily measured with a thermometer, and in making special researches the inside temperature can be measured by a thermojunction, a method, however, which is impracticable for actual work. The temperature inside the coils varies to a great extent with the construction. Heat is generated equally in the body of the coils and passes to the outside and so to the surrounding air, poles, pole-pieces, etc. There is a remarkable drop of temperature in every layer of insulation, this drop being proportional to the thickness of the layer and to the local intensity of the flux of heat. The complicated nature of the phenomenon is demonstrated by Fig. 11, which shows the inside and outside temperatures of the same coil when mounted and dismounted from its pole. It would not be worth while to treat the problem by theory, based on the specific heat-conductivity of cotton, etc. Even though we cannot measure the maximum temperature inside a machine, when the latter has been built—and strictly speaking this is what interests us—the change of resistance offers a very convenient means for ascertaining the mean temperature. According to the Regulations of the Verband Deutscher Elektrotechniker the average heating of magnet coils is measured by this method. A mean temperature rise of 60° is permitted, which corresponds to an increase in the resistance of 24 per cent. This mean value also corresponds, as a rule, to a temperature rise of 40° C. at the exterior, measured by a thermometer. For machines constructed on the same pattern the variation of the ratio of mean to outside temperature, as measured by resistance and thermometer respectively, is very small, as shown by the following table:

AVERAGE VALUES OF TEMPERATURE RISE IN DIFFERENT MOTORS.

H.P.	Heating of the Field-coils as Measured by:—		$\dfrac{\text{Temperature as Measured by Resistance}}{\text{Temperature Measured by Thermometer.}}$
	Resistance.	Thermometer.	
	Mean Temp.	Outside Temp.	
20	54°	32°	1·68
24	50°	30°	1·68
30	45°	27°	1·66
40	55°	33°	1·65

FIG. 11A

FIG. 12A.

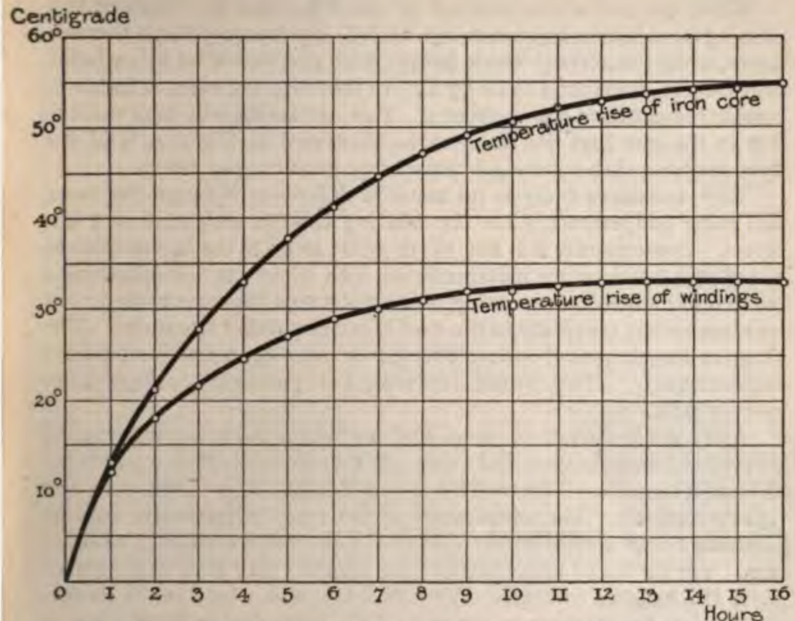


FIG. 12.—Heating Curves of 7·5-k.w. Transformer. (See Fig. 12A.)

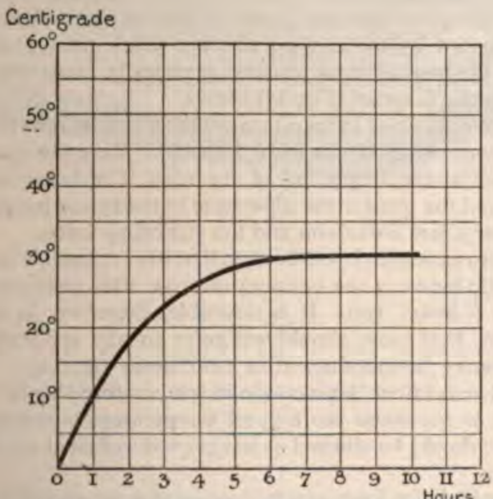


FIG. 13.—Heating Curve of Magnet Coils of 1,600-k.w. Alternator, 500 r.p.m. (See Fig. 13A.)

When the coil above referred to was fitted and the machine was running an outside temperature rise of 9° C. was measured by a thermometer, while the average inside temperature rise, measured by variation of resistance, was found to be 35° C. In this case the ratio of inside to outside temperature rise is about 4. This extraordinarily high value is due to the fact that the machine, as illustrated in Fig. 11A, is of the flywheel type, thus ensuring a remarkably good cooling effect.

With armatures there is no material difference between the inner and outer temperature, since the winding consists only of a very few layers. Consequently it is not worth while to go to the inconvenience of indirect temperature measurements with direct-current armatures; particularly since the measuring of the resistance becomes complicated and inaccurate, especially in the case of series parallel armatures. The German Regulations therefore provide in these cases for thermometer measurements. They limit, however, the permissible temperature rise to 50° C.

As to the influence of time, Fig. 12 shows the heating curves of a 7·5-k.w. transformer, and Fig. 13 that of a 1,600-k.w. turbine-driven alternator. The machines are illustrated in Figs. 12A and 13A respectively. The temperature of the small transformers became stationary after a 16-hour run, that of the alternator coils after a 6-hour run. For the transformer the cooling effect is very poor as compared with the heating capacity. We found that this effect in all Felten-Guilleaume-Lahmeyer transformers of the same design from 3 up to 100 k.w. became stationary after a run of about 16 hours, and the temperature rise after a 6-hour run was for all of them 80 per cent. of its final value for the copper and 66 per cent. for the iron. But with well-ventilated high-speed machines, even in the first hours the amount of energy dissipated by the cooling effect is much greater than the heat capacity of the masses, and so they practically reach the maximum very soon, as the diagram (Fig. 13) shows.

The temperature rise in an oil transformer can be seen from Fig. 14. The transformer itself is shown in Fig. 14A. Here the lower parts do not heat at all at the beginning of the test. The lower the thermometer is placed the greater the difference between the temperature rise obtained after a few hours' run and the stationary value.

From these examples it will be seen that it is impossible to establish a universal ratio between the temperature rise after continuous running and after a 6 hours' run. It is desirable, therefore, in drawing up general rules, that these should not refer to any arbitrary time, but to the stationary temperature after continuous running.

In actual tests either the increase in temperature should be followed up in order to see when the highest temperature is reached, or such running time should be allowed as has proved sufficient for machines of similar type.

With machines for long intermittent service the period of the service should be used as a basis, while with machines for short intermittent service one should endeavour to take into consideration the special

FIG. 13A.

character of the work. In order to avoid complications, however, it is customary in the latter case to take as a basis a 1-hour continuous run.

The greater the heat capacity of the mass of the motor in comparison to its radiating capacity, the more will these values differ from the real value.

If there is no opportunity for fully loading the machine, an idea of its thermal characteristic may be obtained by the well-known method of running it alternately with a higher voltage and with a higher current, so that in each case the full losses are obtained. For example, the following is the method we have employed to test a single 200-k.w. transformer for a voltage of 10,000/28,000, in which the iron losses

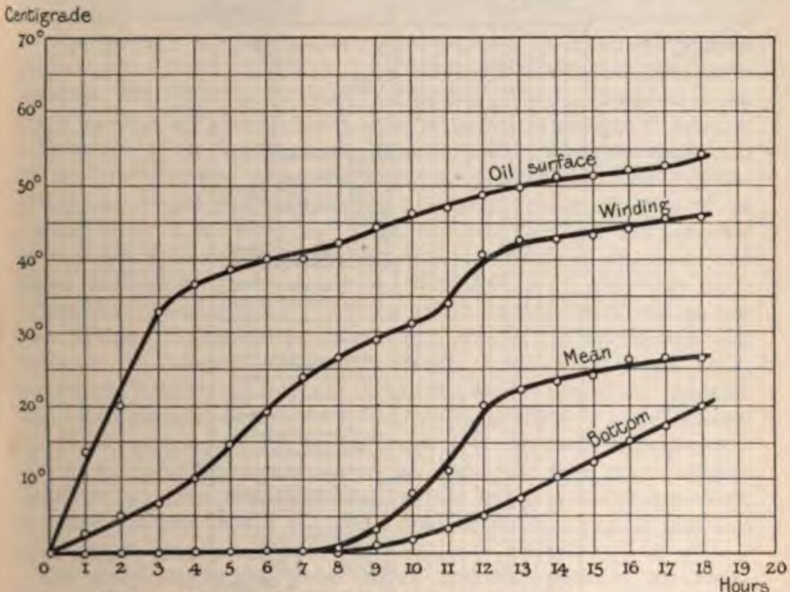


FIG. 14.—Heating Curves of Oil Transformer. (See Fig. 14A.)

amounted to 3,420 watts, and the copper losses to 2,500 watts, under normal working conditions. The transformer was first run for one hour with 13,900/34,700 volts, corresponding to losses of 5,830 watts. It was then run for one hour short-circuited with 19·3 amperes and with 8 amperes respectively, corresponding to losses of 7,040 watts. Though in each case the losses were practically the same as those occurring under working conditions, by continuing the test for a sufficient length of time it was possible to gauge the temperature rise of the machine.

The object of the heating test is solely to ascertain that the recognised standard limits for the heating are not transgressed. Whether the temperature of a machine increases 20° C. or 50° C. is otherwise

wholly immaterial, and it would be entirely premature to draw a conclusion as to the ample design of the machine from the increase of temperature alone. A cooler machine is certainly an advantage, inasmuch as it may be used in warmer localities, but apart from this a low temperature rise is of no practical value if the machine cannct be overloaded owing to its commutator beginning to spark, its regulation being insufficient, or perhaps on account of its mechanical construction, or owing to being driven by a gas-engine. It is not even certain

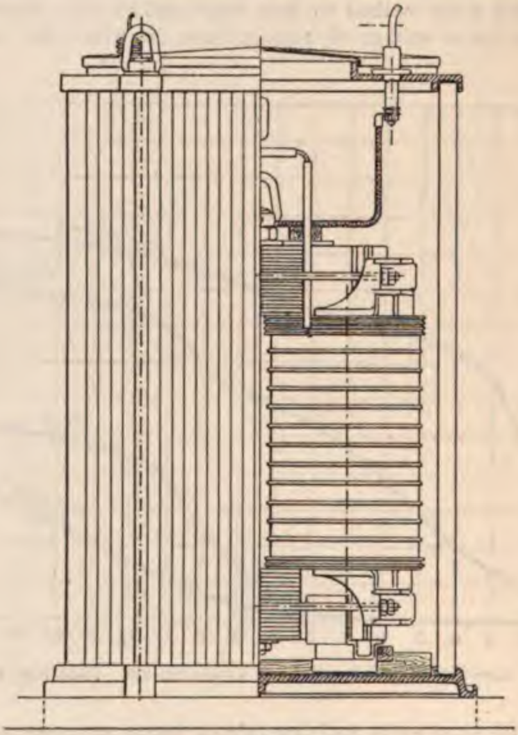


FIG. 14A.—Oil Transformer.

with regard to the thermal qualities that if of two machines A shows 30° C. and B 40° C. increase of temperature with normal load, that A will allow of a higher overload than B, before the permissible limit of 50° C. is reached. This might actually not be the case if A worked with small iron losses, with great copper losses, and with high magnetic field saturation.

Finally much depends upon the reason why one machine heats more than another. If, for example, in a gas-driven plant one of two similarly constructed machines runs at a temperature of 50° and

the other at 30°, the difference being due alone to the fact that the armature openings have been covered in the former case to prevent the entrance of gases and dust, the machine with a 50° rise is to be preferred to the other cooler machine. Extraordinarily low temperatures may no doubt be preferable under certain conditions, if, for instance, a very exact constancy of field is required, with low saturated magnets, but, as a rule, if the temperature does fall below the given limit this does not constitute any particular advantage.

The efficiency of electric machines is defined by the ratio between input and output of energy. The output is, as a rule, mechanical or electrical, the input the same, or a combination of both. The accuracy of mechanical measurements, both by brake for output and by indicator or hydraulic tests for input, is much behind the accuracy of electrical measurements. It is therefore good practice to avoid mechanical measurements, and to check by electric measurements the losses defining the difference between input and output. However, even when dealing with input and output of electrical energy alone from such machines as transformers, converters, and motor-generators, etc., more accurate results are obtained by testing the losses than by comparing input and output. In the first place an inaccuracy of an instrument of 1 per cent. will affect the result by 1 per cent. in comparing input with output, whilst the same inaccuracy in measuring the losses will affect the value of the efficiency by $\frac{1}{10}$ per cent. at a maximum. For supposing that the efficiency may be about 90 per cent., an inaccuracy of 1 per cent. in measuring the losses will influence the value of efficiency only to one-tenth of 1 per cent., the losses themselves being about 10 per cent. of the input. Besides this, instruments for measuring the losses are mostly more reliable and the arrangements less complicated. As the designer calculates the losses, and these are tested in a simpler and more reliable way, the guarantees ought to be based on them, as it is important that they should be of a kind which both the designer and the user can check.

To check the copper losses at full load in armatures, the resistances ought to be measured after a regular normal run. To correct the resistance for the heating at half or three-quarter load is unnecessary, as at these values an inaccuracy of the copper losses scarcely affects the efficiency. Special care must be taken with continuous-current armatures in regard to the different brush contacts, and to the exclusion of the contact resistance of carbons. The results will scarcely be exact within a few per cent., but an error of even 5 per cent. would affect the total losses only by about 1·5 per cent., and the efficiency by not more than about 0·1 per cent. In special cases, where heavy current armatures are concerned, the resistance may be calculated from the copper dimensions.

As regards the testing of iron losses a very simple method was announced last year in the technical press. To investigate those of a several thousand kilowatt alternator small samples of the iron used in

the armature were reserved and tested in the laboratory at the same inductance and frequency as in the machine. By multiplying the figures with the ratio of weights of the iron in the armature and of the sample, the iron losses in the armature were supposed to be checked. Every experienced designer who has ever compared actual losses in machines with the losses calculated on the above basis is aware that the method is faulty and that the results will be too low.

What are guaranteed as iron losses in machines are not the losses which occur in the unfinished iron, when the lines of force are uniformly distributed in exactly the way which the designer has prescribed in the armature core. There are the additional losses due to varying density, dispersion, and above all to hysteresis and eddy current losses in other parts of the machine, whether solid or laminated. The data necessary for the computation of these losses can only be obtained by thorough research, and not alone by simple measurements on a test-piece in the laboratory.

Thus in the early days of dynamo manufacture a difference between the losses calculated by the Steinmetz formula and those found by tests on machines was observed, to compensate for which the multiplication of the theoretical losses by a certain factor was proposed. Fig. 15 refers to observations made at that time on continuous-current machines. The full lines show the losses as found by testing laboratory samples and as measured on actual machines. For a varying induction both for 15 and 30 $\sim$ the ratio between losses in the machinery and losses in the laboratory samples increases both with the frequency and the induction, as will be seen by the dotted lines. This is principally due to the fact that there are more eddy currents in a dynamo than in a laboratory test-piece.

Assuming that eddy currents vary with the square of both the frequency and the induction, while hysteresis varies directly with the frequency and with the 1·6 power of induction, it will be seen that it is impossible to find a constant factor which will give the exact actual losses by multiplication of the theoretical factors. To emphasise the difference as much as possible, the curves are based on measurements taken from out-of-date machines; but similar results will be found in modern machines. We therefore see that to calculate the iron losses of a machine we must take into consideration the machine running under normal conditions, and to calculate the iron losses either for the purpose of designing a machine or for guarantees we must base our calculations on curves of losses as determined from actual machines.

The most thorough method of guaranteeing a machine consists therefore in basing the guarantee on the most unfavourable values, obtained from previous machines of similar construction. Such values will be found to differ to a very considerable degree from those deduced from theoretical considerations.

In illustration of this some curves of iron losses are given in Fig. 16, which are taken from a few of the best standard works on

electrical machine construction. The figure also contains a curve of loss, designed by the author as a basis for guarantees. Naturally the efficiency which has been calculated for a certain machine is very greatly affected by the choice of the curve of iron losses. The curves have been taken as a basis for the calculation of the efficiency of a certain machine in the next figure (Fig. 17). As may be seen,

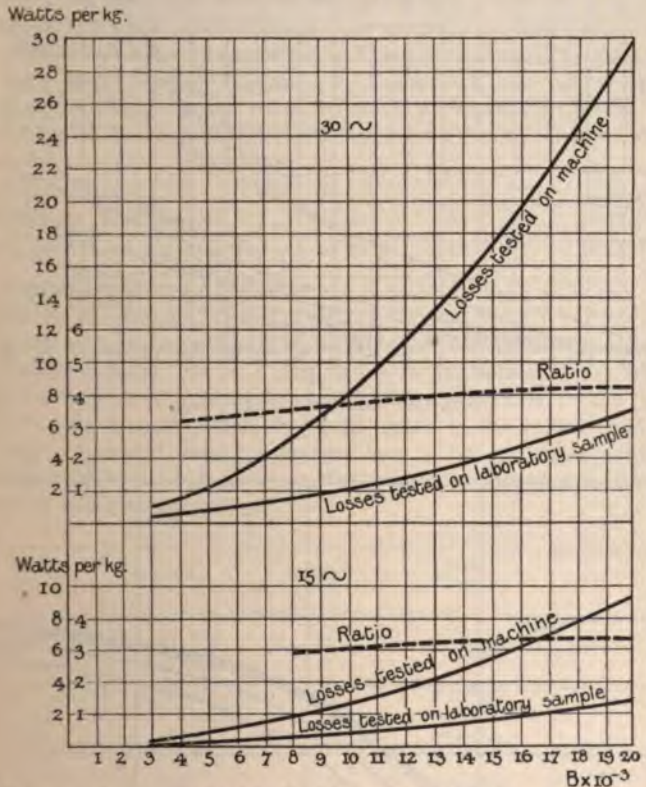


FIG. 15.—Losses as tested in Machines and in Laboratory Samples.

differences occur of 3 per cent. at full load and of 5 per cent. at half load.

In order, then, to establish a comparison of guarantees as regards iron losses and to enable any one to judge the comparative value of different machines, it is essential that these guarantees should be calculated on identical lines. Because otherwise in specifying the efficiency of a machine a low efficiency is often not so much due to the fact that the machine offered is an inferior one as that the specification has been worked out in a more thorough and conscientious manner.

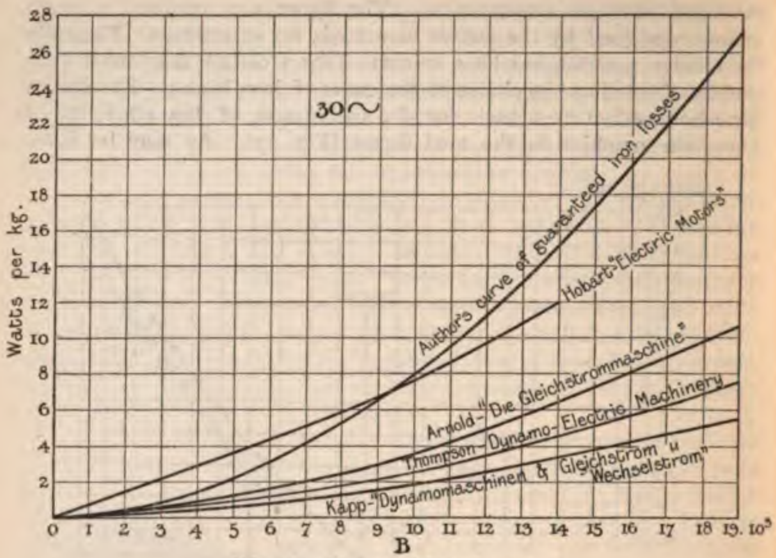


FIG. 16.—Curves of Iron Losses.

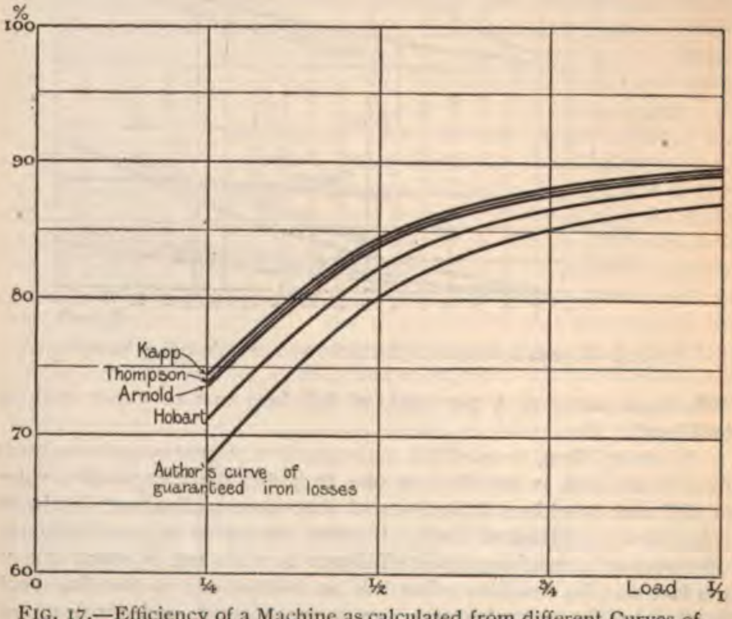


FIG. 17.—Efficiency of a Machine as calculated from different Curves of Iron Losses.

Iron losses can be accurately tested with transformers or motor-generators. With continuous-current machines special care must be bestowed on the position of brushes. Fig. 18 shows the variation observed on a machine running as a motor with different sparkless positions of brushes. In the case of motors the manufacturer for his own information can separate friction and iron losses by different methods, but the buyer will be satisfied with a combined test. With engine-driven generators indicator tests give the no-load losses of the set and the losses due to excitation and magnetisation. The inaccuracy of light-load indicator diagrams is diminished, but not altogether avoided, by throttling the steam to work with lower pressure.

For instance, a 400-k.w. steam alternator gave the following results :—

	Minimum.	Maximum.	Average Value.
Friction losses	37·5	42·8	40 H.P.
Friction and iron losses	54·6	60·3	58 H.P.
Iron losses	11·8	22·8	18 H.P.

A great number of diagrams are therefore required to get some degree of exactness.

If the voltage available differs a little from normal a correction may be made, assuming that the losses vary with the square of the voltage. This assumption is correct in most cases, so that it is not worth while to refer to any special laws with regard to the relation between hysteresis loss and the eddy current loss. This is evident from the following data which have been observed with reference to the iron losses of a direct-current machine.

Volts.	Watts.	$\frac{\text{Watts}}{\text{Volts}^2}$.
244	340	0·00572
327	605	0·00565
415	970	0·00563
500	1,405	0·00562
552	1,715	0·00562
589	1,965	0·00566
	Average ...	0·00565

These figures show that the iron losses of this machine built for a normal working pressure of 500 volts can be correctly represented between the limits of 244–589 volts by the formula : Watts $= 0·0057 \times$ volts^2.

Testing is simplified if the efficiencies of engine and generator are not guaranteed separately but combined. In this case the result will be obtained by indicating the engine and measuring the output of the generator. The simplicity of this method and the importance of the result of this test for the buyer has brought it into frequent use. Sometimes the iron losses can be tested in a more accurate way than by the indicator method, if the generator can be disconnected from the steam engine and if a second set is available to run the first when fully excited. If required in this test the separation of friction from iron

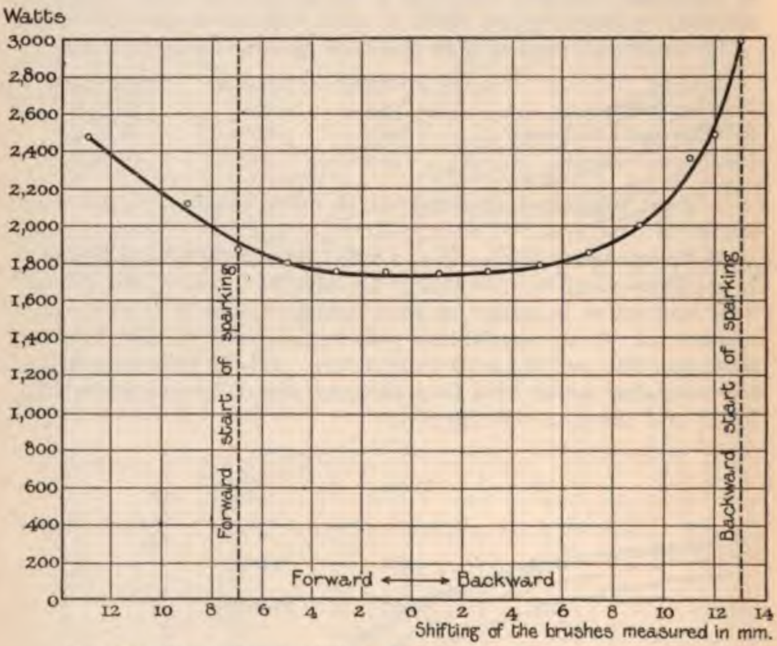


FIG. 18.—Curve showing Effect of Varying Position of Brush Gear.

losses can be effected either by varying the induction and extrapolation or by the running down method.

Though the accuracy of electrical measurements is much higher than that attainable in other branches of engineering, we must bear in mind that it is not absolute. Even with continuous-current tests special care is required to measure current or voltage within 0·5 per cent. In dealing with high-pressure wattmeters it is very difficult to get within 1 per cent., though the readings may be accurate within 0·1 per cent.

Taking into account the inaccuracy of mechanical measurements, it appears absolutely necessary in the case of efficiency tests to allow a margin of error; up to 2 per cent. being commonly agreed upon.

Even higher margins must be allowed in hydro-electric plants, particularly when working with a small head of water.

Formerly the losses in the carbon brushes in direct-current machines were underestimated and regarded as negligible. The friction losses are included in the total friction losses of the machine, while those due to the resistance of the carbons are ascertained either on the basis of curves, plotted for the different kinds of carbon brushes, as shown in Fig. 9, or by adding one insulated brush and measuring the difference of potential between it and the current-carrying brush.

The slip losses of 3-phase motors can be ascertained either by measuring the copper resistance and the current absorbed, or in the case of slip-ring motors the frequency of the rotor can be measured by means of a millivoltmeter.

Moreover, against the no-load method the objection has been raised that under load additional iron losses will occur in the machine due to distortion of the field, and with a direct-current armature additional commutation losses occur in the short-circuited coils. Though theoretically these objections are well founded, they are the less important the stronger the field and the better the commutation. Therefore the better the machine is, the less these losses may be considered.

Efficiencies measured directly and indirectly coincide, as a rule, within the limits of the error of the observations, which is about 1 to 1·5 per cent. The author therefore prefers the simple and more exact method of the separate loss measurements. It is, moreover, his opinion that this method should be used for calculating guarantees, as it is important that guarantees should be capable of being checked in a simple and unobjectionable manner.

There is no difficulty in ascertaining power factors. Due consideration should be taken that possibly differences exist between the phases in two or three phase plant, caused either by a slightly unbalanced line or in certain cases by an unbalanced motor winding, which frequently cannot be avoided if the motor is designed so as to be separable into two or more parts. The simplest way to obtain the power factor is by the 2-wattmeter method. Slight inaccuracies of the wattmeters may be eliminated if necessary by interchanging them.

There is no doubt that the greater the value of cos ϕ the greater the advantage to the owner of the machine, and particularly to the supplier of energy, but in considering the question care must be taken that it is not attained by reducing the air-gap to too fine a degree.

With an alternating current the regulation is defined as the percentage drop of the voltage between no-load and full-load current with a certain power factor. In the case of transformers, however, it is usually ascertained by short-circuiting the secondary by an amperemeter and observing in the primary the voltage required to give the normal amperes. But as a particularly good regulation is essential with transformers, it should not be obtained by reducing to a minimum the distance between the high and low tension coils. In comparing transformers for a very high voltage the author would prefer one with

ample distance between the high and low tension coils, even if the regulation is poorer. Apart from special cases, such as machines for electric furnaces or tramway machines to work in parallel with buffer batteries, the closer the range of the regulation of generators the better. The very small values, however, as a rule are not attainable, on account of the variation of the speed of prime movers. Furthermore, one must not lose sight of the fact that with direct-current dynamos and motors a too great degree of regulation might produce oscillations and fluctuations between machines working in parallel, and small variations of voltage will influence the amperes to a great extent.

The overload capacity for motors must be tested both as regards starting and running. In calculating the torque of alternating-current motors one must not lose sight of the fact that if the torque varies with the position of the rotor the user has to reckon with the lowest value.

For measuring the torque the most convenient method is perhaps to start against a spring balance attached to a rope passed over a pulley, and to allow the motor to choose a position, but more accurate results are obtained by adjusting a lever in different positions and allowing it to raise a weight.

The overload capacity of alternating-current motors is often ascertained in the simplest way by lowering the voltage until the motor ceases to run. In motors for pumps, fans, ventilators, and such-like, where a variation of the torque will never occur in actual service, it is this overload capacity (stability when the voltage falls) which is of chief importance to the user.

Machines, as a rule, are not intended to run with a continuous overload, though it may be important for them to be able to do so for a hort time. According to the German Regulations machines should, as a general rule, be tested with a 25 per cent. overload for half an hour, and with 40 per cent. overload for three minutes.

It is perhaps enough if these prescribed conditions are fulfilled, and the manufacturer should not be pressed to guarantee better results. In building up his machine, he should take all precautions to ensure that it works well under normal conditions, and should not risk mpairing it in this respect by introducing an undue overload capacity.

With continuous current machines the maker must devote attention in designing a machine to making it run absolutely sparklessly. Formerly, when the shifting of brushes was usual for dynamos, we checked the commutation of our machines by ascertaining at different loads the most forward and the most backward position of the brush gear before any sparking was noticed. This is clearly shown by the diagram in Fig. 19, taken from a machine (Fig. 19A) running without any sign of sparking from no load up to 1,400 amperes, and without any injurious sparking from no load up to 1,600 amperes, the brushes being always in the same position. The neutral zone at no load equals 30° and at full load 10°. Even if the driver shifted the brushes and did not take the best position for the given load, there is ample margin for varying the load without sparking. Tests of this description are

less important at the present time, since with compensated machines a fixed position is guaranteed by the construction of the machine.

A fairly good idea is obtained from the commutation diagram. Fig. 20 shows some examples of commutation curves taken from a machine working well and from one working badly.

No doubt a sparking machine is bad, but even if the machine under test does not spark, this does not mean that it is good. It often happens that a machine which runs, say, fairly well to-day will give trouble after some months of actual work. What is required, and what cannot be checked by a mere test, is reliability. This reliability in the first place is guaranteed by the strength of the mechanical construction. A

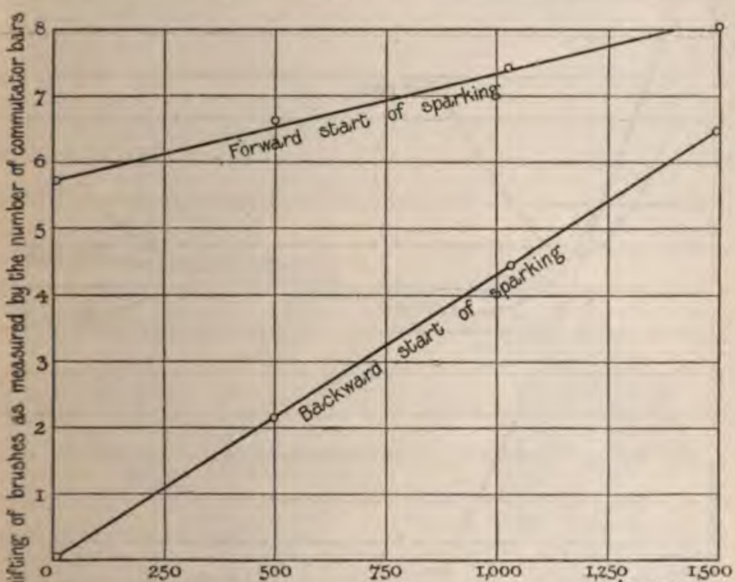


FIG. 19.—Width of Neutral Zone at Varying Load. (See Fig. 19A.)

machine running well under good mechanical conditions may give trouble if these conditions change through wear and tear, just as in testing the materials for a machine we must not lose sight of the fact that the electrical part, although important, is only a part, and that a machine to be good must be sound both from an electrical and a mechanical point of view. It is needless to state that there exist a number of methods and instruments for testing a machine electrically, but it is rather more difficult to test the mechanical part. The simplicity and the exactitude of electrical measurements are important for the progress of the industry, but one is apt to overestimate the points which can be checked without difficulty, to the detriment of those which can be checked only by experience.

It is necessary to have certain standards by which to simplify and strengthen the manufacture in general. As far as necessary in the present paper, the author has followed the German standards, partly because they are familiar to him, and partly because he considers them good. A machine must fulfil certain requirements, otherwise it is bad. But if these requirements are exceeded, the degree of excess is not necessarily a criterion of the quality of the machine as a whole, nor even of its value in respect of any particular requirement which may be exceeded. A 500-volt direct-current machine, which will stand a

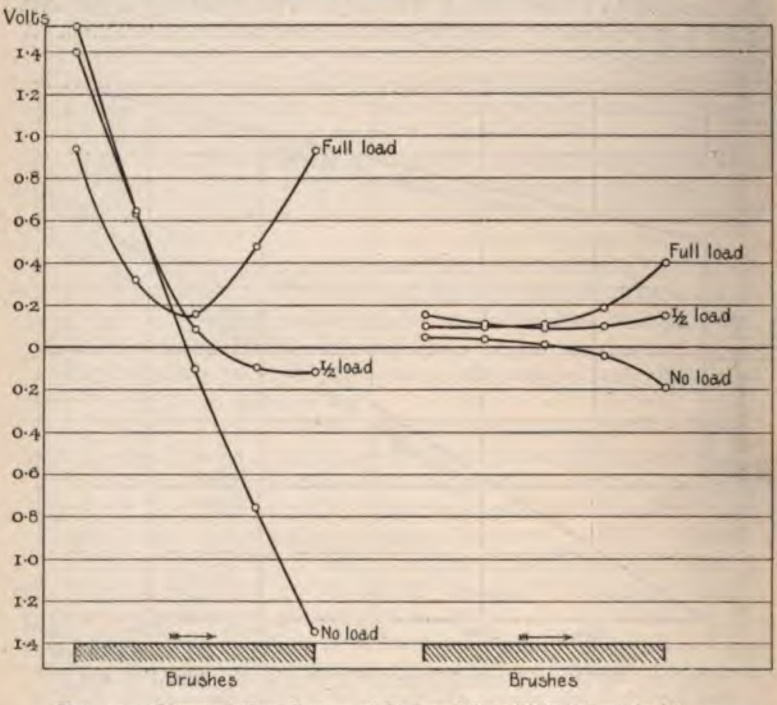


FIG. 20.—Commutation Curves of Bad and Good Working Machines.

3,000-volt insulation test, may not have so good an insulation as another which the manufacturer would trust with only 1,000 volts. For such a machine will never be called upon to stand 3,000 volts in service, whereas copper and coal dust and want of cleanliness in caring for the machine are conditions which are likely to occur. The fitness of the machine to withstand these may be guaranteed by a thoroughly well-considered design, based on experience as to what may prove the weakest point under bad circumstances. The same applies to sparkless running. If two new machines run sparklessly with a final test within the overload limits as given, say, by the German Regulations, the experienced

FIG. 19A.

designer, in comparing them, will not ask if he can go 10 per cent. higher with A or 30 per cent. higher with B, but he will ask how the sparkless running is obtained and definitely guaranteed.

If with B the sparkless running is due to a scrupulously accurate adjustment of the brushes, to the employment of only a certain quality of brush, or to the absolute truth and steady running of the bearings and a highly polished commutator, whereas if with A it is due to structural details, such as the use of compensation poles, ample dimensions of the cross connections and commutator, the designer will recognise in A a better machine in every way, and even a cheaper, though its price will naturally appear higher.

Efficiency as far as motors are concerned is very important in connection with a central station, where a high price has to be paid for power, but as regards generators this point is often over-estimated. It is not worth troubling about one-tenth of 1 per cent. if it cannot be checked with accuracy and if you are losing efficiency in your engine and boiler house. Insist upon efficiency in your boiler house and renounce a few tenths per cent. of efficiency in your machine, if by this it can be improved as regards regulation overload capacity or safety. For instance, we often decrease to a great extent the efficiency of low-tension direct-current machines by increasing the number of brushes. With a lower number of brushes the machine would stand all that is required by the specification, and all that can be checked by the test, but we prefer a machine fitted with a greater number of brushes, as it will stand both an overload and bad attention. The latter is a very important point, as there is a great difference between a machine enduring a test under testing conditions and being able to withstand all irregularities which may happen in actual service, however strong and safe it may be in every respect. Features like this can only be guaranteed by good design based on ample experience, good selection and checking of the materials, and conscientious work in all matters connected with the tendering for and the construction and erection of the machine.

From the foregoing observations it is clear that one must not depend only upon measurements in order to form a due estimate of the capabilities of a machine, nor must one regard it as a better machine the better the values that are obtained.

Such theoretical calculations must always be supplemented by taking due account of the constructive details of the machine, and also of the experience gained from acquaintance with machines of similar construction and origin. Hence the importance of selecting the most suitable materials and of testing them in a practical manner. There is also another personal reason which has led the author to deal in this paper with the testing of materials on the one hand and the testing of machines on the other, namely, that he may be regarded as defending the interests of the consumer in the one case and those of the manufacturer in the other, and having come to the same conclusion from both points of view, this may serve to strengthen his arguments.

Good factors cannot be based on the amount by which the standard values are exceeded, but the practical points must be considered. Where accurate measurements are possible, such measurements should be made by all means; but at the same time the qualities which cannot easily be gauged and are equally important must not be lost sight of. In all cases where measurements do not suffice one must take into account the workmanship, and, above all, make use of the experience obtained from similar objects based on the same lines.

Discussion.

The President.

The PRESIDENT: There are two or three words of explanation that I should like to make this evening. Most of you know that the questions which are touched on in this paper have been discussed and considered very fully by one of the Engineering Standards Committees during the last year or year and a half; and, in order to obtain material for their discussion, the Committee consulted a number of gentlemen in England and in other parts of the world, and received from them very valuable advice and help. But from few, or any, of those whom they consulted did they receive advice and opinions in answer to their questions—and their questions were searching ones—which were of more value and interest than those which were received from our friend Professor Epstein. Professor Epstein has now put the information that he had so generously and freely given to the Committee at the disposal of our Institution, and the paper which you have heard read this evening is the result. We are privileged this evening to welcome amongst us Mr. Skinner, of the American Westinghouse Company, who, on the other side of the Atlantic, has done at least as much as any other man in the country in making tests and experiments of this nature. Mr. Skinner leaves for America in the morning, and if he feels able to say a few words now, or to add anything to the discussion that will take place subsequently, I am sure the meeting will be glad to hear him.

Mr. Skinner.

Mr. C. E. SKINNER (Pittsburg): It is certainly a great privilege for one who has taken a good deal of interest in the proceedings of this Institution on the other side of the Atlantic to attend one of its meetings, and to hear the paper which has been read this evening. I was very much interested in many points, particularly with regard to what would seem to be the allowable temperature rise in machinery. I can hardly believe that the Professor would be willing to put out a machine which would have a 90° temperature rise. The usual limit of rise of temperature called for in contracts and specifications in the United States is 40°C., this figure being adopted as the result of long practice. There seems to be a tendency on the part of users of electrical machinery to force it down to 35° C. or 30° C. I may be wrong in assuming that the figure 125° C., given as the limit at which fibrous materials may be run without deterioration, is taken by the Professor as allowable in electrical machinery. This seems to me to be a very excessive amount, and that long-continued runs at this temperature on the insulating materials

Mr. Skinner.

which are necessarily used in dynamo construction will certainly cause serious deterioration. The result depends on both time and temperature, and the time must be taken into consideration.

We have been endeavouring in America to get down to some sort of standard specifications for the purchase of materials used in construction of electrical machinery. In the working out of these specifications we find that it is absolutely necessary that the specification should be satisfactory to the producer of the materials as well as to the purchaser, otherwise we must pay excessive prices for the materials. Specifications are sometimes written merely to describe a material, so that the manufacturer will know what the user of the material wants. It frequently happens that the producer of the materials has no idea of the use to which they are to be put; consequently a brief specification will enable him to furnish the grade of material best suited to the work. In other cases we find it necessary to specify certain qualities and to demand that the specifications be met to the letter. We find it very difficult to write a specification for a material without considerable experimental work in the laboratory. We must make many and long-continued tests to find the limits which may be imposed; and, in general, we should make the specification as lenient as it is possible, and still cover the material which is desired. Some of the difficulties in making satisfactory specifications may be illustrated by a story that is told of a certain man who was designing a submarine boat. He specified that all the steel used should have an elastic limit of 300,000 lbs. per square inch. When he was told that it was entirely impossible to obtain such steel, he said that his design failed unless he got it. We are compelled to take the conditions as they exist and make the best of them, and we should make our specifications cover the commercial conditions as nearly as possible.

Discussion at Meeting of December 6, 1906.

Mr. Patchell.

Mr. W. H. Patchell: I find it is very interesting to see the way that Professor Epstein has put before us very intricate problems, and has used common-sense where other people probably would have used higher mathematics. One matter which I have come across in my experience is hardly referred to in this paper, namely, in the question of testing, the power behind the test. In testing cables, for instance, or any machine which has considerable capacity in it, it is absolutely useless to set out on that test unless we have not only pressure but plenty of power behind it, because we may get a breakdown and not be able to follow it up. I have had considerable difficulty and delay in testing cables while obtaining experience in that direction. We had a faulty cable, and we could get a kick on the instrument, but we could not localise the fault, because having punctured it we had no power to follow it up and actually show the bad place and point of failure. On page 42 the author refers to the desirability of machines

Mr. Patchell.

being able to stand surges. I take it that there he refers to pressure surges. When we are concerned with large plants and machines working in parallel, the ability to stand current surges is equally important. There are machines which will run excellently by themselves, but their cross connections, which under the normal working of the machine are lightly loaded, become overloaded during certain conditions which arise in practice. These connections are sometimes of too small a section, and break down, so causing considerable trouble in repairing. I have had machines which would run most beautifully without a spark, and yet if we tried to warm them for drying out in the way we have been used to for some years, the internal connections would give trouble. There may be difficulty in arranging internal connections of sufficient area, but I would ask manufacturers to bear in mind that we have in practice to deal with current surges as well as pressure surges. On page 43 the author refers to a matter which has always been a pet idea of my own, and says I am to throw it away! It is the question of a high pressure test weakening a dielectric. Many points which have come before me have rather confirmed, or tended to confirm, my belief that you may strain and weaken a dielectric, so to speak, unconsciously, just as you may, when testing metals, exceed the elastic limit, and so weaken the metal without knowing that you have done so. I have always been under the impression that a high pressure test might leave the machine or cable tested worse than before testing. It may be that the danger I have anticipated is really due to incipient carbonisation, so that if you could lay open or closely inspect the cable you might find that incipient carbonisation had set up, which subsequently would shorten the path and lead to a breakdown. Professor Epstein rather scouts that idea, and says that as far as his experience goes he has never held the view that a high pressure test does weaken a dielectric. In Fig. 10 some curves are given, but no reference is made to time. That seems rather strange to me, because in Fig. 8 time is taken into account. I should have thought the time also ought to have been important and ought to have been mentioned with the creeping distance referred to in Fig. 10. On page 43 we certainly have a reference including time, where 15 centimetres creepage in twenty minutes is mentioned, so that the Professor is quite well aware of the influence of time, and I think it would have been more helpful if we had had that influence plotted on Fig. 10. With regard to the question of oil transformers, the result shown on page 49 is interesting, but there is an absence, I think, of any data as to the size of the oil transformer either in kilowatts or inches. If we could get some idea of the size of the transformer, it would help us better to appreciate the curves shown in Fig. 14. I take it that the transformer had been allowed to stand with the oil in it and soak through, if I may use a common expression, so that the temperature at the start was uniform. In Fig. 14 all the curves start from zero, so I take it they were actually allowed to soak before starting. There is a very rapid rise at the oil surface and less rapid in the winding, and why the bottom and the

mean—I am not quite sure where the mean is—should go along without any rise whatever for seven hours is rather puzzling. On page 51 reference is made to the efficiency of machines. The efficiency is very shortly stated as the ratio between input and output of energy. I remember, when going into the method of testing the efficiency of machines with Professor Epstein some six years ago, we made out, I think, six different possible methods of testing the efficiency, so that it would be rather important to know not only what the efficiency is to be, but to settle before starting how it is to be determined. The particular machines on which we had such a wide range of choice of methods with varying results were motor-generators. The Professor makes a point as to the comparative crudeness of mechanical methods of testing. I remember Mr. R. K. Morcom, of Bellis & Morcom, Birmingham, telling me an interesting experience he had of a test where, in the absence of a direct-coupled dynamo, which is the simplest method of testing steam engines, they test with a Heenan and Froude water brake. He checked with very considerable accuracy the value of "J" by the diagrams of the engine and the heat put into the water brake. On page 52 the question of guarantees is dealt with. I think sometimes too much importance is attached to guarantees. It is equally important not only to know the numerical value of the guarantee but the personal equation of the firm giving the guarantee, because I think many of us in this room would rather have one man's 90 per cent. than another man's 92 per cent. One of the most important points in the paper refers to the same question of guarantees, and is dealt with on page 57. It is not only important to settle the guarantees, but it is absolutely important that there should be easy means of checking whether those guarantees can be sustained or not, in, as the Professor aptly puts it, a simple and unobjectionable manner. We obtain many guarantees, and we cannot always tell whether they have been reached or not. The machine may stand up to its work and do it without trouble, but it is often commercially impossible, without considerable difficulty, to check whether you have got literally what you are paying for, although practically you may feel perfectly assured that you have obtained a great deal more. Then I come to the question of reliability. The Professor sums it up in this way: "What is required, and what cannot be checked by a mere test, is reliability. This reliability in the first place is guaranteed by the strength of the mechanical construction." But I think it goes a good deal farther than that. Speaking as a buyer and user of machines, we do not want a machine cut too fine in creeping surfaces and matters of that sort. We not only want a strong shaft and plenty of bearing surface and insulation, but we want a machine that will not require microscopic and hypercritical cleaning! Some machines will run all right at the test and if you have a squad of men looking after them; but machines are called upon to run under conditions which are frequently anything but ideal. We may get a machine which would behave splendidly so long as it was looked after by skilled hands, but when it gets out into

Mr. Patchell.

Mr. Patchell.

a dusty and dirty place it causes considerable trouble. So that we want plenty of creepage surface on a machine, and that creepage surface of substantial construction, so that it cannot be easily knocked away. I remember in the old days we used to get commutators, where the commutator ring which closed up the cone at the end of it was separated from the commutator bar by perhaps one-tenth of an inch. That was beautiful as long as it was clean, but it was not practical. Then we got machines which had a thin ring of mica sticking out about an inch above the commutator bars. That was very pretty until somebody happened to lean against it, and then the insulation was gone! We want machines which will actually stand up to their work. I have seen machines in factories which will run when a can of water is poured over them, but I look on machines like that as freaks. I remember testing an electric launch before breakfast one morning. We went very close indeed to a tug with a big wash, and we came home the last quarter of a mile with the armature running under water, but the last thing I should have done would have been to call that armature waterproof! On page 61 there is a very instructive passage: "Features like this can only be guaranteed by good design based on ample experience, good selection and checking of the materials, and conscientious work in all matters connected with the tendering for and the construction and erection of the machine." That, I think, might well be taken as the motto for every manufacturer, and then people who bought machines would have a happier life. On behalf, not only of the users but of the Institution, I desire to thank the Professor most cordially for placing such an excellent paper before us.

Mr. Peck.

Mr. JOHN S. PECK: I have been much interested in reading Professor Epstein's paper. There are quite a number of points on which I agree with the author, and others on which I disagree.

On page 33 it is stated that the permeability of sheet steel is not of great importance in the construction of armatures or transformers. This is true in the majority of cases, but where the output of apparatus is limited by the extent to which certain parts can be saturated, as, for example, the teeth of certain classes of armatures and in low frequency transformers, high permeability is of great importance. For single-phase railway working, where very low frequencies may be used, it is probable that for transformer steel the permeability will be of more importance than the actual loss.

On page 35 the author refers to the "figure of loss" in the measurement of sheet steel. I think that the standard chosen, *i.e.*, 10,000 lines at 50 periods, is a good one, and that this standard should be adopted in Great Britain. It should, however, be specified that the loss is to be measured on a circuit having a sine wave-form of E.M.F., as differences in wave-form will frequently make 10 to 20 per cent. difference in the measured loss.

On page 36 the definition for ageing is given. It is defined as the percentage increase in loss after 600 hours at a temperature of 100° C.

It appears to me that 100° is too high a temperature, because there are very few machines that are operated at such a high temperature; 85° would be a more reasonable value to choose. It is a well-known fact that steel which will age at 100° may not age at 80°, or steel that will age at 80° may not age at 65°. What the manufacturer wishes to know is what guarantee for ageing he can make after his apparatus has been in service for a certain length of time. He may know that if his steel does not age at 100° it will not age at 85°, but that is not sufficiently exact. Manufacturers are being called upon for closer and closer ageing guarantees, and it is quite essential to know what the ageing will be at the normal working temperature of the apparatus. I hope that when a standard is adopted—and I think that a standard should be adopted—it will be at a lower temperature than 100° C. Mr. Peck.

On page 36 the author states that insulation resistance is of relatively little importance. I quite agree with this statement. Insulation resistance measurements have, however, one use. They serve as a rough indication of the condition of the insulation. If there is moisture present, it will be indicated by a low resistance. Some manufacturing companies always take the insulation resistance of high-voltage apparatus before making an insulation test. If the insulation resistance appears low, the apparatus is dried out before the test is made. In general it is found that where insulation resistance is low, it may be very greatly increased by proper drying.

On page 38 reference is made to the size of the electrode, and it is stated that since a large electrode covers a greater surface, it touches more weak spots, and, as a result, the breakdown voltage is reduced. I believe that the capacity and heating effect of the large electrode has more to do with the low breakdown voltage than has the fact that it covers more weak spots. There is another point that will greatly influence the breakdown voltage, and that is the weight of the electrode, or the pressure at which it is applied.

I cannot understand how the connecting of condensers in parallel to the electrode can make any material difference in the breakdown voltage. It would appear to me that the lower breakdown voltage must be accounted for by some distortion in wave-form, or by some error in the voltage measurement. I cannot understand how so small a condenser can distort the wave-form to any great extent. I should appreciate the author's views as to any explanation for the lower breakdown values. I should also like to ask how accurate temperatures are obtained on the heating of insulating tubes, as described on page 38.

The question of nitric acid is touched upon at the top of page 39. There is a good deal said at the present time on this subject, and while I think that there is a possibility that nitric acid may be formed wherever a brush discharge occurs, I do not believe that a great deal of trouble is due to this cause. I believe a more simple explanation will usually account for the trouble.

Some twelve years ago, when our general knowledge of alternate-current phenomena was somewhat vague, it was customary to charge

Mr. Peck. every mysterious breakdown to self-induction. A few years later, some one invented the term "dielectric hysteresis," and for a long time that proved to be a most successful means of explaining any insulation trouble which occurred. After dielectric hysteresis came "resonance," and even at the present time many apparently mysterious troubles are put down to this cause.

While there is no doubt that self-induction, dielectric hysteresis (or heating of the dielectric from whatever cause), and resonance have been responsible for many breakdowns, still I think that a great majority of the troubles which have been charged to these could really have been explained in some much simpler way.

So it is with nitric acid. I do not say that there is no trouble due to the formation of nitric acid, but I believe that such troubles are small, and I agree with the author when he says that if the insulation of the machine is properly designed to stand the insulation tests and the mechanical stresses to which it is subjected, there will be little danger from the formation of nitric acid.

On page 40 the author refers to high conductivity brushes. I quite agree with the general conservative view which he takes, and while we hear a great deal at the present time of brushes which may be worked up to 80 and 100 amperes per square inch, the conservative engineer is not designing his machines with any such densities. It may be possible that brushes worked at such densities will work successfully on the test-bed, but with the present limited experience in the manufacture and use of such brushes it does not seem advisable to attempt these abnormally high densities on commercial machines.

On pages 41 and 42 reference is made to mica and leatheroid. It is stated that mica is essential for the insulation of high voltage machines. I do not agree with the author on this point. Some of the largest manufacturers of high voltage machines have entirely eliminated mica from much of their high voltage apparatus. Others use a small amount of mica interleaved with fibrous material.

Regarding the very heavy solid mica tubes used by certain manufacturers, I am informed that one well-known manufacturing company experienced very serious trouble with their high voltage machines insulated with these tubes after they had been in operation for a short time, and that they had now entirely abandoned the use of solid mica tubes and were using paper and mica.

Leatheroid is a very hard strong material, but it cannot be handled readily unless it is moist. When it becomes very dry it is extremely brittle, and I should hesitate to rely upon it entirely for the insulation of any electrical apparatus which is subject to vibration.

On page 43 certain values for insulation tests are given. I do not know whether the author is in agreement with these tests, but I certainly am not. They are what are called ordinarily "low-pressure long-time tests," and I am opposed to this system of testing for two reasons. First, I think there is a possibility that the insulation may be permanently damaged without actually breaking it down; and second,

I do not think that the long-time test at low voltage is the one that shows best the suitability of the apparatus for actual service. Mr. Peck.

If we test the insulation of a machine for thirty minutes it may stand, and yet break down after thirty-one minutes. Can any one believe that if the test had been stopped at the end of thirty minutes the insulation would have been as good as it was before the test was applied?

The author refers on the same page to a test where static discharges were observed creeping over the insulation. These discharges produced carbonisation of the dielectric, and their effect was gradually increased with time, so that at the end of twenty minutes they had extended over a distance of fifteen centimetres. If the test had been stopped at the end of twenty minutes, would the insulation have been as good as it was before the test was applied? In the machine referred to, the static discharge was in such a position that it could be seen, but there is always the possibility that a discharge may take place where it cannot be seen, and if the apparatus does not break down during the test, it will be left in a weakened condition.

I have seen an insulating barrier taken from between the coils of a very high voltage transformer. On either side of the barrier was a small hole the size of a lead pencil, while the interior was carbonised and eaten away so that there was a hole in which it was possible to bury the hand. Evidently carbonisation had been going on in the interior of the barrier where the heat was greatest for a long time, until finally the whole barrier became so weakened that it was pierced by the voltage.

To my mind, the object of an insulation test is to show that the insulation of the apparatus is in such condition that it will not break down under actual service conditions. The majority of apparatus on which the insulation fails does not break down because the voltage is increased by 50 per cent. for a long time, but because it is subjected momentarily to a very high voltage, such as may occur on a transmission line when there is a lightning discharge near it, when the circuit is grounded or when switching is done.

A long-time test at low voltage is not the proper one for testing the dielectric strength of air or of oil, or of creeping distance, and when instantaneous high voltages are thrown upon electrical apparatus it is usually a discharge through air, through oil, or over the surface that breaks down the machine.

In testing the dielectric, we should endeavour to give the machine the same sort of shock that it is liable to receive in service, and this is not accomplished by putting on 50 per cent. excess voltage and keeping it on for half an hour. We should put on a higher voltage for a very much shorter time. I believe that an instantaneous test at double voltage will be more valuable as a test and more satisfactory to manufacturer and customer than a thirty-minute test at 50 per cent. above normal voltage.

Regarding tests of temperature by increase in resistance measure-

Mr. Peck. ments, I think that a method should be adopted by the Standards Committee for calculating temperatures from increase in resistance measurements. At the present time there is no standard method of making these measurements. Some manufacturers use the coefficient 0·4 per cent. and make no allowances for different room temperatures. Other manufacturers use a different coefficient and make certain allowances for room temperatures. Others reduce all temperatures to zero, and make various corrections and allowances. I hope that the method which is adopted will be a very simple one, as I believe that for all ordinary testing work, where mathematical accuracy cannot be obtained, a simple formula, though not absolutely correct, will prove more satisfactory than a complicated one which gives results to the third decimal place, when in commercial practice readings will not be taken beyond the first or second decimal place.

I agree with the statement on page 48 that temperatures should be taken after stationary values have been reached, and not at the end of a fixed period of time, unless this period be of sufficient length to permit the apparatus to reach its maximum temperature. Apparatus may be very heavy, but very poorly ventilated. After a six-hour run, such apparatus will probably not have reached anything like its maximum temperature, and a six-hour test would not be a fair one for such apparatus.

On page 49 certain curves are given of oil-insulated transformers. I have made a large number of tests of oil transformers, but I have never before seen an accurate curve which had a hump in it at the end of four hours. The temperature curve should be a perfectly smooth one. It is well known that wide differences in temperature are obtained at different oil levels, as shown in the curve, but it seems remarkable to me that the oil at the bottom of the case should remain cold for so long a time.

There is much truth in what the author says on pages 49 and 50 about operating temperatures, but I think he will have great difficulty in persuading a customer that a piece of apparatus with 60° rise will have as long a life as another with 35° rise.

On pages 51 and 52 reference is made to the testing of alternators by measuring the losses on samples of the material. I do not think any one has ever claimed that this is a mathematically exact way of measuring the losses, but with large alternators which are never erected in the manufacturer's works, but are built up in the power-house with the engine, it seems about the only way to obtain the losses. If the author has any better way to suggest, I should be glad to hear of it. It seems to me reasonable to suppose that an engineer who is calculating machines every day, and obtaining results of measurements on completed machines, should be in a position to predetermine the losses in a large machine when he knows the exact quality of the material of which the machine is made.

Mr. Rayner. Mr. E. H. RAYNER: I think a paper which deals with the various points in electrical testing is one that ought to come periodically before

an institution of this sort as progress goes on. A chain is no stronger than its weakest link, and Professor Epstein has given us the methods of finding out the weakness of each length in the completed machine. I will, therefore, simply confine my remarks to those points on which I have had some little experience, and those also on which I hope to have some experience in the future. I will first of all refer to page 38, on which Professor Epstein deals with the piercing tests, and Mr. Rayner.

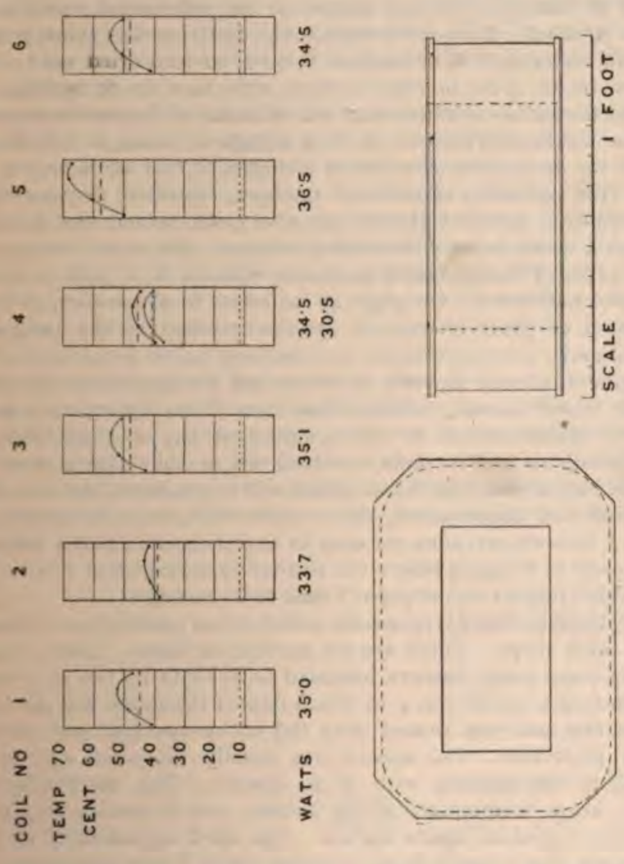


FIG. A.

on which also he says that, by increasing the size of the electrode, he gets a reduction of nearly 50 per cent. in the piercing voltage, and also by means of capacity in parallel. I should very much like to have some further details of that, because it is a matter in which I am specially interested. Has the author measured the voltage on the high-tension side of his transformer, or has he assumed the transformer ratio? That is a very important point. If he has measured the high-

Mr. Rayner. tension voltage, then this great reduction in voltage of something of the order of 30 per cent., simply by putting a condenser in parallel, is a very serious matter. Something must happen. If he measures the high-tension voltage, it must be due to the immense change of wave-form of the machine. At the bottom of page 38, Professor Epstein says he fills the tubes with rods. Are they solid rods, such as are used in induction motors, or small rods? It may make some difference to the piercing voltage. Then in Fig. 8 the author gives two samples, A and B, but he gives no reason, or no substantial reason, why sample A should differ from sample B. Is it because there is more adhesive material in it, or because it is not so well dried out? Such matters are of great interest to those who have to do testing. On page 39 the author mentions that the thickness of insulation which he uses for mechanical reasons up to a voltage of 10,000, is sufficient to prevent the production of oxides of nitrogen, so that he is on the safe side. This formation of oxide of nitrogen, therefore, is presumably produced by a certain dielectric potential gradient, and that potential gradient it would be very interesting to know. He makes no mention of the order of that potential gradient—whether it is 5,000 or 10,000 volts per millimetre. On page 43 he refers to a question which is at present of great interest to us—the question of the fatigue of material under prolonged stress at something under breakdown point. I imagine Professor Epstein to mean that the application produces damage which is really visible—that there is no mysterious internal weakness which cannot be distinguished on any machine, but that such damage as may be done by a long test is really visible, and may occur in such a form that the insulation will break down later on. Then as to field-coil temperatures, the author's work naturally follows, or rather, I believe, precedes, my own in that direction; and I take this opportunity of bringing before the Institution some further work of my own, which follows on the paper I read two years ago.

The diagram (Fig. A) represents some tests on six different field coils of the same shape. There are six varieties of finish. Coils 1, 2, 3, 4 were on sheet-metal formers insulated from them by two thicknesses of press-spahn, except No. 4, in which thinner insulation was used.

The first coil was wound with the cotton-covered wire without further protection. The second was exactly the same, only it was thoroughly impregnated with white enamel. This has produced a slightly lower temperature at the surface, and a considerably lower temperature gradient inside the coil. The third was identical with the first except that it had a layer of string round it as a mechanical protection. Coils 1, 2, 3, 5 had practically the same resistance when cold. The difference in the watts is due to difference in resistance as the coils attained different temperatures, all being run in series. They were filled with cotton waste to simulate running conditions as nearly as possible. Coil No. 4 was made by a different firm and had a lower initial resistance. On being run in series with the others 30·5 watts were used. A larger current was put through it to obtain as nearly as

possible the same wattage as No. 1. This gave the upper curve in the diagram. This coil was impregnated with brown varnish, and though the winding took more space, "bulging" considerably at the sides of the coil, it shows a flatter temperature curve inside the coil than No. 1. Mr. Rayner.

Coil No. 5 was identical with No. 1, except that no former was used. The greater temperature rise is due partly to two external layers of unvarnished tape, ½-lap; and perhaps more to the absence of the metal former which has very considerable effect in abstracting the heat from the core side of the coil and conveying it to the flanges where it is radiated away. This is confirmed by the fact that the vertex of the curve is very much nearer the inner surface of the coil than in the case of the other coils.

Coil No. 6 was identical with No. 1, except that a larger number of layers of a thinner wire were put on it. It shows a slightly higher temperature when run at a slightly lower wattage.

There is an interesting point as regards these curves. The author remarks in his paper that it is no use attempting to calculate the conductivities of a coil from a known value of cotton, copper, and so on.

If, however, we take coils 1, 3, 5, and calculate from the curves, which may be assumed to be parabolas,* and also assuming that the heat travels radially outwards, a sort of mean conductivity, k, may be obtained.

Now this k comes out in the case of the three coils mentioned, whose curves are somewhat different due to surface conditions, as 0·0058, 0·0057, 0·0059. The problem may of course be inverted, and having found the constant for a coil of certain size, we may at once find the internal temperature curve for a similar coil with different covering or other variation if we know the surface temperature and the watts expended in it.

Coil.	Finish.	Wire.	Watts.	Watts per sq. in.	Watts per sq. cm.
1	Bare	19 layers 0·058 in.	35·0	0·140	0·0216
2	White enamel... ...		33·7	0·134	0·0208
3	String		35·1	0·140	0·0217
4	Brown Varnish		34·5	0·137	0·0213
4			30·5	0·121	0·0188
5	Two layers unvarnished tape ...		36·5	0·145	0·0225
6	Bare	38 layers 0·032 in.	34·5	0·137	0·0213

* Rud. Goldschmidt. Discussion. *Journal I.E.E.*, 1905, vol. 34, pp. 711–719.

Mr. Rayner.

Coil.	Watts per cub. in.	Watts per cub. cm.	Temperature by Thermometer.	Maximum Internal Temperature.	Mean Temperature from Resistance.	Maximum Temperature − Mean Temperature.
1	0·208	0·0127	39·7	52·2	[45·0	7·2
2	0·201	0·0123	36·5	43·4	39·0	4·4
3	0·210	0·0128	35·4	54·0	48·0	6·0
4	0·205	0·0125	35·5	46·9	45·0	1·9
4	0·182	0·0111	32·5	43·2	41·0	2·1
5	0·217	0·0133	44·0	67·8	57·5	10·3
6	0·205	0·0125	39·2	54·1	45·3	8·8

Coil.	$V_0 - V_1$ Maximum Temperature − Surface Temperature.	a_1. Distance to Surface from Point of Maximum Temperature.	a_1^2	k	V_1. Temperature of Surface above Air.	h_1. Surface Heat Conductivity.
		ins.			Degs.	
1	13·5	0·87	0·76	0·0058	27·4	0·0066
2	6·2	0·92	0·85	0·0138	25·5	0·0073

dynamos round London of various types, including turbines, and I can quite confirm that. By the time a machine is stopped the surface temperature is practically the same as the inside, or rather the inside is the same as the outside. I found by means of thermo-junctions that in some cases there was a slight increase on the surface if there are ventilating ducts. The heated air comes up through the ventilating ducts, tending to heat the outside and abstract the heat from the inside on the way. On page 49 the curve with regard to the transformer is given. You will notice that Professor Epstein got tired, I suppose, after eighteen hours' testing of the transformer. I have had several to do in the same way. Not only did I use the oil temperatures, but I put about fifteen thermo-couples into each transformer, and measured the temperatures right through them. I know it is rather a tiring job. I could not sit up for more than eighteen hours, so I had to put up the load during the night, and so got the steady state by the morning. I did attempt on one occasion to do that curve quite correctly, keeping the load steady, but it is rather too heavy a piece of work. You must have two or three people to do it, because a small change of a half per cent. in the current is quite sufficient to put the temperature up or down when you get near the steady state. With regard to my own work on transformers, I did some with air and some with oil. Those without oil gave quite interesting curves, being very much hotter at the top than the bottom, as one would expect. It would by no means show the highest temperature reached in an air transformer if one took the temperature rise simply by resistance, the bottom being much colder and the top much hotter than the mean. Mr. Rayner.

Mr. S. Evershed: When I first read this interesting paper it occurred to me that perhaps there was hardly anything in it which an instrument maker ought to be allowed to discuss, because I noticed that the title was "The Testing of Electric Machinery and of Materials for its Construction." I do not know whether instruments can properly be called machinery; but, in the absence of the manufacturers of heavy machinery, perhaps I may be allowed to put a few points before you, particularly with regard to the testing of materials so far as it affects the manufacture of instruments. I confess to envying Professor Epstein rather the work which he has undertaken. Had he been an instrument maker his paper would possibly have been five times the length, because the number of tests which an instrument maker ought to carry out, in order to manufacture good electrical instruments, is something like five or ten times the number which are required in making heavy plant. In Fig. 1 of the paper I notice, for example, some curves giving the errors in the diameter of copper wires. Those departures from standard sizes may amount to two or three mils. The instrument maker has to work with wires and other things which have dimensions measured in mils. or even fractions of a mil. What is an almost inappreciable error to the dynamo maker is the whole dimension to an instrument maker. Moreover, when he has measured those wires he has not done half his tests. He has to find out whether the wire is Mr. Evershed.

Mr. Evershed.

magnetic, and when he has found out whether it is magnetic or not—and it usually is so—he has to puzzle his brains to find out how to make wire which is non-magnetic. I need not say the wire manufacturers will not do it for him. The wire becomes magnetic in being drawn through steel plates, and it must afterwards be made non-magnetic, and at present no satisfactory method has been discovered for doing that. Again, when a dynamo maker starts to make a dynamo, he has a shaft to turn of a considerable size, and consequently he can put a heavy cut on it when it is in the lathe. He can easily put on a heavy cut, and still get the diameter correct within a few thousandths of an inch. But an instrument maker has to turn an axle of the minutest size, and it has to be equally correct in its percentage accuracy. Think of the difficulty of doing that with an axle perhaps one-eighth of an inch in diameter. It is not strong enough to stand up to the tool when you are turning it. You approach the best-known tool makers in England, or in the world, and they immediately guarantee to supply a lathe which will turn your axle within one-thousandth of an inch, or less. You give the order, and they promise to deliver the lathe next week. They take a year, and then they write and say they cannot do it. At the end of that time, if you are an instrument maker, you have to find out how to do it for yourself. In short, we get into difficulties from the extreme smallness of the things we are making. Take, for instance, insulation; we get into unimaginable difficulties owing to the extremely small space there is inside an instrument for insulating against high voltage. That would not matter if we were left a free hand to design instruments and make them as big as we might desire, but the consulting engineer generally tells the instrument maker what size an instrument is to be. Take a small dynamo, or a little motor for driving a sewing machine or something of that sort; just think of the monstrous disproportion there is between the field coils and the work the armature is called upon to do. I am sometimes staggered when I work out the ampere-turns required to excite such a machine, and the weight of wire you have to put on it. All those difficulties vanish directly you make large dynamos, and I only wish I were in the enviable position of Professor Epstein. Now let me come to some points in the paper which do naturally appeal to instrument makers. The first relates to one of the most surprising things in the whole paper. The author gives us for the first time some tests of a new iron alloy, which has just about half the losses in hysteresis and eddy currents of any iron which has ever been produced before. I think that is a sufficiently remarkable discovery. Where it was discovered I do not know.* I do know that Messrs. Sankey, of Bilston, some time ago introduced an alloy of a very similar kind under the name of Stalloy. The two alloys, that which Professor Epstein mentions in his paper and that introduced by Messrs. Sankey, are so very similar that one cannot help suspecting that possibly they are

* My ignorance has been enlightened since making these remarks, but the hysteresis is still inextricably entangled with the eddy currents!—S.E.

Mr. Evershed.

identical. I have plotted Messrs. Sankey's curves on the top of Professor Epstein's curves, and they go very nearly one on top of the other. I notice that the two alloys have another property in common, and that is that no one appears to have separated the hysteresis loss from the eddy-current loss, and hence any one who starts the design of a dynamo or transformer with these curves before him, finds that he is completely stumped at the very outset, because unless he chooses to work at a density of 10,000 and a frequency of 50 he cannot tell what the iron will do. I should like to ask Professor Epstein to add a curve connecting the hysteresis loss with density of induction. Then, if we knew the specific resistance of the material, we should be able to estimate the eddy-current losses, and thus get the total loss at any density and any frequency. It is fairly obvious from the curves that the greater part of the improvement brought about in this new alloy is due to the increased specific resistance, which diminishes the eddy currents.

In Fig. 8 the author gives particulars of some tests on mica tubes which have been referred to by other speakers. I only draw attention to the point because there is a little indefiniteness as to the maximum voltage at which the tests were carried out. The paper states that there was a condenser in circuit and so on, and one infers that the tests were done with alternating current. But if they were, one would like to know whether the figures given refer to the maximum or to the effective pressure? I quite agree with what the author says on page 41 about the testing of the insulation. Mr. Peck referred to this subject, and I am glad to find myself in agreement with him also. It does not matter very much how many megohms there are in the insulation, but it does matter very much indeed whether those megohms are going to remain constant or not. That is the reason for testing insulation. If you find that the insulation resistance keeps constant, you may be certain the insulating material is trustworthy. If you find that, from day to day, the insulation resistance falls, you may be perfectly certain that you will have a breakdown if you do not do something to stop it. As to the detection of moisture which Mr. Peck referred to, it is quite true that in most insulations the breakdown occurs through moisture getting in, and it is for that reason I have devoted a great many years of my life to making an instrument which detects moisture in insulators without the slightest risk of breaking them down. One word as regards the risk of damage to insulation by subjecting it to much higher tests than the working voltage. I think the analogy with the tests made in engineering practice, the tests on bridges for example, is rather a false one. When a railway bridge has been built, half a dozen locomotives are put on it for the purpose of seeing how much it deflects. The deflection and the permanent set having been noted, one is able to state positively that the bridge will stand the working load without any risk of a permanent alteration in the structure. But do we know that definitely with reference to any insulator? I do not think it can be said we do. It appears to me that, when you

Mr. Evershed. subject an insulator, which is probably of the nature of an electrolyte, to a high pressure, then you may do some permanent injury to it; you may partially decompose it—I do not mean decomposition due to brush discharges, but some sort of electrolytic decomposition may be started. The longer you maintain the high pressure the more damage will be done. I do not say positively it is so, but it seems to me it would be very rash in the present state of our knowledge to assert it is not so, and that extra high pressure tests do no harm.

Mr. Fynn. Mr. V. A. FYNN: I think Professor Epstein has given us a very good insight into part of the difficulties which face the designer. Naturally his paper only refers to one part as its title implies. I think we ought to do all we can to help the designer; and this could be done to a greater extent than it is in the specifications prepared by those who use machinery or who advise the purchasers of machinery. In perusing this paper, one comes to the conclusion that the designer can hardly hope to be successful in every case unless he has absolute knowledge of the conditions under which the particular machine is going to work. Those conditions should, I think, be included more freely in the specifications which are issued and would advantageously take the place of a good deal of irrelevant matter still to be found in many of them. I should like to ask Professor Epstein whether he has had any experience with the apparatus devised by Dr. Drysdale for testing steel castings. As far as I can make out, it is a very convenient method, and perhaps superior to the yoke method which Professor Epstein mentions. As regards the method of ascertaining the efficiency of dynamos, the method proposed by Professor Epstein is one of the oldest known, and, I should think, will do very well in the case of a good machine; but from the point of view of either the consulting engineer or the consumer, it is not of so much importance to know whether the manufacturer has succeeded in calculating his iron losses, or guessing at them pretty correctly, but it is the over-all efficiency which is important. Probably the greater majority of firms nowadays make a very reliable machine, but we ought to have a method of some kind by which the losses in a bad machine can always be discovered. The method advocated by the author would not include a good many losses which would occur in bad machines; for instance, machines which commutate badly, although they may not spark at first. For that reason, I think, whenever possible, one ought to make an over-all efficiency test. It would, I think, be very difficult to improve on the German regulations for temperature rise or the methods of ascertaining it. I know of a great many excellent machines which have been running for years with a total temperature of between 90° and 100° C., and they are now as reliable as ever. Thanks to the courtesy of Mr. C. E. L. Brown, I can quote the case of two 100-B.H.P. 2-phase 2,000-volts 500-revs. induction motors which I put down for him in 1894. These machines are in daily operation, and their total temperature (as ascertained by a thermometer!) varies between about 100° and 110° C. These machines have never given the slightest trouble to this

day although they have been moved. This shows that the German temperature limits may even be exceeded when the machine is otherwise well designed. I would not, however, advocate this; I think that the German regulations are most reasonable in every way. Mr. Fynn.

Mr. A. CAMPBELL: On pages 33–35 Professor Epstein describes the wattmeter method of measuring the "figure of loss" in iron sheets, which is much used in Germany and to some extent also in this country. It is well to remember that when this method is used for purposes of accuracy the following difficulty arises. The measurements are made at a definite value of the flux-density B (*e.g.*, 10,000), and this must be the *maximum* B and not the effective or "root mean square" B for the wave-form used. The maximum B is determined by observing the voltage induced in the primary or secondary winding on the iron; but, as the voltmeter shows the effective and not the mean value of this induced voltage, we require to know also the *form factor*. To determine this an oscillograph or a synchronous commutator of some kind must be used, for we may make considerable errors by assuming the wave-form to be a sine curve. Mr. Campbell.

Mr. J. T. IRWIN: I wish to refer to a method of analysing out the various losses that occur in electrical machinery, more especially to those that occur in ordinary continuous-current machines, so that one may be able to judge accurately whether measuring efficiency by the Swinburne method will give a result which is consistent with the efficiency measured by the ordinary brake test. To do that one wants to find out how the losses are made up, whether the iron losses are relatively important, or the copper losses. There are a number of methods of analysing out the losses in a machine—for instance, the method of running from a calibrated machine, and measuring the additional losses when one couples up the machine to be tested. The great difficulty in that case is that one must be very careful completely to demagnetise the machine about to be tested before making a friction test on it, because although one might think that the armature was demagnetised completely so that there was no voltage across the brushes in the normal position, yet on twisting round the brushes into some other position one may find not a large voltage, but still a voltage which would produce a certain amount of hysteresis and eddy-current loss commensurate with the friction to be measured. To get rid of that and to form a simpler method I produced this plan (see next page). Mr. Irwin.

If one puts an absolutely constant current on the field magnets, and (taking the case of a direct-current machine) varies the voltage across the armature, plotting the back E.M.F. against the current supplied to the armature, one gets a number of points which lie on a straight line as shown on the diagram. That is well known; I have had a number of tests made to prove that that is so, and it is also well known from other tests that have been carried out. If this line is produced it gives at zero E.M.F. the current that is required to drive the armature round against the torque that is independent of the

Mr. Irwin.

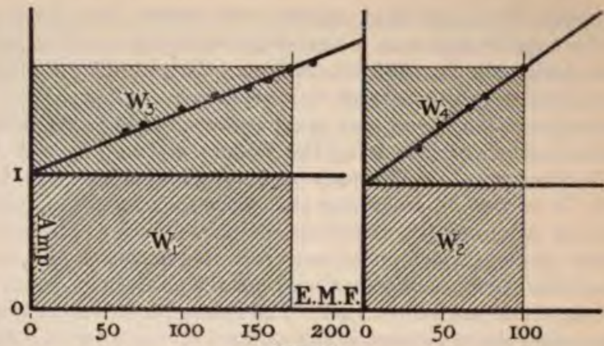


FIG. B.

speed, in other words, the friction and hysteresis torque. The increase of current is due to the losses that produce an increase of torque with increase of speed—in other words, the windage losses, the eddy-current losses, and perhaps an increase of the mechanical friction with the speed.

Suppose one takes a definite speed, S, corresponding to an E.M.F. $= V_1$, the area W_1 will represent the energy spent at that speed due to the hysteresis and friction losses. The area, which I will call W_3, will represent the energy spent in turning the armature round against the eddy-current and windage losses. To separate out the portion W_1 into two, hysteresis and friction, one puts a different exciting current on the field, making the difference as much as possible in the two cases.

One gets a second curve, as shown in the second portion of the diagram. Supposing, in the second case, the field is weaker; then, to get the same speed S, a smaller E.M.F. is required across the armature. W_2 will now represent the energy spent in hysteresis and mechanical friction. The mechanical friction at the same speed will be the same in both cases. Therefore one gets—

$$W_1 = \underset{\text{Mechanical Friction.}}{K_1 S} + \underset{\text{Hysteresis.}}{K_2 S V_1^{1 \cdot 6}}$$

and—

$$W_2 = K_1 S + K_2 S V_2^{1 \cdot 6},$$

since the hysteresis loss varies directly as the speed and as the 1·6 power of the induction, and the induction is proportional to the E.M.F. at a definite speed. Subtracting, one gets rid of $K_1 S$, and can get at once K_2, and can solve for K_1 in the equations. The value of K_1, multiplied by the speed, gives the mechanical friction at any speed.

Similarly it is possible to separate out the eddy-current losses.

$$W_3 = \underset{\text{Windage.}}{K_3 S^2} + \underset{\text{Eddy Current Loss.}}{K_4 S^2 V_1^2}$$

$$W_4 = K_3 S^2 + K_4 S^2 V_2^2.$$

One can easily get rid of the windage term by subtracting and getting an equation for K_4, and can therefore get the value of K_3. That gives all the constants which will determine the losses in the machine in a very simple manner. K_3 may not be due to windage alone, but may depend on increase of the mechanical friction with the speed. The brush friction may be separated from the bearing friction by raising a number of the brushes and observing the decrease in driving current. Mr. Irwin.

Dr. GISBERT KAPP (*communicated*): Last September I had the advantage of making some tests with the author's apparatus (Fig. 4) in his laboratory at Frankfurt, and found for "alloyed iron" values for the figure of loss, which agree fairly well with the dotted curve in Fig. 6. I then ordered a quantity of this material from the same maker, and a sample was analysed by Professor Turner in the Metallurgical Department of the University of Birmingham. As it may interest members to know the composition I give here Professor Turner's figures:— Dr. Kapp.

Carbon	0·03 per cent.
Silicon	3·40 ,,
Sulphur	0·04 ,,
Phosphorus	0·01 ,,
Manganese	0·32 ,,
Iron (by difference)	96·20 ,,

The curves of iron losses given in Fig. 16 are not directly comparable, because some refer to the loss found in testing samples, and others (notably the author's and Hobart's curve) refer to the loss occurring in the finished machine. We all know that additional losses take place in the machine under load, and the only question is how this addition shall be estimated. Some engineers calculate the losses on the basis of the loss found in the laboratory test, and then make an addition to the calculated loss to allow for distortion of the field, unequal distribution of lines, and curving of lines; other engineers add a percentage to the laboratory figure and make then no further addition to the total calculated loss. The author seems to favour the latter method, but then he ought not to have put my curve, to which no addition has been made, into the same diagram with his curve to which the addition has been made. For the same reason Fig. 17 is also misleading, for my curve has been applied without correction. Allowing for the difference in frequency, the full curve in Fig. 6 agrees almost exactly with my curve in Fig. 16. Yet it would be wrong to use either curve without correction. The only question is as to the correcting factor. I have found that an addition of 50 per cent. to the loss as calculated from the sample is a fair average and 100 per cent. an outside figure, whereas the author's curve shows additional losses of about 300 per cent. It would be instructive to have the

Dr. Kapp. author's explanation for this enormous discrepancy between his curve in Fig. 6 and his curve in Fig. 16.

On page 48 the author recommends the thermometer for testing the temperature rise of an armature, and says that resistance tests, especially in the case of series-parallel armatures, are complicated and inaccurate. I have found the contrary. The thermometer is a very misleading instrument. On the other hand, there is no difficulty in measuring percentage increase of resistance by a potentiometric method, even in the case of series-parallel windings. A known current is sent through the armature and the voltage drop is measured. It is, of course, not permissible to measure the drop over the brushes, because the uncertainty of the contact resistance of the brush contacts would vitiate the result; neither is it permissible to leave all the brushes on, since the different brushes may have different contact-resistances, so that the current may not equally divide between the parallel armature circuits. These difficulties can, however, be eliminated in a very simple way. We need only slip paper under all the brushes except one positive and one negative brush, take out on those brush spindles one positive and one negative carbon, and set the potential points in their places on to the commutator. The drop of potential between these two points can then be measured by a millivoltmeter, or, better still, by a potentiometer. If this test be made in the same way before and after the run, the ratio between the two readings is the ratio between the armature resistances before and after the run. Since we are only concerned with a percentage increase of resistance we need not trouble about determining the absolute value of the resistance, but even this can be found for a lap-wound armature in a very simple manner. If the apparent resistance as measured by the potentiometer in the manner above described be unity, then the real resistance of the whole armature is $\frac{1}{3}$ for a 4-pole, $\frac{1}{5}$ for a 6-pole, $\frac{1}{7}$ for an 8-pole machine, and so on.

On page 57 the author recommends a millivoltmeter for measuring the slip. If such an instrument be not at hand, an ordinary pocket compass may be used. It is only necessary to hold it near one of the leads connecting the slip-rings with the starting resistance and to count the oscillations of the needle in a given time. With the usual frequency of 50 this method is easily applied up to 4 or 5 per cent. slip.

Mr. Mordey. Mr. W. M. Mordey (*communicated*): The new iron alloys referred to by the author are of very great importance. The credit of these alloys belongs to England. They were made and their properties discovered by Hadfield and Barrett. A great deal of work has been done on them during the last few years to obtain uniformity and to get the best results. These further improvements have been effected by Hadfield and Sankey, who have for some time been supplying this material under the name of "Stalloy." As I have had to do with this material in an advisory way, I may perhaps supplement the author's brief reference to it. His Fig. 6 is very striking, but still more

striking are the curves for different thicknesses. As compared with the best transformer iron the "Stalloy" curve is unexpectedly flat. It is nearly a straight line, rising very slowly. The practical importance of this is, of course, very great, as it follows that lamination need not be carried down to anything like the degree necessary with ordinary magnetic iron. One or two figures may be given to illustrate this. First, for the same thickness: at a thickness of 0·024 in., the total energy loss in the new material is less than one-half what it is in the best obtainable iron. Second, for the same loss: ordinary practice is to use iron sheets of 0·014 in. thick. The new material of 0·05 in. thick gives about the same loss as the 0·014 iron. Both illustrations are at 10,000 B, 50 ∾. Mr. Mordey.

The results given in the author's Fig. 3 for permeability are much worse than ours here, for the new material, especially in the lower part of the curve.

It is interesting that this new material should have been produced just when it is wanted for new developments of alternate-current work, such as in the construction of laminated alternate-current motors. The effect on transformer design will also be very marked, a gain of 50 per cent. or more in output resulting from its use. This is on the assumption that the magnetising loss and the maximum temperature are kept the same for a given size transformer.

I am interested to learn that our German brethren are now adopting English practice in many electrical matters. An exchange of ideas and information on this subject cannot fail to be very useful in removing some common misconceptions as to the relative position of the two countries. On the question of iron testing the author refers to the "modern and better practice" of measuring the total energy loss in iron and not merely the hysteresis. That has been the recognised method in this country for at least sixteen years, and for a similar period we have made use of the plan referred to by the author of testing iron on delivery for total loss and not waiting until it got into the machine. We went further than that sixteen years ago by fixing standard conditions for testing iron, namely, 2,500 B, 50 ∾, 0·014 in. thick, watts per pound, 0·38, and paying under a bonus and penalty arrangement above or below the fixed price according to the results of the wattmeter test for total energy loss.

The author refers more than once to the Steinmetz coefficient and the Steinmetz law of the hysteresis being $= B^{1·6}$. It may interest him to know that the total energy loss also follows a $B^{1·6}$ curve.

Mr. H. H. BROUGHTON (*communicated*): I should like to ask Professor Epstein if he has noticed in any of his experiments on insulating materials any signs of *dielectric fatigue* with age. The constant break-down of high-tension plant designed with ample factors of safety without apparently any cause leads one to believe that insulating materials are liable to become fatigued. I would suggest that an engineer with apparatus at his disposal should verify this, taking a series of specimens and heating them by applying the normal voltage; then Mr. Broughton.

Mr. Broughton.

allowing them to cool, and re-heating continually for several weeks or months, breaking down specimens each week, and noting if the dielectric strength diminishes under this treatment.

Mr. Creedy.

Mr. F. CREEDY (*communicated*): In connection with Professor Epstein's remarks on the calculation of the losses in dynamo-electric machines, I think it may be of interest to put before the Institution certain methods for the calculation of the losses which I have found useful in designing induction motors. I recently had occasion to calculate a batch of induction motors, containing nearly fifty designs, and I found it very difficult to ascertain in a simple manner the precise effect on the characteristics of the motor of a change in the general proportions, say, for instance, a change in the ratio of diameter to core length. One may ascertain $d^2 l$ by means of output coefficient formulæ, etc., but there seems to be no recognised means of proceeding further, except that of trial and error, which becomes insufferably tedious when dealing with such large masses of calculation. The simplest way out of the difficulty seemed to be to work out from the first principles of the magnetic circuit a few formulæ, expressing the iron and copper losses in terms of the fundamental dimensions of the machine, *i.e.*, the pole pitch, core length, etc. These come out in a surprisingly simple form, as is usually the case in formulæ derived from first principles.

I obtained—

$$\text{Iron losses} = 0{\cdot}44 \times 10^{-3} \times V_o\,(k_1 L_t + \gamma\,\tau_1)$$

and—

$$\text{Copper losses} = 416\,\frac{\text{K.V.A.}}{\sqrt{V_o S}}\,(\Delta + \Delta_1)a.$$

The significance of the various symbols is (all dimensions in cms.):—

K.V.A. = R.M.S. kilovoltamperes input.
V_o = max. magn. current per phase × max. volts per phase/equiv. air-gap.
L_t = tooth length.
γ = pole pitch at gap/4 × core depth.
k_1 = Tooth pitch/tooth width (mean).
τ_1 = pole pitch taken at the mean diameter of core.
Δ = R.M.S. current density in stator.
Δ_1 = R.M.S. current density in rotor.
S = synchronous speed (rev. per min.).
a = length of mean turn/perimeter of square of same area as pole.

$$= \left(0{\cdot}5\sqrt{\frac{\lambda}{\tau}} + 0{\cdot}75\sqrt{\frac{\tau}{\lambda}}\right) \text{ if } \tau = \text{pole pitch at air-gap,}$$

and λ = gross core length.

α is clearly a function of λ/τ alone, so the most convenient way to ascertain it when this ratio is given, or the converse, is to plot a curve between α and λ/τ, which may be done once and for all, and then use the curve for reference. Mr. Creedy.

The most striking feature of these formulæ is that the iron losses are absolutely independent of the core length, while the copper losses depend only on the ratio of the core length to the pole pitch. On reflection the cause of this is clear. If the core length were, say, doubled, and if we wish to retain the same magnetising volt-amperes, the flux must only be increased $\sqrt{2}$ times. The density will then be reduced $\frac{1}{\sqrt{2}}$ times, and hence if the turns per pole do not vary, the magnetising current will be reduced to $\frac{1}{\sqrt{2}}$ times its former value. But the volts will now have been raised $\sqrt{2}$ times, and hence the magnetising volt-amperes will be the same as before. If the flux had been increased by more than this ratio, the volt-amperes would have increased, if by less, they would have diminished. But if the quantity of iron is doubled, and the density reduced to $\sqrt{2}$ times its former value, the iron losses will be unaltered if we assume that they are on the average proportional to the square of the density. The reasons for this assumption are given below.

Again, the iron losses are independent of the frequency, if the magnetising volt-amperes are kept constant. If the frequency is doubled and the flux density reduced to $\frac{1}{\sqrt{2}}$ times its former value, it will easily be seen that the magnetising volt-amperes are the same as before. But the losses are not halved as they would have been in the former case, but on account of the doubled frequency they are the same as before. Thus we see that the "magnetising volt-amperes per cm. of equivalent air-gap" form a true measure of the iron losses. With the aid of these formulæ (and also an estimate of the friction losses) one can predetermine the efficiency and heating of a design of given proportions without any difficulty. For instance, suppose it is required to design a machine whose efficiency is specified at half and quarter load as well as at full-load. The magnetising current is readily predetermined from the power-factor or overload capacity which it is desired to attain. Hence we know the magnetising volt-amperes. It is easy to determine what must be the relative proportions of the variable and constant losses in order to obtain the required efficiencies. Having ascertained this, we may determine the pole pitch, core depth, etc., from the iron loss formula, bearing in mind at the same time the requirements with regard to leakage, and then the core length from the copper loss formula. Other uses of these formulæ will suggest themselves to any designer. It may, perhaps, be of interest if I give the derivation of these formulæ,

Mr. Creedy.

Derivation of the Formulæ for Iron and Copper Losses in an Induction Motor.

Besides the symbols already given, we shall require the following:—

β_g = max. gap density, β_c = max. core density.
ρ_i = "Figure of loss" of the iron per c.c. reduced to unit frequency and unit induction.
ρ_c = resistivity of copper.
R = reluctance of a magnetic circuit.
r = resistance of an electric circuit.
T = turns per pole.
ι_0 = maximum magnetising current.
e = maximum counter E.M.F.
M = maximum flux in webers (1 weber = 10^8 lines).
Δ = current density in copper.
A, a = area of a magnetic or an electric circuit.
L, l = length of same.
μ = permeability of iron (if the flux is in webers, the permeability of air may conveniently be taken as 10^{-8}).
J = full load current (R.M.S.).
P = number of poles.
f = frequency.
ε = ratio of effective area of bore to total area.

The simplest method of deducing the required formula is to take advantage of the very close analogy between the electric and magnetic circuits. For alternating fluxes this may be extended to cover Joule's law as well as Ohm's law. To make the analogy evident I set out the equations of the two in parallel columns.

Magnetic Circuit.		*Electric Circuit.*	
We assume—			
Watts lost per c.c. $= \rho_i \beta^2 f$	(1)	Watts lost per c.c. $= \rho_c \Delta^2$	(1a)
Total watts lost $=$ vol. of iron $\times \rho_i \beta^2 f$	(2)	Total watts lost $=$ vol. of Cu $\times \rho_c \Delta^2$	(2a)
$e = 2\pi f \mathrm{T M}$	(3)	$e = \iota r$	(3a)
$\iota_0 e = 2\pi f \frac{\iota_0 \mathrm{T}}{\mathrm{R}} \mathrm{R M}$.			
$\frac{\iota_0 \mathrm{T}}{\mathrm{R}} = \mathrm{M}$	(4)	$\frac{e}{r} = \iota$	(4a)
Substituting from (4) in (3)—			
$i_0 e = 2\pi f \mathrm{M}^2 \mathrm{R}$	(5)	$\iota e = \iota^2 r$	(5a)
$\mathrm{M}^2 = \beta^2 \mathrm{A}^2$	(6)	$\iota^2 = \Delta^2 a^2$	(6a)
$\mathrm{R} = \frac{\mathrm{L}}{\mu \mathrm{A}}$	(7)	$r = \frac{e}{\rho_c a}$	(7a)
Substituting—			
$\iota_0 e = 2\pi f \mu \beta^2 \times \mathrm{A L}$	(8)	$\iota e = \rho_c \Delta^2 \times a\, l$	(8a)
$\iota e = 2\pi\mu \times$ vol. of iron $\times \beta^2 f$	(9)	$\iota e = \rho_c \times$ vol. of Cu $\times \Delta^2$	(9a)
Substituting from (2) in (9)—			
$\iota e \times \frac{\rho_i}{2\pi\mu} =$ total watts lost	(10)	$\iota e =$ total watts lost	(10a)

Mr. Creedy.

It should be noted here that the above equations implicitly assume that both the magnetic circuit and the electric circuit are of constant cross section and of constant resistivity or permeability at all parts.

Let us now introduce the assumption which is characteristic of the induction motor, viz., that the whole reluctance is concentrated in an air-gap slightly augmented to take account of the reluctance of the rest of the magnetic circuit. Let us also assume that the effective area of the bore is ϵ times the total area and that $\mu = 10^{-8}$.

We now have as before—

$$\iota_o c = 2\pi f M^2 R \qquad (5)$$

and—

$$M^2 = \left(\frac{2}{\pi}\right)^2 \beta_g^2 A^2 \qquad (6b)$$

for the flux is now supposed to be sinusoidally distributed.

$$R = \frac{\delta \times 10^8}{A\epsilon} \qquad (7b)$$

Hence—

$$\iota_o c = \frac{8}{\pi} f \beta_g^2 A \frac{\delta}{\epsilon} \times 10^8.$$

But, as before—

$$\text{Losses} = \rho_i \beta_g^2 f \times A L \left(\frac{2}{\pi}\right)^2 \qquad (2)$$

Hence substituting—

$$\text{Losses} = L \frac{\epsilon \iota_o c}{\delta} \times \frac{\rho_i}{2\pi} 10^{-8} \qquad (11)$$

The effect of the slot-openings in increasing the reluctance, which is expressed by the factor ϵ, is clearly equivalent to an increase of air-gap in the ratio $1/\epsilon$. Hence we may substitute—

$$V_o = \frac{\epsilon \iota_o c}{\delta}$$

where V_o = "magn. volt-amperes per cm. of equivalent gap," as defined above.

Lastly, if the cross section of the magnetic circuit during some portion of its length is reduced to k_o times that at the air-gap it is clear that the density will be raised proportionally to $\frac{1}{k_o}$. Since the losses per cubic cm. are proportional to β^2 they will be raised proportionally to $\frac{1}{k_o^2}$. But since the volume of the iron has been reduced k_o-fold the total losses will only be raised proportionally to $\frac{1}{k_o}$.

Hence finally: If the area of any length of a magnetic circuit be k_o times the corresponding gap area the losses in that length will be—

$$\text{Losses} = \frac{L}{k_o} V_o \frac{\rho_i}{2\pi} 10^{-8}.$$

Mr. Creedy. Now, if we neglect for the present the iron losses in the rotor the magnetic circuit of the induction motor may be divided into two portions—the teeth, and the core behind them.

In the case of the teeth we have—

$$\frac{1}{k_o} = \frac{\text{tooth pitch}}{\text{tooth width}} = k_1 \text{ say.}$$

In the case of the core we have—

$$\text{core density} = \frac{\text{pole pitch}}{\text{2-core depth}} \times \text{average gap density.}$$

$$\text{bore area} = \frac{\text{core depth}}{\text{pole pitch}} \times \text{area of one pole.}$$

This may be easily seen from a diagram.

In this case, then—

$$\frac{1}{k_o} = \gamma = \left(\frac{\text{pole pitch}}{\text{2-core length}}\right)^2 \times \frac{\text{core depth}}{\text{pole pitch}} = \frac{\text{pole pitch}}{\text{4-core depth}}.$$

In order to give results agreeing with the usual assumption that the core density is uniform throughout, I assume that the length of the magnetic circuit per pole is equal to the mean pole pitch taken at the centre of the core.

Hence we get as above—

$$\text{Total iron losses} = 1{\cdot}15 \frac{\rho_i \pi}{8} V_o (k_1 L_t + \gamma \tau_c) 10^{-8}.$$

The factor 1·15 is intended to allow 15 per cent. for the rotor losses.

Now if we write $\rho_i = 240{,}000$ (it must be remembered that β is in webers per sq. cm.) we get—

$$\text{Total iron losses} = 0{\cdot}44 \times 10^{-3} V_o (k_1 L_t + \gamma \tau_c),$$

as given above.

It may be doubted whether the assumption that the losses are proportional to $\beta^2 f$ is legitimate. For instance, in some cases the losses are more nearly proportional to f^2. If this is so we need only define $V_o = \frac{\varepsilon t_o c}{\delta} f$ to escape from the difficulty.

I give a table of the losses per kg. taken from Professor Epstein's curve and those calculated from the nearest parabolic curve. It will be seen that the maximum deviation is not greater than about 15 per cent. It is useless to attempt a higher degree of accuracy when the curves given by different authors vary among themselves by 100 per cent. or more. Such an error will not cause a deviation of more than 1 per cent. in the total efficiency.

It must be remembered, moreover, what is the principal object of this formula. It is to obtain a preliminary estimate of the efficiency, whereby the effect of different methods of dimensioning may be checked.

Mr. Creedy.

After the calculation is completed and β found, we may apply a correction to the formula by multiplying the iron losses so found by the ratio between those given by, say, Professor Epstein's curve, and those given by the formula at the same flux density.

However, even if we assume—

$$\text{Iron losses} = \rho_i \beta^{2n} f^m,$$

which should surely be general enough, the formula is not very much complicated. We have now to substitute for V_e the expression—

$$V_1 = \left(\frac{i_0 e}{f \times \text{vol. of air-gap}}\right)^n f^m = f^m \times (\text{maximum magnetic energy stored in 1 c.c. of gap})^n.$$

All the constituents of this expression can be readily calculated beforehand, the length of the air-gap being settled by mechanical considerations, and its area by the Steinmetz *dl* formula or some such way. However, in my opinion it will never be necessary to depart from the simple parabolic law.

Comparison of Formula with Professor Epstein's Curve.

$\beta \times 10^{-3}$ (lines sq. cm.)	1	2	3	4	5	6	7	8	9	10
Loss by formula	0·095	0·38	0·85	1·52	2·48	3·42	4·65	6·1	7·7	9·5
Loss by curve...	0.100	0·50	1·00	1·50	2·20	3·15	4·15	5·3	6·5	8·0

Copper Losses.—From equation (2*a*) above.

$$\text{Copper losses} = \text{vol. of copper} \times \rho_c \Delta^2.$$

If the full-load current is J, then area of wire $= J/\Delta$.

$$\therefore \text{Vol. of copper} = T\,l\,\frac{J}{\Delta}.$$

$$\therefore \text{Copper loss} = J\,T\,l\,\Delta\,\rho_c.$$

It should be remarked that if T = turns per pole l must be taken, not as the length of a mean turn round one pole, but as that of a mean turn round all the poles.

If we use Hobart's rule, that the end-connections are equal in length to three times the pole pitch, we have—

$$l = P\,(2\,\lambda + 3\,\tau),$$

and consequently—

$$\text{Copper loss} = \rho_c\,J\,T\,\Delta\,P\,(2\,\lambda + 3\,\tau) \quad . \quad . \quad . \quad . \quad . \quad (12)$$

Now—

$$\frac{2\,J\,T}{\tau} = q = \text{ampere-conductors per cm. of periphery},$$

and $\tau\,P = \pi\,d$ the circumference.

Mr. Creedy.

Thus—

$$\text{Copper loss} = \frac{\pi \rho_c}{2} q \Delta d (2\lambda + 3\tau).$$

Professor Thompson has shown that ξ, the output coefficient—

$$\left(\text{defined as } \frac{\text{K.V.A.}}{\text{RPM} \times d^2 l}\right),$$

is equal to $0{\cdot}825 \times 10^{-12} k . \beta q$ (see "Dynamo-Electric Machinery," 7th edition, vol. ii. p. 709) or $c_o \beta q$ say.

Hence—

$$\text{Copper loss} = \frac{\pi \rho_c \xi}{2 c_o \beta} \Delta d (2\lambda + 3\tau) \quad . \quad . \quad . \quad . \quad . \quad (13)$$

This relation between the copper loss and the output coefficient is of interest in many ways, but it does not for the moment lead us to the result of which we are in search, viz., a formula expressing the copper loss in terms of the chief dimensions of the machine.

In order to obtain this one must insert in the equation—

$$\text{Loss} = \rho_c \text{J T} \Delta \text{P} (2\lambda + 3\tau) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

the value of T as a function of the dimensions of the motor.

From equation (3)—

$$e^2 = p^2 \text{T}^2 \text{M}^2.$$

From equation (5)—

$$\iota e = p \text{M}^2 \frac{\text{L}}{\mu \text{A} \varepsilon}.$$

Writing—

J is the current in all the phases, of course. Substituting these values we get the formula— Mr. Creedy.

$$\text{Copper loss} = \frac{\sqrt{2 \times 120} \times 1{,}000 \times \rho_c \times 4}{\sqrt{2\pi\mu}} \; \frac{\text{K.V.A.}}{\sqrt{V_o}\,S} \Delta\, a,$$

But this gives only the stator loss. If the current density is the same in the rotor as in the stator, we shall not be far out if we assume the losses are the same. If it is different, say Δ_1, then the losses will be increased in the proportion Δ_1/Δ.

Hence finally—

$$\text{Total copper loss} = 416 \frac{\text{K.V.A.}}{\sqrt{V_o}\,S} (\Delta + \Delta_1)\, a,$$

as given above.

I do not wish to lay too much stress on the exact values of the constants I have given. I have rather endeavoured to set the matter out so that each designer could use his own constants. However, those given will give fairly satisfactory results. Since working out these formulæ I have also obtained formulæ for alternating-current generators and continuous and alternating commutating machinery.

Mr. J. Goodman (*communicated*): With regard to the testing of insulating materials for disruptive strength, in my opinion the usual tests on such substances as micanite, varnished cloth, paper, etc., can at best only give comparative results; in the actual machine the insulating material has most probably to be bent into position (in the case of micanite, generally rendered pliable by heat). Would not therefore the tests be more reliable, and of more practical value to the designer, if the material to be tested had been previously mechanically strained, as is the case with the insulation in a completed machine? Mr. Goodman.

If an apparatus such as that illustrated on page 14 of Messrs. Turner & Hobart's work on Insulation be used, it is of importance to know the pressure between the electrodes; the spring might, perhaps advisedly, be adjusted so that this pressure is roughly equal to the extreme mechanical pressure to which the insulation is likely to be subjected in the machine for which it is intended. Possibly, in order to exert such a pressure as is found in practice, a better method would be to have one electrode, fitted loosely in a screwed rod, the rod working in a strong support.

It appears also for the same R.M.S. volts a flat-topped E.M.F. wave is more detrimental to insulation than a peaked wave.

The curves, for the 20-k.w. transformer, shown in Fig. 14, are interesting, though the top curve is perhaps incorrect, or if correct somewhat unique in its form.

With respect to the two bottom curves, which show no temperature rise until after seven hours' run, it would be interesting to know the distance between the winding on any one leg of the transformer and the case, *i.e.*, the space allowable for circulation of oil between the trans-

Mr. Goodman.

former and the case, as this dimension very probably determines (for the same design and load) the time for which there will be no recorded rise of temperature at the bottom. It may be of interest to mention here that "rise in temperature" curve of many transformers seems to follow, with certain definite limitations, a simple logarithmic law of the form—

$$T = a \log t - b ;$$

where T is rise in temperature—

t is time
a, b are constants.

Mr. Schultz.

Mr. G. SCHULTZ (*communicated*): Having been engaged for nearly twenty years in supplying electrical engineers with material for the construction of dynamos, etc., I have found a constant source of difficulty in the entire absence of any organised scheme of standardisation of the properties of the steel and iron used by them. This applies even at the present time, nor is there in existence a universal or generally accepted method by which determinations of electrical qualities can be made. In some cases Ewings' hysteresis meter is still referred to as an arbiter of quality, but it cannot be accepted as sufficient or reliable under modern requirements, apart from its own standards being frequently at fault.

The paper of Professor Epstein deals with this question, and shows us how much Germany is in advance in methodical treatment of such matters. I am in hopes of seeing our own National Physical Laboratory take up the question, not only in conference with electrical engineers, but with the Standardising Committee, to arrange suitable standards for the various qualities of iron and steel to be used for various purposes in electrical machines, but also to fix upon a suitable standard measuring apparatus for purposes of reference. I also consider that the National Physical Laboratory should in all cases become the recognised authority to which questions as to quality should be submitted for determination.

In relation to insulating material, I am afraid the electrical engineer goes too much by what the manufacturer tells him of his own goods, without possessing a standard means of testing the truthfulness of his claims. Professor Epstein, for instance, refers to the size of the electrode for determining the dielectric strength of insulating materials, and again to his method of testing mica tubes. Similar tests are frequently performed under very irregular conditions, and often lead to injustice in comparing various makers' products, besides being detrimental to a correct selection of materials. Therefore a need for a standard method of tests for insulation material is also imperative.

With regard to the method explained in the paper of testing mica tubes, it seems to me that such tests in themselves cannot be considered final or guard against mishaps when in the machine, for mica tubes are notoriously weak mechanically, and are easily injured when drawn

into the slots, or, if protected against abrasion by fibrous material, the efficiency of the machine is weakened by reason of the extra allowance required for space. Mr. Schultz.

To get over this difficulty, the "pertinax" insulating tubes have recently been brought to the knowledge of electrical engineers for high voltage machines, and seem to be appreciated on account of their excellent mechanical properties, being less hygroscopic than mica tubes, and having a greater creeping distance although they are entirely devoid of mica.

But my principal object in commenting on Professor Epstein's paper is to insist upon the need for "standardisation." It will help both the manufacturer and the constructor of electrical machinery. In the meantime we must be thankful for the help the paper of Professor Epstein has given us by showing up our own weaknesses.

It would be interesting to know from Professor Epstein whether the alloyed sheet iron shows better results in testing after annealing of the samples used in the test, and further, whether the ageing test for 600 hours is continuous, as I hold that in the first instance 85° would be a more suitable temperature, whilst interrupted heating would bring the iron nearer to working conditions.

Mr. A. P. M. Fleming (*communicated*): Regarding the selection of insulating materials, I agree with the author that for low tension work the thickness of material required from mechanical considerations gives ample margin of safety even if comparatively poor dielectrics such as fibrous materials are used. The disruptive strength of such materials is relatively of secondary importance compared with their non-hygroscopic and mechanical features, and the permanence of these properties under all conditions such as dampness, heating, vibration, etc., likely to occur in practice. Regarding insulating tubes for high tension windings, I do not agree that mica is indispensable for this purpose. Mr. Fleming.

In my opinion, one of the most likely sources of deterioration and ultimate breakdown of the insulation of high voltage windings is the discharge which may occur in the air space between the conductors and the inner surface of the tube.

If the electrostatic stresses which produce the discharge are merely sufficient to cause an almost imperceptible "glow," ozone may be formed, and any oxidisable material thereby rendered brittle. No danger, however, is likely to occur from the action of ozone, unless, of course, entirely unsuitable materials are used, particularly seeing that it is considered practically impossible to produce ozone at the temperature at which machines usually operate.

If the discharge is heavy enough to produce heat sparks, oxides of nitrogen will be formed, and possibly under exceptionally favourable circumstances slight traces of nitric acid. The number of failures, however, directly traceable to this cause are not sufficient to justify the possibility of the formation of nitric acid being considered a very serious source of danger.

Mr. Fleming.

If the discharge is heavy, particularly when moisture is present, a serious charring of the insulation may result, the discharge practically burning its way along the path of the most easily carbonised materials. An example of this is given by the author in the description of the 60,000-volt tests applied to an insulating tube, and while this is an extreme case, a similar action may easily take place at comparatively low voltages if the insulation is of such a nature and so proportioned that high local electrostatic stresses are produced.

The principal considerations governing the distribution of the electrostatic stress in the air-gap and consequently the amount of discharge are, the potential difference between conductors and iron, the shape of the conductors, thickness of air-gap, and thickness and nature of the insulating tube.

The electrostatic stress for each layer of insulation will vary inversely as its specific inductive capacity, so that to ensure a practically uniform potential gradient across the insulating medium (consisting of insulating tube, air-gap, and material on conductors) separating conductors from iron, the entire insulation space should be occupied with materials having the same specific inductive capacity.

Seeing that mica has a specific inductive capacity of at least four times that of air, if the insulating tube is made of this material, a very abrupt change will occur in the potential slope across the air-gap compared with that across the mica tube, and the electrostatic stress in the air-gap rendered sufficiently high as to produce a discharge such as that already referred to.

Within certain limits this stress may be reduced by using, instead of mica, a material having a lower specific inductive capacity. At the present time, however, the selection of such materials which are otherwise suitable is small. A compromise, however, may be effected by using mica and paper sandwiched, thereby reducing the total capacity of the dielectric, and this composite insulation has been found generally satisfactory for high voltage work. Apart from the effects of a discharge, there is in my opinion a serious possibility of danger due to chemical change resulting from the action of electrostatic stresses.

It is well known that some fluid dielectrics when electrically stressed tend to split up into component parts which group themselves in the order of their specific inductive capacities, and it is not improbable that this action occurs to some extent in the varnishes and cements used for insulating purposes, resulting in the formation of partially conducting media, and while such materials may be entirely satisfactory for low voltage work, it does not follow that they are chemically stable when subjected to high voltages. This chemical action would be more pronounced at high temperatures, and would depend on the voltage gradient, which by the use of suitable materials should be kept as uniform and low as possible.

For the insulation of windings up to, say, two or three thousand volts, probably little danger need be feared from discharge or chemical action, as the potential gradient is comparatively small even if mica is

Mr. Fleming.

used. For higher voltages, however, a composite tube of lower capacity is preferable.

For very high voltages, say above 10,000, while a composite tube properly proportioned may be used, a safer insulation would result from the use of a material having a very low specific inductive capacity (nearly unity), and as an additional precaution all air should be excluded from the windings in the slot portion by impregnating with a suitable chemically stable compound. As a general proposition for high voltage work where insulation space is limited, materials should be used which have a very low specific inductive capacity.

With regard to the recommended insulation tests, I do not agree that "time" tests are desirable either from the customer's or manufacturer's point of view. A test fails in its object if it in any way weakens otherwise sound insulation, also if it is not sufficiently severe to break down any inherent weakness in the insulation which under operating conditions would develop into a serious fault.

While agreeing with the author that the bulk of insulating materials do not exhibit such characteristics as fatigue or resilience met with in the physical testing of metals, it is certainly possible for insulation to be weakened by long continued electrostatic stress, resulting in carbonisation due to internal heating, charring resulting from discharges, or chemical action, and in each case a certain amount of time elapses before damage results. Considered from this point of view, a "time" test is inadvisable.

I thoroughly agree that good design and workmanship are a far better guarantee than that derived from any kind of pressure test that may be devised. Unfortunately, however, workmanship cannot always be depended on, and it is possible for a purely mechanical break to exist and for the insulation still to withstand the comparatively low voltage tests suggested, and yet under service conditions when subjected to moisture, dirt, vibration, etc., break down at normal voltage. Such a weakness will much more likely be detected under a higher test applied for a short interval than the lower test continued for a longer time.

I agree that it is not commercially practicable to insulate electrical apparatus to withstand such severe stresses as may result, say, from lightning and such disturbances. On the other hand, the insulation should be sufficient to withstand a momentary pressure rise or surge of twice normal voltage. Such a momentary rise, while having little or no effect on solid insulation owing to the time element involved, would immediately break down an air space or surface insulation insufficient for the voltage. In high voltage machines for reasons of economy the length of the projecting tubes is cut down to a minimum, and these, as pointed out by the author, may constitute the weakest portions of the insulation. In the case of a 10,000-volt machine, which is tested at 15,000 volts for half an hour, while, when clean, the insulating value of the surface may be sufficient to stand the test, an accumulation of dust, which is always likely to occur in the vicinity of high voltage con-

Mr. Fleming.

ductors, might be sufficient to reduce the value of the surface down to that of the working voltage, and in the event of a surge or possibly under normal conditions a breakdown may occur. From this point of view a higher pressure maintained for a short interval only would give a better indication of the sufficiency of the surface distances.

Professor Epstein.

Professor J. EPSTEIN (*in reply*): I propose this evening to discuss the few points only which have been raised repeatedly, and to reply later on to other questions more fully in writing.

The curves of the oil transformer seem to have been rather puzzling; the explanation, however, is much more simple than it appears, as will be seen on referring to Fig. C, showing the transformer T, in the case F, and thermometers fixed at A, B, C, D. As the transformer becomes heated the warmed oil rises to the surface, and consequently the upper thermometer D will indicate the rise in temperature first, while the lower ones will not be affected at the beginning of the test, as there is no convection of heat in a horizontal direction. It takes practically about five hours before the volume of oil at G is heated up. Immediately after this stage is reached heated oil begins to fill up the space H, which process takes another three hours, and during this time the rate of rise of temperature proceeds more rapidly, owing to the narrower space.

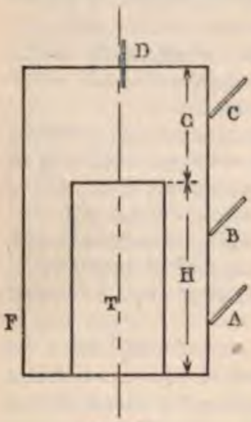


FIG. C.

As I have already pointed out, the question of the rate of temperature rise has nothing to do with the size of the apparatus—it is merely a question of the ratio of two effects, viz., ratio of cooling surface to heat capacity of the whole volume. The case of the oil transformer experiment is somewhat complicated by the motion of the oil.

As to the fatigue of insulating material, I am sorry to state that we all agree that we know nothing about it. I do not deny the possibility of causing deterioration of a machine by excessive pressure. But I never have held the view that the high-pressure test weakens a dielectric in *the same manner* as excessive mechanical stress weakens the strength of material. What I intended to point out was that the physical process seems to be a totally different one. I demonstrated the case by the example of the creeping of electricity along a tube. The weakening may be due to a carbonising effect, or it may be the result of a vapourising effect, due to insufficiently dried mica tubes or cotton-covered wire.

I was much interested in the results of Mr. Rayner's researches in connection with the subject which he brought forward some years ago. I quite agree with him as regards the importance of filling up coils with some material of high heat conductivity. Some experiments

Professor Epstein.

I have made in the same direction showed that an improvement of about 20 per cent. is effected in the transmission of heat. That means that two coils, otherwise equally constructed, measure equal external temperature, with the same watt loss, but the average temperature inside the coil filled with the heat-conducting material is 20 per cent. lower than in the other. Unfortunately, I did not succeed in obtaining constant results, as the heat-conducting materials soon altered their condition. I hope that some other material may be discovered which will preserve its conductivity, and therefore give more satisfactory results, and I look forward with great interest to further information which Mr. Rayner may be able to give us.

Several questions have been asked regarding the influence of the area of the dielectric under test. I distinguish two effects as particularly noticeable, which are caused by enlarging the area of dielectric :

1. The greater probability of detecting weak points—as can be proved mathematically—and

2. The influence due to enlarged capacity. I should like to suggest that those interested should repeat my experiments.

Besides the lowering of the piercing pressure, it will be noticed that with a condenser in parallel the sparks have quite a different appearance than without a condenser. You will observe the sparks scintillating and blue coloured. I quite agree with the different speakers that the phenomenon may be due to a change of wave-form, which, however, I could not verify at the time. I hope I may be able to do this in the near future with the aid of an oscillograph, and if time permits, I will include an illustration of the wave-form in my reply.

Communicated: Mr. Evershed has told us that if I had been an instrument maker my paper would possibly have been five times the length. I am sorry to say that, speaking for myself as a dynamo manufacturer, the length of the present paper would have been at least equal to that if it were limited by the material and not by the time available, which compelled me to confine myself to a few points, which could not be completely dealt with even then. I have to thank the speakers who assisted to complete certain points, and I am pleased to be able to answer some questions which have arisen in the discussion and to remove any misunderstandings.

I never stated "that I would be willing to put out a machine with a 90 per cent. temperature rise," nor "that 125° might be an approximate safe limit to be reached in actual working machinery." On the contrary I would recommend the German rules with 50° temperature rise as measured by thermometer, and 60° temperature rise of non-rotating coils as measured by the increase of resistance.

Mr. Skinner told us that on the other side of the Atlantic the usual limit of the rise of temperature is 40°. I do not see any reason for such a low temperature limit, and on this matter I am inclined to side with the British Engineering Standards Committee, which says in its report of July, 1904 : "It will be of interest to designers to note that

Professor Epstein.

the experimental work has made sufficient progress to indicate with considerable certainty that the temperature limits ultimately to be recommended by this sub-committee are likely to be more liberal than those laid down by either the American or German electrical standardisation committees."

The very interesting experiences Mr. Fynn reports are also a strong argument in favour of the fact that the German limits are not too high.

Though Mr. Peck agrees with my statements as regards temperature rise, he fears some difficulties with the customer, and the same feeling is expressed by Mr. Skinner, who reports, with reference to the United States, that there seems to be a tendency on the part of users of electrical machinery to force the temperature rise down to 34° or 30° C.

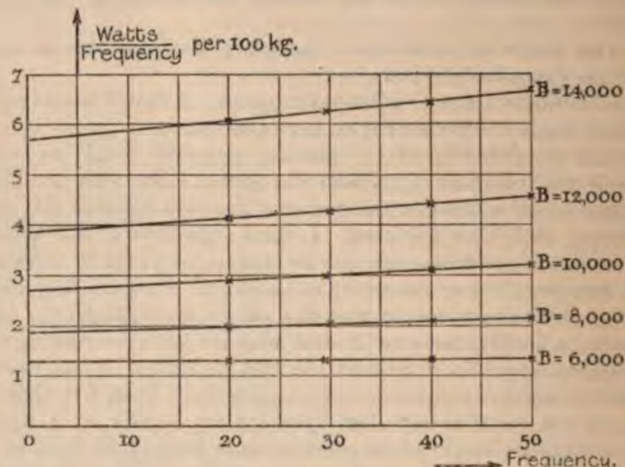


FIG. D.

I am pleased to state that during six years' working on the basis of the German regulations all such scruples have disappeared, and the more experienced the consumers become with machinery delivered to them in accordance with these regulations, the more willingly they agree to accept them.

In accordance with Mr. Peck's requirements the figure of loss in the German rules is based on measurements on a circuit having a sine wave-form of E.M.F. The temperature of 100° for ageing tests was taken from the point of view that this temperature as a round figure might exceed the highest temperature up to which iron may heat in engine rooms of 35° temperature, and so represent the most unfavourable conditions.

I am pleased to answer Mr. Evershed's request by giving the accompanying curves in Fig. D.

Professor Epstein.

As regards the difference between iron losses tested in the actual machine and in laboratory samples, I gave some explanations for the discrepancy mentioned by Professor Kapp between curve Fig. 6 and Fig. 16.

In testing the dielectric strength of a machine we are in a much worse position than the mechanical engineer when testing a bridge. To ascertain whether he is within the safe limits, he measures the ductility of the system, and can determine from well-known facts whether the behaviour of the construction corresponds with the standards. In the case of an electrical machine, we try if it will withstand a certain stress, which it ought to withstand if constructed throughout on right lines. If, instead of satisfying ourselves only as to whether the dielectric will or will not stand the test, we were to investigate the behaviour of the dielectric when subjected to increased pressure in a manner analogous to that in which the mechanical engineer measures elasticity, we should no doubt ascertain whether there exists a dielectrical condition analogous to mechanical fatiguing.

The problem is to establish rules for standardising the conditions of dielectric testing so that a bad or faulty machine can easily be recognised, and it remains for the designer to construct his machine in such a manner that it will stand the test. The tests I recommended were criticised as "low-pressure long-time tests." When double pressure was proposed by one of the speakers, his proposals coincided with the rules recommended by me up to 5,000 volts, whilst at 10,000 volts there was a difference of 20,0co as against 15,000 volts.

With reference to duration of the test, an instantaneous test was recommended in preference to a half-hour test. With this arrangement the probability of detecting failures is diminished to a great extent. If we wish to check the machine by an over-pressure test, we must give the pressure time to act. If we wish to check it by a breakdown test, we should not have to fear the breakdown, but the risk was mentioned by some speakers that the test might injure the dielectric, not seriously enough to cause breakdown during the test, but later on. No doubt there is a risk of this kind—namely, that an incipient failure caused by the test may not become visible whilst testing—but this risk is diminished the longer the duration of test. Puncturing through momentary excessive pressure without any immediate sign of such is a well-known occurrence to station engineers, who often report that they observe surging, whilst the breakdown due to punctures caused by this surging occurred later on. For that reason long-time tests are more capable of showing up a weak point during the test, and thus bring a test to a definite result.

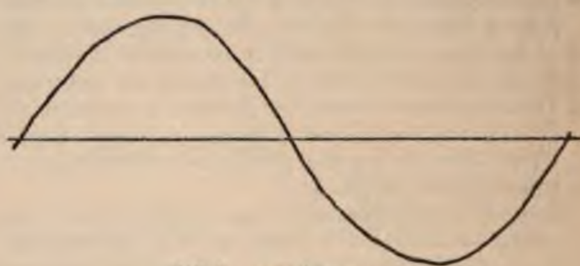
Several speakers were interested in the observation that when testing dielectric strength in the usual way, the piercing pressure was lowered as soon as a condenser was connected in parallel. I am pleased to have been able to fulfil my promise and to make some further experiments.

To simplify the conditions I tested air alternately with the con-

Professor Epstein.

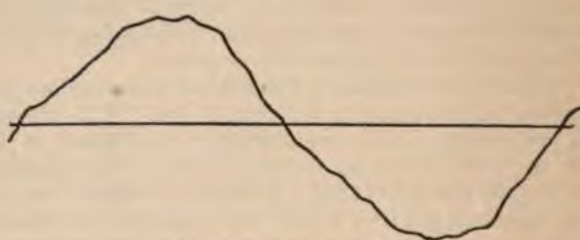
denser in parallel to the electrodes and without the condenser. When the voltage was regulated by a resistance in the primary, the average piercing pressure, as measured by a high-tension dynamometric voltmeter, was observed to be 6,700 volts with the condenser and 7,400 volts without the condenser.

The wave-forms of the pressure applied *before piercing occurred* with and without the condenser are shown in Fig. E and Fig. F. It is understood that the phenomenon is not due to the distortion of the wave-form, acting in the opposite direction, but is due to what Mr.



With condenser.

FIG. E.



Without condenser.

FIG. F.

Patchell called the "power behind it." I hope to give more details on the subject in the near future.

In answering different questions as regards testing of mica tubes (page 38), I wish to state that the voltage means effective volts.

The heating was measured by thermometers placed inside the tube.

The diameter of the iron rods with which the tubes were filled was about 6 mm.

I hope that every detail asked for in connection with the oil transformer experiment is clearly shown by diagram Fig. G. The sketch is drawn to scale. The thermometers A, B, C are fixed in the position indicated outside the cage, whilst D is immersed near the surface of the oil. The transformer used was one of somewhat unusual design and the proportion of oil volume to material was great. That is due to

Professor Epstein.

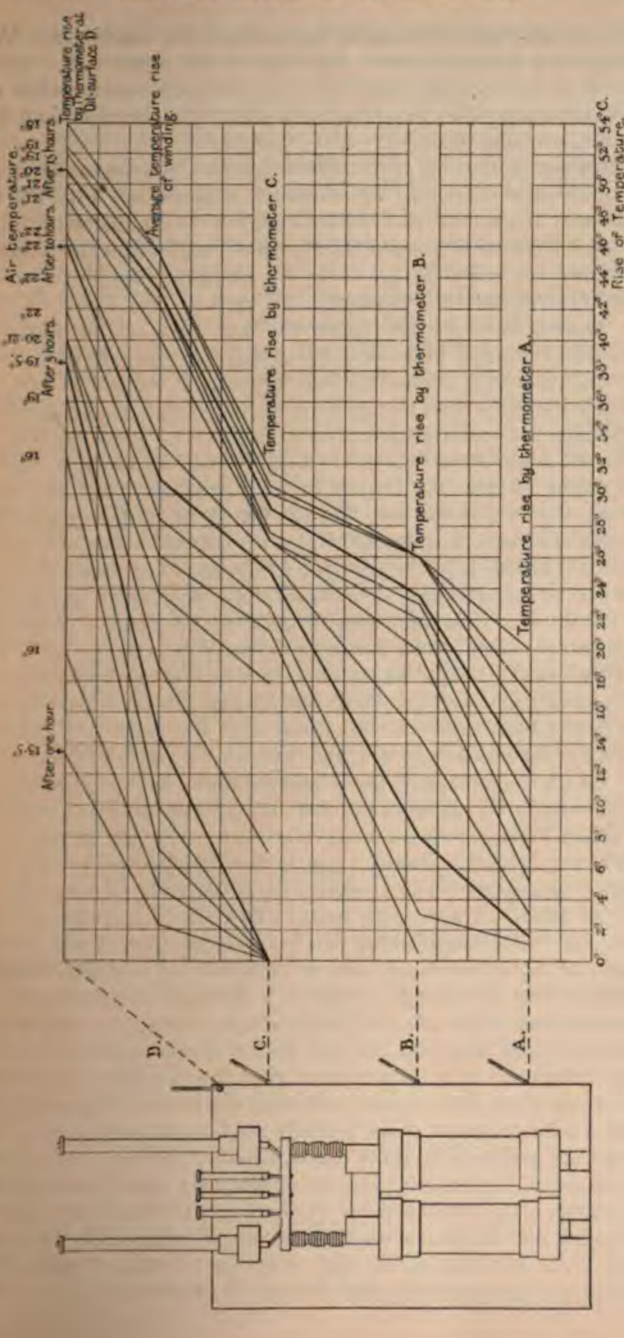


FIG. G.

Professor Epstein.

the fact that the transformer, suitable for 40,000 volts, had to work with two of the same pattern in series and one pole earthed.

The effect of the heat capacity of oil is therefore especially prominent in this instance. The temperature readings, including those of the outside air, and the progress of temperature rise in the various levels corresponding to the thermometers A, B, C, D, are shown on the right hand of the drawing. Synchronous readings are connected by straight lines; the thickened lines indicate the readings after 5, 10, and 15 hours. Thermometers A and B correspond to "bottom" and "mean" of Fig. 14, D to "oil surface," while the average temperature rise of winding corresponds to "winding" in Fig. 14.

The sudden bend on the upper curve of Fig. 14 is explained as follows: The radiating effect of the case is proportional to the product of radiating surface by temperature difference. If we had to deal with an air-cooled transformer, the cooling surface would be constant and we should get the well-known curve of temperature rise (see Fig. 12). With the oil transformer the surface of radiation increases with the volume of the oil heated. Owing to this the curvature of the line is steeper in the beginning and flattens the more as the volume and surface of the heated oil increases, but when the heated oil has replaced the cooler oil in the upper large space of the case and begins to enter into the narrower portion between vessel and transformer, the radiating surface of the case increases more rapidly and the curve of the temperature rise of the upper oil flattens simultaneously.

The President.

The PRESIDENT: It is hardly necessary for me, I think, in asking you to express your thanks to Professor Epstein for his paper, to try to sum up in any kind of way the great merits of the paper or the various reasons why you should express your thanks. I should, however, like to refer with your permission for a few moments to two points. One of them concerns the experiments that Mr. Rayner has put before us, and to which Professor Epstein has referred in such very kindly words. The great importance of those experiments arises from the fact that, although you cannot, from knowing the amount of copper and the amount of cotton in your coil, make any kind of calculation of its average heat conductivity which will be of value to you, you are able by means of experiments on one or two coils of a similar shape to those which you are going to use on a machine to calculate from experiments such as Mr. Rayner has made the average conductivity of the material; and then you can use that average conductivity, when you have calculated it, in determining what the maximum temperature will be, if you know the average, and what the rise of temperature will be in other coils of similar shape actually used on the machines. I think his results are sufficient to show that a valuable addition to the power of calculation of the designer may be attained by experiments such as he described. The whole point really turns on the work that Mr. Goldschmidt put before us some time ago, in which he first called attention to the fact that curves such as appear on page 18 of Professor Epstein's paper are parabolas, and the rest all follows from that. Then

The President.

with regard to the question of elastic fatigue, I am glad to find that Professor Epstein does not consider that point as definitely settled, and that he too welcomes further experiments on it. The questions of the proper tests to be applied to high tension apparatus have been considered very carefully by the Engineering Standards Committee for some time past, and they have decided that, before attempting to formulate what those tests should be, further experiments are required; and arrangements are now being made, thanks to the kind assistance we are receiving from a number of manufacturers, for the carrying out by Mr. Rayner in the National Physical Laboratory of a large series of tests to elucidate that very important point. As I say, it was especially interesting to me to find that Professor Epstein agreed that these experiments were necessary and desirable, and in hoping that we might from them arrive at results as important and as interesting as those already reached by Mr. Rayner in the experiments to which Professor Epstein has referred. There is one remark on page 46 to which I should like to refer. Professor Epstein here writes: "From experiments made on samples of cotton-covered wire by the National Physical Laboratory of Great Britain, 125 deg. C. was taken as an approximate safe limit." I do not think Professor Epstein meant to imply that that was an approximate safe limit that might be reached in actual working machinery. All Mr. Rayner's experiment showed was that a coil of wire wound on a proper former and left at rest could stand that temperature for a very considerable time without showing any fall in insulation, or rather that it showed on the whole an increase of insulation. But that that temperature should be a permissible one in actual working never crossed the minds of the Engineering Standards Committee, who were responsible for these measurements, and it was not, I think, Professor Epstein's intention to suggest that in anything he has written in his paper.

I have trespassed, I am afraid, too long on your time, by these remarks, but I wanted to call attention to one or two points that seemed to me of great interest and importance. I now have to ask you to express, as you have really already done, your hearty thanks to Professor Epstein for his paper.

The resolution of thanks was carried with acclamation.

Proceedings of the Four Hundred and Forty-sixth Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, December 6, 1906—Dr. R. T. GLAZEBROOK, F.R.S., President, in the chair.

The minutes of the Ordinary General Meeting held on November 22, 1906, were taken as read, and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

TRANSFERS.

From the class of Associate Members to that of Members :—

Ernest Marmaduke Sellon.

From the class of Associates to that of Members :—

[redacted]

[redacted]

Donations to the *Library* were announced as having been received since the last meeting from Messrs. C. Barus, H. Borns, G. Dettmar, Capt. E. O. Henrici, J. H. McGraw and W. D. Weaver, E. W. Marchant, P. F. Rowell, G. W. de Tunzelmann, and The Vulcan Boiler and General Insurance Company, Ltd., to whom the thanks of the meeting were duly accorded.

The discussion on Professor J. Epstein's paper was concluded (see p. 63).

The meeting adjourned at 9.35 p.m.

Proceedings of the Four Hundred and Forty-seventh Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, December 20, 1906, Dr. R. T. GLAZEBROOK, F.R.S., President, in the chair.

The minutes of the Ordinary General Meeting held on December 6, 1906, were taken as read and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

[redacted]

THE TRACK CIRCUIT AS INSTALLED ON STEAM RAILWAYS.

By H. G. BROWN, Associate Member.

(*Paper read December* 20, 1906).

There are, I venture to think, many members who are unfamiliar with the practical application of electricity to railway signalling. As the entire subject is, however, too large to bring within the limits of a single paper, I have decided to confine myself to a description of the track circuit only, as installed on steam railways, of the method of its operation, and of the laws governing its working, which are simple and definite, and have been thoroughly understood by specialists for many years.

I believe that the first track circuit used for the control of train movements was installed in 1871 by a Mr. Pope. His apparatus was, however, crude in design, and was regarded by railway men in general with some distrust, but it did not take long to convince those interested that the principle was sound and that reliability was a question of design and construction only.

Since that time rapid progress has been made, and experience has shown that the track circuit not only affords a greater degree of safety, but also that its use permits a closer headway, and therefore a greater traffic capacity, at a smaller maintenance cost than any other system. As a means of automatically informing the engine-driver of the condition of the section of line to which it gives admission, it is undoubtedly the best, because it is operated by the presence of the train itself upon the metals.

It is a fundamental principle in signalling that the failure of any part of the system should result in the signal going to danger. In designing the circuits, therefore, each scheme of connections must be carefully analysed to determine the probable effect of the earthing and short or open circuiting of every instrument and wire.

General Description.—The principle upon which the track circuit is operated differs from that governing the majority of electrical combinations.

In railway signalling, as well as other applications, the opening or closing of electrical circuits, the energising or de-energising of the apparatus employed, as a general rule gives the signals, but the condition of the track circuit is indicated, or I might say translated,

by means of the total or partial de-energisation of the translating device or relay: not by the opening or closing of the circuit, but by forming a shunt across the relay, reducing the potential drop to an approximate zero, or at least to a point below that required for its operation.

The circuit is formed by the battery (Fig. 1), track rails and relay in series. The battery is connected between the rails at the end, and the relay at the beginning of the section, the end and beginning being determined by the direction of traffic. The current flows from the battery through the entire length of the section on one rail, through the relay, and back to the battery through the other rail. The section of line included in the track circuit is isolated by means of insulated fish-plates. All rail-joints, except those separating the sections from each other, are bonded to ensure electrical continuity.

The actual resistance of the ballast and sleepers is low, especially in long track sections. It sometimes falls below that of the relay itself,

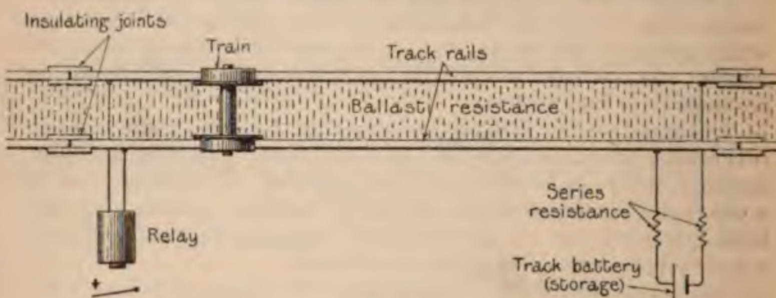


FIG. 1.

and therefore the latter obtains but a portion of the total current, the rest leaking across from rail to rail. The average resistance per 1,000 feet depends on local climatic conditions and the materials employed in the construction of the permanent way. Broken stone makes the best, and cinders the worst ballast from the signal engineer's point of view.

There are therefore two, and when the section is occupied three, resistances in parallel, namely, the relay, the ballast, and, in the latter case, the shunt formed by the train. These parallel resistances are in series with the internal resistance of the battery or its equivalent in effect. The resistance of the rails is not here considered, as in properly installed short track circuits it should be negligible.

The fact that the working of the circuit is dependent on the variation in the resistance between the rails points to the necessity of a resistance external to the track but in series with it. This series resistance plays a most important part, for if the battery had no internal resistance, or if its equivalent were omitted, the full potential

would be maintained across the relay no matter how much the resistance between the rails might be reduced by a train occupying the section.

Relay Shunts.—Effective shunting is the main question. The shunt formed by an engine and train has a resistance so low as to be without interest, but if formed by a light single vehicle the resistance is sometimes relatively high, on account of the smaller contact surface and pressure between the rails and the wheels. If the rails and wheels are rusty it is, of course, greater than when they are worn clean and bright. The fact that the shunt formed by a light vehicle may have an appreciable resistance does not mean that such vehicles are ineffective as shunts, but that a different adjustment is necessary where their presence is probable. These adjustments may be obtained by altering the electrical relations between the relay and series resistance with a given battery, also by determining the length of the track circuit from the minimum resistance of the ballast per 1,000 feet.

The resistance of the shunt formed by a vehicle when stationary is constant, but its value is dependent on the condition of the rails and wheel surfaces at the points of contact as well as on the weight of the wagon. The actual amount will therefore vary with different positions of the same vehicle in the same track circuit.

The shunt resistance of a single pair of wheels in motion varies. It is obvious that the resistance of a number of pairs of wheels is proportionately less than that of the individual units, and the variation is also less, as the minima and maxima of the units would not coincide. Rolling stock, as regards shunting effectiveness, may be approximately divided into two groups, namely, trains and engines, and single wagons, and an arbitrary resistance assumed for each class, high enough, with a factor of safety, to exceed the maximum obtained by actual tests. In the case of coaches fitted with ordinary Mansel wheels the hubs and tyres must be bonded. For instance, the London and North-Western coaches running over the District Railway have bonded Mansel wheels which give no trouble whatever.

It is possible to install track circuits which will be operated in a reliable manner by trolleys, inspectors' tricycles, etc., but this is generally considered unnecessary, and with good reason.

The sanding of rails is undoubtedly detrimental to the satisfactory operation of the circuit. But the quantity of sand that is really necessary to make the drivers grip slippery rails will cause little trouble.

The maximum resistance that will shunt a given track circuit bears a definite relation to the ballast resistance which would cause the relay to drop its armature. It also varies inversely with the variation of the ballast resistance above the failing point. If the shunt is to be effective the combined resistance of the ballast and the shunt must be equal to or less than that of the ballast resistance only, at which the track is adjusted to fail. Again, the combined resistance of the ballast,

relay, and shunt must be equal to or less than the combined resistance of relay and ballast with the section unoccupied, at the ballast resistance at which the track is adjusted to fail. The resistance of the relay, being constant, does not affect the relation between the effective shunt and the ballast resistance for a given adjustment.

The first factor to be considered is the minimum ballast resistance. The next, the maximum and the range between the two. The third, the resistance of the poorest shunt which will be effective. This latter is settled by the first two factors—no matter what the resistance or pick-up volts of the relay may be.

The Relay.—The resistance of the relay is largely a question of economy in operating power and of its adaptability to the battery arbitrarily selected. Its function is to open or close local circuits which control a signal either directly or indirectly, by working the various devices used in the interlocking of points and signals. It should be a well-made and efficient instrument, as but little power is generally available for its operation. On steam railways a relay should work with not more than 0·015 of a watt. On electric railways, on account of the presence in the track rails of extraneous currents of some magnitude, it may be advisable to maintain a higher potential between the rails than would otherwise be necessary, and to employ a less delicate instrument on account of the risk of it being subjected accidentally to the full potential of the motive power service.

There are, broadly speaking, two kinds of relays, the neutral and the polarised types. The neutral type is uninfluenced by the polarity of its field. The polarised relay has an additional armature which is polarised and operates contacts selectively in response to the polarity of the current energising its magnets. One of the many uses of the polarised relay is to operate distant signals by means of the reversal of the track circuit, thereby rendering unnecessary the line wires which with the neutral type of relay would be required between the distant or caution signal and the stop signal controlling it.

The potential required to operate the relay increases with its resistance, and although the current in the relay is decreased, the total current in the circuit and therefore the total watts are increased, as the ballast resistance does not vary with that of the relay.

The relay contacts are operated by means of the movement of an iron armature. The air-gap in the de-energised position is greater than when the relay is energised and its armature has responded. For this reason the potential required to pick up the armature is greater than that required to hold it up. The range in potential between the armature pick up and drop should be as little as possible, and is in actual practice approximately equal to 30 to 50 per cent. of the pick up volts.

It is important that the armature bearings should be practically frictionless and the contacts constructed so that there is no risk of their fusing together. The armature itself should be prevented from actually touching the pole pieces on account of the possibility of the

presence of residual magnetism. Any neglect of these points might result in keeping the armature in the energised position at the wrong time with possibly disastrous consequences.

Front contacts are known as those which are closed when the relay is energised and are platinum to carbon, the movable members being platinum and the stationary ones carbon. The immunity afforded by this combination from disturbance by lightning discharges and heavy currents from other sources fully warrants its use, although the contact resistance is a little higher than might be wished.

Bottom or back contacts are closed when the relay is de-energised, and are therefore generally platinum to platinum, as there is less contact pressure and the circuits they control are relatively unimportant.

The Battery.—A battery having a constant potential and internal resistance should be used for this class of work. The Daniell cell, commonly called the gravity battery, was at first almost universally employed. It has, however, one unfortunate characteristic, namely, an extremely variable internal resistance. The E.M.F. of this battery being approximately 1·1 volts, and its internal resistance being comparatively high, a supplementary series resistance is not required. In fact, it is generally found necessary to decrease the internal resistance by connecting two standard 6×8 inch cells in multiple, or by constructing special cells which will give an equivalent result. A definite ballast failing point adjustment is difficult to obtain with this type of cell unless it receives exceptionally careful and frequent attention.

On the other hand the storage battery has much to recommend it. Its internal resistance being low, it is necessary to use an additional external resistance in series with it, the value of which may be altered to suit local conditions as accurately as desired. Its potential difference is fairly constant and need not vary more than 10 per cent. In many cases it is more economical from the maintenance point of view, although its first cost is greater. As its electrolyte has a much lower freezing point than the zinc and copper sulphate solutions in the gravity battery, a deep battery well is unnecessary for protection from cold.

Bonding.—To ensure the electrical continuity of the running rails, it is necessary to bond the rail joints. Two bonds should be used at each joint on account of their liability to break, and also in order to reduce the total bond resistance. No. 8 S.W.G. galvanised iron wire may be used under ordinary conditions. Copper is preferable when a greater current-carrying capacity is desired than that afforded by the iron wire, or where sulphurous fumes are prevalent. It is also good practice to use copper in tunnels, and where the 6-ft. and 4-ft. ways are timbered flush with the rails.

Two kinds of signal bonds are in general use, the "rivet" and the "taper sleeve" galvanised iron wire patterns. The rivet pattern was the usual form employed, but lately the taper sleeve has come into greater favour. The former consists of two galvanised iron rivets turned down for a part of their length, leaving a shoulder under the

head of the original diameter, round which the galvanised iron wire is turned twice, and the whole is then dipped in solder. The holes in the rails are drilled slightly smaller than the machined part of the rivet, so that when driven home the rivet is shaved by the edge of the hole, ensuring a tight fit and clean surface.

In the taper sleeve type of bond the wire is passed through a split taper sleeve, which is then driven into the hole in the rail. The bonds should be long enough for a complete circular turn to be made in each end, to allow for the usual movement at the joint without bringing a strain on the rivets or sleeves, and they should never be fixed to the sleepers or chairs in any way.

Insulating Joints.—Various types of insulating joints are in common use, such as all steel, steel and wood, and all wood joints. I am afraid I have an old-fashioned prejudice in favour of the oak joint, for flat-bottom rails at least, and I see no reason why this joint should not be adapted for use with the bull-headed rail used in this country, provided that it is properly supported.

The wooden joint consists of two heavy oak blocks milled to fit the rail, and are bolted to the rails, one on each side; that is, the joint is fished with wooden blocks instead of the usual iron plates. The rail ends are separated by one or two pieces of ¼-inch fibre of the same shape as the rail section. Some of the types of insulated joints in use fall far short of even a practical ideal.

General Adjustment.—In considering the adjustment of a track circuit, I will assume that it should be adjusted to shunt, as well as to give a clear indication under the worst ballast conditions. This would seem to be an unnecessary statement, but I make it advisedly, as many track circuits are installed and adjusted to give a clear indication when the track is unoccupied, but if the conditions are such that they shunt indifferently, track circuiting as a principle is apt to be criticised.

The rolling stock, as regards its shunting effect, has previously been classified into two groups, the first being engines and trains, the second single vehicles. This would imply two classes of track circuits, viz.: (1) Main line; (2) sidings, station bays, junctions for fouling, or for clearing or for point locking. Fortunately the circuits in the second class would be shorter than those in the first.

The phrase "Ballast resistance at which the track is adjusted to fail" may here be explained as follows:—

If the ballast resistance falls below the assumed minimum, it is evident that either extraordinary weather conditions exist or that there is some fault which forms a connection between the rails, supposing, of course, that the section is unoccupied.

The minimum ballast resistance is important only as a point at which the track potential must be sufficient to cause the relay to pick up its armature, thereby insuring the continued working of the circuit. By virtue of the series resistance there is a resistance below the assumed minimum of the ballast at which the relay will fail to pick up its armature on account of the attendant decrease of the track

potential. The circuit may be adjusted so that this minimum bears a safe and definite relation to the minimum ballast resistance, and one might say that this is the point at which the track is adjusted to fail.

This is important because it determines for all conditions of ballast resistance the maximum effective shunt, that is, the shunt resistance which in parallel with the existing ballast resistance will bring the track potential down to the failing point of the relay.

The relay has two failing points: the armature pick up and the armature drop. The adjustment must be such as to allow the armature

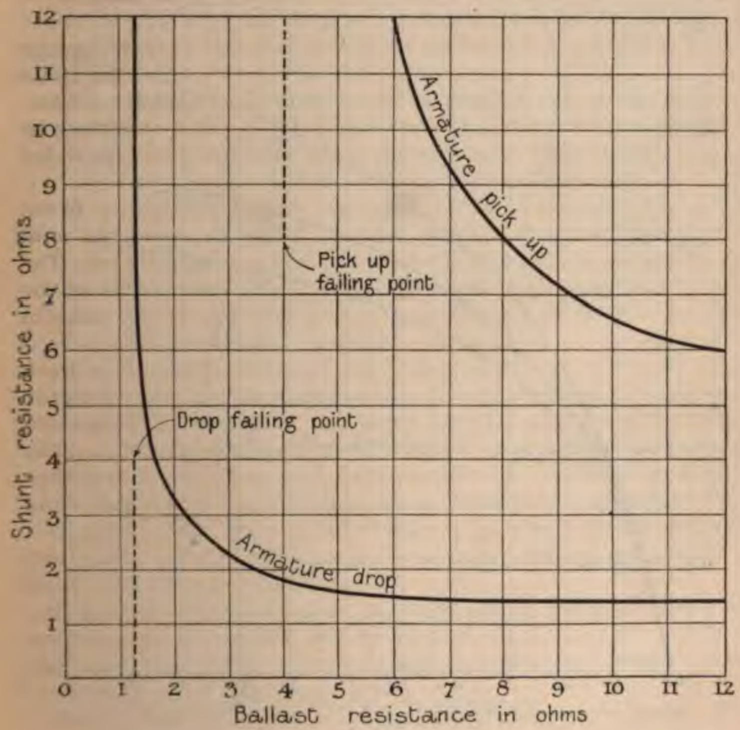


FIG. 2.

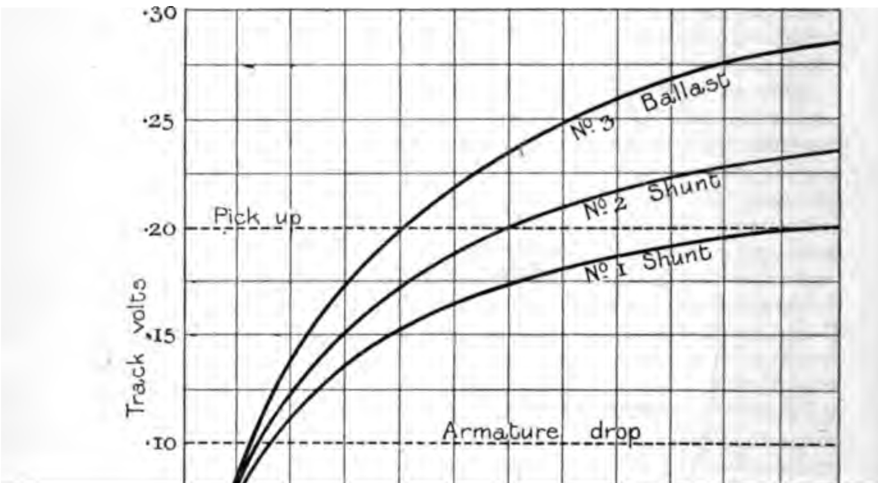
to pick up with a margin at the minimum ballast resistance. A shunt to be effective must cause the armature to drop. It is evident that a shunt of lower resistance is required to drop the armature than is necessary to prevent it being picked up. In Fig. 2, the top curve represents the shunt which will prevent the pick up, and the bottom that which will cause the drop. The ordinates represent shunt resistance, and the abscissæ the resistance of the ballast.

The diagram is based on the following figures: A ballast minimum of 6 ohms and a relay having an armature drop point equal to one half

the pick up potential. The track is adjusted so that the pick up failing point will be reached if the ballast resistance should drop to 4 ohms.

The relay has a resistance of 4 ohms, a pick up point of 0·2 volt, and a drop point of 0·1 volt. With a battery potential of 2 volts, a series resistance, including the internal resistance of the battery, of 18 ohms would be required.

The ends of these curves extend indefinitely in both directions, for at the failing point the effective shunt would be infinite, and with an



[redacted]

I want to make one point very clear, namely, that if a track is adjusted to give a pick up failing point at a certain ballast resistance or its equivalent, the maximum effective shunt at any ballast resistance is not altered by the resistance or pick up volts of the relay used. If a track is adjusted to give a pick up failing point at a certain ballast resistance with a 4 ohm relay, and an 8 ohm relay is used, the effective shunt will be greater at all ballast resistances; not through any virtue of the relay, but simply because, by its use without altering the series resistance, the pick up failing point is reached at a higher ballast resistance.

With a pick up adjusted for a given ballast resistance, the top curve would be the same for all relays. The position of the bottom or armature drop curve and its relation to the top curve will vary with the adjustment of the relay armature.

This armature adjustment may be obtained by decreasing the percentage variation of the armature air-gap, either by lessening the movement of the armature when adjusted near the pole pieces, or by keeping the original amount of movement, and increasing the minimum air-gap. The adjustment is limited in the first instance by the necessity of maintaining a sufficient space between contacts when open, and in the second case by the watts required on account of the total increase in the air-gap. With the type of relay in general use, the armature drop volts, as previously mentioned, should be from 30 to 50 per cent. below the pick up volts.

Relays should be adjusted and sealed by the manufacturer. This adjustment gives the best average results which can be obtained, and it should not be altered by the buyer unless he is sure of what he is doing. The minimum ballast resistance is to a certain extent under control, as it is approximately determined by the length of the circuit, but the relation of the minimum to the maximum depends on local conditions. Take the 6 ohm minimum in Fig. 2 as an instance. This equals a 333-ft. circuit at 2 ohms per 1,000 feet, or a 2,000-ft. circuit at 12 ohms per 1,000 feet.

The shunt is the most important factor, and we will suppose that for a certain class of traffic a definite figure is decided upon. There is more than one way of adjustment, but the following method is productive of the most satisfactory results.

A relay and battery to be universally employed are chosen. The length of the circuit should be roughly determined by its minimum ballast-resistance per 1,000 feet, and the adjustment obtained by altering the amount of the series resistance.

If the series resistance is non-adjustable, a relay should be chosen with its pick-up and drop volts bearing the correct relation to the series resistance and battery E.M.F. This, however, is a much less flexible method of adjustment.

In both methods the relay drop failing point should occur with a ballast resistance as close as possible to that of the minimum ballast resistance. Sufficient allowance should, of course, be made for the pick up and the margin between the pick up and minimum ballast

resistance. The virtues of a track having a 4 or 5 ohm minimum are largely lost if the relay drop occurs at a ballast resistance lower than is necessary.

The relations between the drop and pick up failing point are fixed, whereas the margin referred to is a factor within control.

It should be sufficient to make allowances for a ballast resistance reasonably below the anticipated minimum, but not great enough to insure the circuit working under conditions caused by the existence of a fault. The whole question of the value of the margin required to insure uninterrupted operation is one of degree.

If an exceptionally low resistance between rails exists but seldom, such an occurrence can be justly classified as a fault rather than considered as an average minimum, thereby greatly increasing the efficiency of the circuit from the standpoint of effective shunting.

Where the conditions of traffic require a long block section and the nature of the track is such that it is undesirable to have a long track circuit, the section may be cut into two or more circuits, which collectively operate the one signal.

The measurement of resistances by substitution is often advantageous. The simplest manner of ascertaining the resistance required to shunt a track is to connect an adjustable resistance from rail to rail and gradually decrease its value until the relay armature drops. But in making tests with measuring instruments it must be kept in mind that the results obtained may need correction by calculation, because the conditions may be changed by the use of the instruments themselves, with the result that values are obtained different from those which existed before their introduction.

* * * * *

In conclusion, it will be noticed that the use of mathematical equations is avoided in the paper. I have endeavoured instead to explain the principles of the track circuit in such a way that they will be logically and graphically evident. It has been necessary for the sake of clearness to speak of certain factors as having definite dimensions, when in practice they will vary from hour to hour and from year to year. For the same reason emphasis and prominence have been given to certain conditions which have always to be borne in mind, though in practice they occur but seldom.

In the case of shunting, for instance, the resistance of the shunt of anything from a reasonably heavy brake van to an entire train on a clean track is so low that a Weston instrument with a 1·5-volt scale connected across the relay would not show any movement of the needle from zero. It becomes a serious question only when the rails are in a bad condition and the vehicles light in weight. If a track circuit does not work efficiently, its arrangement, and not track circuiting as a principle, is open to question.

DISCUSSION.

Mr. H. M. SAYERS: I desire to make one or two remarks for the purpose of obtaining information. I do not understand the small amount of energy available for the relay. The potential between the rails is stated by Mr. Brown to be from 2 to 5 volts, 3 volts usually. With from 3 to 5 volts available between the rails, 2 or 3 watts, at any rate, should be available for the relay coils; and since the relay is not like a telegraph relay, in which the time constant is important, but may have a relatively large mass and therefore be wound with a large number of turns of comparatively large wire, there appears to be no particular reason why such a construction should not be used, and more certain action would seem to be secured. The difference between the "drop failing point" and the "pick-up failing point" Mr. Brown tells us has been reduced to nil. The telegraphic practice of using a relay in which the moving part does not make any material difference to the reluctance of the magnetic circuit seems to be the right way to get over the difference mentioned in the paper. Probably a moving coil relay would be a good device for the purpose. To get the greatest sensitiveness and to fulfil the conditions that are required in these particular circumstances, a polarised relay is desirable. I should like to ask if Mr. Brown has noticed any critical point in the voltage to be maintained between the rails. Claude, in an interesting paper read before the International Society of Electricians in 1900, dealing with the question of electrolysis due to stray tramway currents, cited a large number of experiments which showed that, for low voltages, earth resistance between rails and buried pipes acted as a metallic resistance—that is, it appeared to have a definite resistance, and the current through it varied directly with the applied potential up to a certain point. Beyond that point there was an electrolytic or polarising resistance. Consequently it would seem that, if the potential between the rails is maintained a little above the polarising point, it will be much steadier under differing conditions of weather and "ballast conductivity" than if it is below that point. That point will probably be about 2 volts, or perhaps a little higher. Mr. Brown may know something about this.

Mr. W. J. THORROWGOOD: I think some objection might be taken to the term "ballast resistance" as used in the paper. In the track circuit the current passes from one terminal of the battery through one rail to the relay, and back through the other rail to the battery. Any current passing from rail to rail without going through the relay is in the nature of a leak, and in general electrical language the resistance of that leak path is known as the insulation resistance. In the literature of the subject we are discussing, it is called the track circuit insulation resistance. It seems to me that is a better way of regarding it, seeing that we are new to the subject. Then, again, the term "ballast resistance" is not generally applicable, seeing that, in some electric railways at any rate, there is no ballast. On steam roads, where the track circuit Mr. Thorrowgood

Mr. Thorrowgood.

is used, the ballast is cleared away from the top of the sleepers, and consequently you might speak of the "sleeper resistance." In any case, in these days of standardisation I think we should keep to the same term for the same thing used in different cases. With reference to using the difference of potential, it has been the practice hitherto to speak of the number of milliamperes which will actuate the relay. From the theoretical point of view it may be well to use the difference of potential when making your calculations to set out your system, but I think it will be found in practice easier to adjust your relay by means of the current passing through it rather than by the difference of potential on its terminals. With reference to the difference of potential of the storage battery, although the storage battery difference of potential may be allowed to vary 10 per cent., there is a minimum difference of potential which must be decided upon, below which your variations must not take place. For instance, you may go from 2 to 2·2, but not below 2. Coming to the rail resistance, as the paper says, it should be negligible. So it may be for 330 ft., but when you come to 2,000 ft., the resistance of the rails themselves is six times as large. Taking the resistance of a length of rail as 0·18 ohms—and that is about right for 1,000 ft.—the current received by the relay is about 1¼ per cent. less than if you left the rail resistance out of calculation altogether. That may be important if you are working near the margin, and in the figures given in the paper there are only 8 milliamperes in excess of the 50 necessary to actuate it. Mention is made in the paper of possible interferences of the track circuit by extraneous currents on electric railways where they take a certain amount of care to avoid them. I think the track circuits on steam roads are just as likely to be affected by these extraneous currents as are electric railways, especially if the track happens to be in proximity to an electric tramway. You have always the liability, at any rate, of having a difference of potential of 7 volts on the extreme ends of your electric tramway. I think the power, or the quantity, of these stray currents is rather more than is sometimes thought or expected. In one case a little while ago I found that from a rail to a point twenty yards distant there was a current of 95 milliamperes passing, and in other cases I have found a difference of potential of half a volt in a distance of a mile. In cases that have come under my notice where instruments have been affected, the difference of potential must have been 2 volts at the very least, and probably 4. It appears to me that this question of the interference of tramway currents is rather important. In London it is well known to telegraph people that there are many so-called earth currents flying about. With the advent of electric tramways and railways, the necessity may easily arise of having to insulate the track more carefully, or it may have to be insulated altogether if we are to avoid interference with the track circuit and the signalling. Alternating currents may be used in some districts to overcome the trouble. It may be a remedy, but the real remedy, it seems to me, rests with the tramway companies to provide efficient copper returns, so that their stray currents shall not

affect other people using the earth return. Possibly, if they did so, their running charges might be a little more economical, and at the same time it would be advantageous to those using the earth as a return. Mr. Thorrowgood.

Mr. E. C. IRVING: I do not think there is much I can say in criticism of the paper, which is very clear to me, probably because I have had a lot to do with track circuits. There are one or two points I should like to mention with regard to the wooden block joint. So far as my experience goes, I have seen a wooden block joint pulled apart about 2 ins. in the morning after a cold night through the contraction of the rails, and then close up again through the expansion of the rails as soon as the sun came out, so that I do not think a wooden block joint itself is quite strong enough for railway work. It would be all right so far as the lateral stress is concerned, but not for the contraction. There is another point which is not very important, and that is the use of the copper bond. I think galvanised iron is rather better than copper, because there does not seem to be so much electrolytic action between the two pieces of iron as there is between the copper and the iron. This action, I think, where small voltages are concerned, is rather detrimental. It may be interesting to the members of this Institution to call their attention to the fact that the use of the track circuit in this country has passed the experimental stage, there being over 300 miles of it in use on various railroads, where it is giving every satisfaction. Mr. Irving.

Mr. F. GILL: The remarks I have to make are more in the form of questions. There are a number of points in the paper that I have not been able to follow at all, so that I thought if I put some questions to Mr. Brown it might assist others who are in somewhat the same difficulty as myself. With regard to the question of the design of the relays, I do not see at the moment why he wants to operate his relays with such a small expenditure of energy. He says on steam railways a relay should work with no more than 0·015 of a watt. Then further on in his paper Mr. Brown talks about economic working, but I cannot see that economy of current in the track circuit can be a very important point. I should have thought you could have afforded to spend energy rather liberally if it produced a safe result, remembering that there is a train at the back of the whole thing, and that if there is trouble it may be serious. Then with regard to the ballast resistance, or the leakage circuit, as one speaker called it, I do not see in the paper any definite figures under that heading. We get various illustrations saying that it may be one thing or another. On page 115 Mr. Brown says it may be a 333 ft. circuit at 2 ohms per 1,000 ft., or a 2,000 ft. circuit at 12 ohms per 1,000 ft. One would like to know what figure he actually gets in practice. In the same way, what is the resistance actually caused by sanding? I rather find all through the paper an absence of definite figures which one can seize and work on. With regard to the relay contacts, I notice that they are carbon and platinum. I take it that those are the local circuit contacts; but the reason given for the use of carbon and platinum contacts is so as to give freedom from lightning discharges. It is not very apparent to me why that should be the Mr. Gill.

Mr. Gill. case. I should like to know what is the pressure on the back contacts of the relay. When the relay has fallen back, when it is shunted, I understand there is a series of back contacts made, and there is no mention made as to what pressure is on those contacts. With regard to the question of storage cells, last year I was in the States, and in going across the Union Pacific line I went over mile after mile of automatic signalling, going out West. I am not sure whether it was worked on this principle; it was some sort of track circuit. The rails were certainly bonded, and the signals worked very nicely. I would rather like to know if Mr. Brown can say how they manage to look after the supply there. Do they use portable cells or primary cells, or have they some manner of keeping the secondary cells charged?

Mr. Duddell. Mr. W. DUDDELL: Like the last speaker, I fear that I have no knowledge of railway matters. I can only ask a few questions, and Mr. Gill has largely anticipated the questions I intended to ask. I think if Mr. Brown could give us in his reply some tabulated data as to the resistance of the track between the rails under different conditions of work, and also as to the resistance of the different classes of vehicle when shunting the track, it would be a valuable thing to have on record, so that when we want to calculate track circuits on railways in the future we may know what sort of figures we may expect to obtain. We want to know more or less the average results he has obtained in his extensive practice.

I think a little difficulty has arisen to-night in reading the paper from the terms the author has used. The relay drop failing point and the relay pick-up failing point are not terms that we are quite accustomed to. If Mr. Brown had referred to the volts with which the relay will pick up its armature or drop it, I think I should have read the paper with greater ease. I think the terms Mr. Brown used are terms which are used in another country, and which are not acclimatised in England yet.

One of the speakers has referred to a possible 7-volt drop along the rails. Mr. Brown, in his paper, speaks of 0·2 volt as being the sort of voltage at which his relay operates, and it looks at first sight as if a 7-volt drop along the length of line might make it an impossible thing to work the relay. If I understand him correctly, it is like this:—

FIG. H.

A B and C D are the two rails, and I assume that there is a considerable difference of potential—say 7 volts—between A and B, and

Mr. Duddell.

also between D and C. This difference of potential, being due either to extraneous causes or to very large but equal currents in the rails, will be in the same direction in the rail A B as in the rail D C, and I represent these voltages by the long arrows. The small arrows represent the voltages caused by the track battery. If we follow these small arrows round the circuit, we find that the voltage represented by the large arrow is added in one rail and subtracted in the other, so it should not hinder the working of the relay. If this is not the correct explanation, I shall be glad if Mr. Brown will explain, as it is rather difficult to understand how he operates his relay with only 0·2 volt, with possibly some thousands of amperes flowing in the rails.

Mr. Thorrowgood.

Mr. THORROWGOOD: May I explain that the 7 volts I referred to were on the extreme end of a tramway line, not a railway line. You get the leaking current from the tramway through the railway line, and back again to the central station of the tramway, not the electric railway.

Mr. Duddell.

Mr. DUDDELL: Seven volts on both rails?

Mr. Thorrowgood.

Mr. THORROWGOOD: Yes.

Mr. Johnson.

Mr. A. H. JOHNSON: I had the honour twenty-three years ago of being connected with some of the first experiments in America on track circuits. Track circuits were first started in this country. One of the first experimenters was Mr. W. R. Sykes, who tried the scheme on the Chatham and Dover Railway in 1870. I believe the first large installation was at the Crystal Palace, about 1876, but, owing to the principles of the thing not being clearly understood, it was a failure, although it is to be noted that Sykes used a one-cell battery and a one-ohm relay in that installation. The Mansell wheel was the difficulty. When we first started in America we had a great many failures through not understanding the principles on which we were working. Although several members have spoken of the paper Mr. Brown has just read as not being very clear, it is the best exposition of the matter that I have ever read. Perhaps Mr. Brown will admit that the margin of 25 milli-amperes between the current at which the armature picks up and that at which the armature drops is not ideal, and I think he says so in his paper. That is already being overcome in the alternating track circuit relay, but the last word has not yet been said on the design of direct-current relays. I think it is possible to design a direct-current relay with a very little margin. The armature might be balanced in quite a different way. Instead of having to make a hard contact it might make a mercury contact, and the whole thing might be protected from the weather and moisture by being immersed in a light oil. I have had such a relay made, and I believe you will find that relays will in the future be made on those lines. In conclusion, I think we have to thank Mr. Brown very much for his able paper.

Mr. Raphael.

Mr. F. C. RAPHAEL: There is one question I would like to ask. Is it right that the whole system should depend on a $\frac{1}{4}$ in. thickness of fibre between the two lengths of rails, for if this $\frac{1}{4}$ in. gets short-circuited the signal would immediately go to danger and the whole system breaks down. There is another point I would like to ask about

Mr. Raphael. in connection with the insulating joints. Mr. Brown showed us on the screen an illustration of these joints (in which, by the way, the width of fibre certainly looked greater than ¼ in.), and subsequently he pointed to the bolts being insulated with bushes; but I do not quite see why, if he uses wooden blocks as fishplates, he wants to insulate the bolts that go through them as well.

Mr. Sayers. Mr. J. SAYERS (*communicated*): I am sorry I cannot be present. I should particularly have liked to have asked Mr. Brown for more definite information as to the means he proposes for making dirty, irregularly used tracks as safe and reliable with track-circuiting apparatus as are ordinarily clean, well-used rails. I presume his remedy is simply to cut up the track into more and more sections, and so gradually get nearer to a short and perfectly insulated pair of rails. I think myself that track-circuiting should be limited to roads which are in practically constant use, and it should never be absolutely relied on for any track which may lie idle for several hours, especially if the track is then liable to receive light engines or similar vehicles which have been standing all night.

There is also the question of sand to consider. Unfortunately, this is mostly used in front of starting signals; consequently when a light engine comes to a stand at a starting signal it is on the very piece of line which is most likely to have a bad conducting film. I have had cases on the Midland Railway of complete insulation of an engine on a track standing in front of a starting signal where a lot of sand had been previously used by a light engine. Light engines are worse in this connection, as there are no train wheels after them to help clear away the insulating layer. I think it is very desirable for some means to be adopted which will render the operation of the relays more independent of weather conditions in order that the maximum rolling shunt-resistance which will operate a given relay shall remain a constant.

In the event of the use of track-circuiting for automatic signalling it is particularly necessary that the track controlling the last signal should overlap the next one by the usual space of about 400 yards. This has not been done in all cases that I have seen, and the absence of this overlap is to my mind a rather serious matter.

Mr. Brown. Mr. H. G. BROWN (*in reply*): To avoid repetition I will deal with the various points brought out by the discussion under their headings, instead of answering the speakers seriatim.

Economy of Power.—The energy required for the operation of a system of automatic signals is used for the production of the "clear or go-ahead" indications. The "danger or stop" indications are obtained by the interruption or withdrawal of the energy producing the "line clear or safety" signals. The armature of a relay and the semaphore arm of a signal go to the danger position, or go to a position that causes a danger signal to be given, by means of the force of gravity. The movement due to gravity must be reliable, and the weight required to obtain reliability depends largely on the design of the apparatus. To enable the system to operate, sufficient power must be applied to raise

these weights in a reasonable time. Economy of power may be obtained by decreasing the necessary work, providing the reliability of operation is not sacrificed. A steam railway track circuit can be reliably operated by maintaining a potential of from 0·2 to 0·5 volts between the rails. Mr. Brown

On an electric railway both the traction and the signal currents may make joint use of one at least of the track rails. The presence of both currents in the conductors forming the track circuit is a normal condition on a railway using the track as a return, and although abnormal on a system with a return conductor rail, it exists with sufficient frequency to warrant a similar treatment of both systems from the signalling standpoint. It is essential, however, that the signal apparatus must be in a certain manner unresponsive to the traction current, that is, it must be impossible for the traction current to cause a false "section clear" indication.

In the direct-current system obtaining this result, the potential between the rails must be greater than one-half the potential drop within the limits of the track section caused by the traction current. This minimum track circuit potential value is necessary to insure an uninterrupted exhibition of the "line clear" indication when the section is unoccupied.

It is unnecessary and therefore unwise to use more power for a signal system than is required to meet the above conditions.

Storage and Gravity Batteries.—The following are three methods of using batteries for track circuiting: the gravity battery, the portable storage battery, and the permanent storage battery charged from mains run to the battery locations. Which scheme is used depends on the size and importance of the installation and other local conditions. Permanently located storage batteries with a system of charging mains would be cheapest and would give the best results on a four or six track line having short sections and a frequent traffic, while the gravity battery would be a more economical means of obtaining power on a line carrying a relatively small traffic with long distances and requiring a small amount of power per unit of distance. The use of storage batteries is to be preferred in intermediate cases where there may be uncertainty as to which would be cheapest.

The Relay.—The efficiency of the track circuit will be increased by the reduction of the difference in value between the armature pick-up and drop points. This reduction can be obtained by the use of the moving coil relay, although the difference cannot be entirely removed. An application for a patent has been made to cover the use of this type of relay, in connection with a track circuit to obtain the benefits of the principle involved.

The platinum to carbon contacts do not "give freedom from lightning," but prevent the contacts fusing together when subjected to intense extraneous currents.

The back contact pressure of the standard steam railway relay described is approximately 25 grammes.

Mr. Brown. It is universal practice to adjust relays by the milliamperes passing through them. I believe it is better to use a voltmeter in multiple than a milliammeter in series when the relay is in service on a track circuit. The calculations are simpler and the condition of the circuit is more graphically evident. I most heartily join Mr. Sayers in his wish that the operation of the relay could be independent of weather conditions and the value of the effective rolling shunt could be always the same.

Ballast Resistance.—In actual practice the minimum ballast resistance will be found to be from 4 to 12 or 18 ohms per thousand feet. With fang bolt construction in normally wet and poorly drained places it is not unusual to find but 1 or 1½ ohms per thousand feet. There are many long track circuits working satisfactorily for main line traffic that have a total resistance between rails of only one quarter or one half ohm. I have made many ballast resistance tests to find out whether or not the resistance varied with the potential used, but I have never obtained results that led me to believe this was the case. If there is any difference up to, say, 7 or 10 volts, I think it is so small as to be practically of no importance.

Rail Resistance.—The resistance of an 85-lb. rail is approximately 0·01 ohm per thousand feet. The total rail resistance of a track circuit varies. The total resistance of the bonds is usually greater than that of the rail itself, particularly so when short rails are used. The variation is caused by the varying resistance of the rail joints. The total resistance will never be less than that of the rail only, and never as great as the resistance of the rails plus the bonds. The resistance of an unbonded joint may be anything from practically nothing to an ohm or two. I once tested a joint carrying heavy traffic that was tightly bolted up and had a maximum resistance of 15 ohms. Any result from a very low resistance to the maximum mentioned could be obtained by tapping it with a sledge hammer. The better the bonding the better the results.

Insulating Joint and Copper Bonds.—In my experience a copper bond properly installed gives no trouble whatever, due to electrolytic or chemical action. I should think that the case mentioned where a wooden joint pulled apart and the rails separated two inches should be considered a criticism of the permanent way construction or maintenance rather than of the wooden joint.

Sand.—It is evident that there must be a more or less intimate relation between the vehicle and the rails. But it is impossible to say without knowing the exact conditions existing at the time, in the case mentioned by Mr. Sayers, whether or not the difficulties encountered were insurmountable. In my personal experience I have never known a case when these difficulties could not be overcome.

Overlap.—An overlap is absolutely necessary to insure safe working. In no case should it be possible for a signal to go to the safety position until the tail of the train has passed a certain distance beyond the next signal.

The length of this overlap should be determined by the braking rate

and maximum speed of the trains. It is usual practice to take the distance required to stop a train travelling at maximum speed by an emergency application of the brakes made when passing the signal, and to add, say, 25 per cent. as a factor of safety. The distance is not always the same at all signals, as it is altered by the alignment and grade. This allowance is amply sufficient, as it is reasonable to assume that the application of the brakes will be made before passing the signal, if at all, unless the automatic stop is in use. If the overlap is short the signal need not go to danger until the front end of the train has reached the release point of the previous section, but if it is long it is better to make the overlap absolute, that is, the signal is put to danger as soon as the front end of the train passes it, causing both signals to be at danger while any part of the train is in the overlap. Mr. Brown.

Extraneous Currents.—The time available is insufficient for an exhaustive treatment of the effects of extraneous currents on the track circuit. It is extremely unlikely that the total return drop of a train system would ever be felt on a single track circuit, though it is often the case that extraneous currents create a potential between the rails of a sufficient value to interfere seriously with the operation of the system. It is evident that this potential need not be very high to accomplish this result, because the normal track potential is less than a volt.

Generally, this interference tends towards the interruption of traffic only, but under certain conditions it may cause the exhibition of falsely clear indications when the section is occupied, unless a system which is safeguarded from the dangerous effects of these influences has been adopted. Such a system should be chosen for localities where trouble may occur from these causes.

The PRESIDENT: I now ask you formally to convey your thanks to Mr. Brown for his paper, and for giving in his reply some of the additional information asked for in the discussion. The President.

The resolution of thanks was then put and carried with acclamation.

The meeting adjourned at 9.40 p.m.

BIRMINGHAM LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN,

R. A. CHATTOCK, Member.

(*ABSTRACT.*)

November 21, 1906.

I am deeply sensible of the honour you have done me in placing me in the position of Chairman of this Section of the Institution, an honour for which I feel the highest appreciation. I can assure you that it will be my earnest endeavour to maintain the high tone which has always characterised the Birmingham Local Section, both as regards the papers to be presented before it and as regards the discussions upon those papers.

When we look down the list of names comprising the Members, Associate Members, and Associates in our Section, we come across a considerable number of gentlemen who are connected with the educational side of electrical engineering—gentlemen who are holding some of the highest positions in the educational world. I think you will all agree with me that our Section is to be warmly congratulated upon this fact, more especially because these gentlemen have associated themselves to a very great extent with the furtherance of the interests of this Section.

Such men are not specialists in any one branch of electrical engineering in the same way that most of us are. They keep in touch with—in fact, many of them are responsible for—the latest developments in the electrical industry, and they have to be thoroughly conversant with all branches of that industry in order to train the young engineers who pass through their hands. We all know how important education is to the young engineer at the commencement of his career, and it is here that we reap the benefit of being associated with gentlemen who are connected with the educational side of the profession.

Necessarily we are all becoming specialists in one branch or another of our profession. Personally, I have myself specialised for a considerable number of years now in that branch dealing with electric supply, and as this branch has recently been making very considerable strides in Birmingham, it may be of interest to the members to offer a few remarks upon the considerations governing this development.

In one way Birmingham is to be congratulated upon having delayed the development of its electrical supply until the present time. By so doing it has been able to see how electric supply has been

developing in other large centres of commerce, and it has been in the position to start afresh, and to make provision for dealing with the enormous power supply that it is confidently anticipated will be required in its area. It has also been able to adopt the most modern and efficient types of machinery for giving this supply, and to proportion the size of its units in accordance with the demand that may be expected.

Already, although the first instalment of the general scheme is barely completed, our large manufacturers are looking carefully into the question of adopting electrical power for driving their works, and they are doing this not in a hesitating way, such as by installing a few small motors in their works, but many of them are considering the installation of some hundreds of horse power, varying from two or three hundred to as much as fifteen hundred. It would therefore appear that the spirit in which Birmingham is dealing with its electrical supply is being appreciated by its citizens, and there is every indication that the policy adopted is a right one, and will eventually tend greatly to increase the welfare of the city.

As an initial reason for developing this large scheme of supply Birmingham had its tramways, which it was decided to electrify, and by obtaining such a load upon its new station it assured itself of an immediate return upon a considerable proportion of its capital outlay.

The city is, however, as I have mentioned, confidently anticipating a very much larger demand than even its tramways can give it, and a demand which has a very much better load factor than that of a tramway supply. The importance of this will be recognised when it is pointed out that in many manufacturers' works a load factor as high as 60 per cent. can be obtained, whilst the load factor on a tramway supply averages about 25 per cent.

The great benefit of a high load factor in reducing the total costs is now well recognised, and it is only fair to those users who operate with such a high load factor to give them the full benefit of the reduction in the costs, by charging them a low price for the current.

By following up this policy and offering inducements to large power consumers to take a supply of electric power—inducements which are perfectly legitimate—there is no doubt that an enormous supply business can be built up and made remunerative in an area such as we are considering.

In the foregoing remarks I have called attention to the development of a supply for power purposes; I think, however, that the question of a supply for lighting should not be lost sight of. In connection with this, it would appear that the public have been very quick to recognise the great advantage of using lamps which have a higher efficiency than that of the ordinary incandescent lamp. I refer, of course, to the "tantalum" and the "Nernst" lamps, which are now established on a thoroughly commercial basis.

The current consumption in these lamps is roughly 1½ watts per

candle-power, as compared with $3\frac{1}{2}$ to 4 watts per candle-power in the ordinary incandescent lamp, and although in some cases by the adoption of these lamps the amount of current sold to certain consumers is reduced, yet there is no doubt that this cheaper form of lighting is popularising the use of electric light, and many people are increasing the quantity of light they use now they find they can get it at a cheaper rate. It need not therefore be feared that the smaller consumption per candle-power will make any real difference to the business of the supply company, but, on the contrary, I am of the opinion that business will be considerably stimulated by this development.

I should also like to mention to you another form of lighting, which is now beginning to come to the fore in connection with factories and workshops, that is, the "Cooper-Hewitt" or "mercury vapour" lamp. This lamp really consists of an electric arc passing through mercury vapour in a vacuum tube. It has the peculiarity that there are no red rays produced in the light given off, and the general effect is somewhat ghastly until one gets used to it; it is, however, one of the most efficient forms of lighting that we have, the consumption being from half to three-quarters of a watt per candle-power given off. In addition to this the light is so well diffused, being considerably whiter than ordinary daylight, that it is possible to see round a corner with it. The advantage of this in a workshop will be very apparent when it is remembered what intense shadows are thrown by ordinary arc lighting. Many workshops in the country are now being fitted up with this form of lighting, and there will shortly be some in this city. The results obtained are, I understand, giving the highest satisfaction both as to the quality of the light and the economy in the consumption of current. The absence of attention necessary is also another point in its favour, there being no trimming required, whilst the life of the lamps is anything from one to two thousand hours.

Turning now to the development of the supply undertaking, the first point to consider in connection with the general scheme of supply was that of the existing supply and how it could best be worked in.

The existing supply was a direct-current one, given by means of a 3-wire network at a pressure of 440 volts across the outers. The greater portion of this supply was confined to the centre of the city, only one of the outlying residential districts being equipped as well.

The existing generating stations were completed up to their full capacity, and a very considerable capital outlay was involved in them, which it was inadvisable to discard.

As a convenient site for a large power station was obtainable near to the existing generating stations, it was decided that the best way to deal with the increased demand that was expected from these generating stations was to give a bulk supply of low-pressure direct current from the new power station to each of the existing generating stations, and to use these generating stations for dealing with the peak load only. It was found economical to supply direct current in bulk in this way rather than to supply high-tension current and transform it on site.

This point therefore necessitated that a certain proportion of the machinery at the power station should be of the direct-current type.

In the outlying districts of the city, which have up to the present never been exploited, it was decided, for the sake of uniformity, to give a direct-current supply at a similar pressure to that in the centre of the city. For this purpose the city was mapped out into five districts, and a centre was chosen in each district, at which a sub-station was erected, from which it was arranged to distribute the current.

These various centres are roughly located at a radius of about three miles from the power station, and in order to supply them economically it was necessary to utilise high-pressure current. A 3-phase alternating current at a pressure of 5,000 volts was decided upon as being the most suitable for this purpose, and a periodicity of 25 cycles per second was adopted in order that it might be possible to use rotary converters for transforming the current in the sub-stations. This rather low periodicity was considered necessary on account of the considerable fluctuations in the load that are expected by reason of the large demand for power.

Rotary converters were chosen for the sub-stations on account of their very much higher efficiency over an alternative of motor-generators, with which latter, of course, a higher periodicity could be used satisfactorily.

The fact that direct-current generating plant was required in the power station for the bulk supply to the other generating stations led to the decision to give a local supply for lighting and power purposes in the immediate neighbourhood of the power station by means of direct current. By so doing the losses in transformation in this district were obviated.

It will thus be seen that a direct-current supply is available for all ordinary distribution purposes for lighting and power throughout practically the whole of the city, and the cables used for distributing this current have been installed of sufficient size for all ordinary requirements in the districts.

It was felt in the case of large manufacturers, who might require a considerable supply of power, that it would not be economical to give such a supply by means of the ordinary distributing network. The question of pressure regulation of such a network would be interfered with to too great an extent.

It was therefore decided to deal with all large manufacturers' demands by means of high-tension current supply, and for this purpose eventually a high-tension ring main will be run completely round the city, joining up the various sub-stations, and looped into the various large works where the supply is required.

In these works it is proposed to transform the high-tension current by means of static transformers into low-tension 3-phase current, by means of which a supply will be given to 3-phase motors in the works. Sanction has been obtained from the Board of Trade for a supply to be given in this way.

The fact that direct current was available in such a number of centres in the city suggested the advisability of splitting up the supply to the tramways into districts. In this way less interference from possible breakdowns is anticipated, and arrangements have been made so that, should the supply in any one district fail, the other districts can take it up between them. The power station has therefore been equipped with a certain amount of direct-current machinery for giving a supply direct to the tramways within a radius of about two miles. The sub-stations have also been equipped with distinct rotary converters for giving a supply to the tramways within their own districts. The tramway feeder cables emanating from the power station and from the sub-stations are linked up at various points so that an emergency through supply can be given should there be a failure in any one district. I trust that this short review of the points governing the development of electric supply in Birmingham will be of interest to the members. It has certainly been a most interesting problem to work out, and I hope I have made clear the various points in connection with it.

LEEDS LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN,

GEORGE WILKINSON, Member.

(*ABSTRACT.*)

October 25, 1906.

At the outset of my address I desire to thank you for the honour conferred on me by my election as Chairman of the Leeds Local Section for the Session 1906–1907. It will be my earnest endeavour, with the loyal and hearty co-operation of the Committee, to secure the further growth and useful development of our Section.

As a supply station engineer much of my time is taken up in carrying out the varied duties connected with the supply of electrical energy, and in endeavouring to solve, in the most direct and economical way, problems that arise in connection with such supply. Such solutions are necessary if the substantial benefits which electricity can furnish are to be available at a price which all can afford to pay.

Considering, first, the power station. It will often be found that while reasonable economy is secured in the generator room, and care is taken to employ efficient plant and skilled labour therein, the boiler house, being a less congenial sphere, is a department that receives less attention, and the details are not so carefully considered. An investigation of supply station records will show that from half to four-fifths of the works' cost of generation is incurred in the boiler house, and this is chiefly for coal; it is therefore obvious that where a fixed sum must be allocated annually for the repayment of capital charges, the only possible chance of cheapening the cost of production to any substantial extent lies in reducing the consumption of coal.

The free use of steam traps is a source of heavy loss in some generating stations. Due to their liberal use and to the scattered positions they occupy, the hot water they discharge is frequently led into the nearest drain and runs to waste. A long experience has shown that an equally satisfactory and more economical arrangement is to connect all the water separators, valve casings, cylinder jackets, &c., to a small pipe, from which steam is taken to operate a bank of boiler feed injectors. By this means all the steam required to work the injectors is derived from points where condensed water collects and

it is thus all saved and expeditiously carried back with the steam through the injectors direct into the boilers. The injectors used should be of varied sizes, the smallest, when at work continuously, being equal to feeding the boilers on low loads. One steam trap of suitable size is provided at the end of the injector block, and this trap comes into action during the short and infrequent intervals when the injectors are out of action for repairs. Another advantage of this arrangement is, that the large and often wasteful steam-driven feed pumps can be shut down and maintained as reserve plant.

An economical stand-by boiler-feed arrangement is obtained by employing three throw pumps driven by electric motors and fitted with variable stroke gear which allows the use of a constant speed motor and permits the graduation of the stroke of the plungers from the maximum to zero, or *vice versâ* whilst in action. An indicator should be fixed on each pump to show the amount of water delivered per hour.

A further easily realised and very substantial economy is to employ water of steam temperature to feed the boilers. Effective plant for securing steam temperature feed is simple, easily applied, and neither costly nor cumbersome, and its adoption will secure an increased economy averaging at least 7 per cent. Where steam temperature feed water is introduced, a further step in point of output and efficiency may be expected if the boiler furnaces are enlarged and fitted in every case with a brick arch so that the flames do not directly impinge upon the heating surfaces. This will not only reduce the smoke, but will raise the furnace temperature and increase the steam output of the boiler.

Practical objections to such enlarged furnaces are (1st) the difficulty of covering the bars with fuel, (2nd) the liability of the fire to burn in holes. These objections, I think, may be efficiently met in tubular boilers, by employing the well-known chain grate bars of extra length and width, and in Lancashire boilers the most effective remedy appears to be the removal of the furnaces from the narrow flue tubes to the front of the boiler, larger furnaces being built out on the boiler front with wrought iron plates well lined inside with firebrick and provided with firebrick arches, and on the outside by ordinary brickwork, a cavity being formed between the outside brick lining and the metal shell of the furnace. Air for supporting combustion will be drawn through this cavity entering by an aperture located over the furnace and round the sides thereof, thereby picking up any waste heat and passing at a high temperature through the grate bars to the fuel. By these methods not only will a higher furnace temperature be reached, but the heat upon the boiler plates will be better distributed and the ebullition area thereby extended.

So long as carbon filament lamps continue to be used and in view of the improvements in efficiency and grading which will be effected in these lamps in the near future by British manufacturers, it is important that more uniform pressure should be obtained upon electricity supply mains. My experience is, that pressure regulation

by automatic apparatus is the only satisfactory and effective method of obtaining good and dead beat regulation. In addition it is necessary to have distribution mains of ample section, as pressure regulators can maintain uniform pressure only at predetermined points upon the network. The too frequent practice of increasing the pressure to an excessive degree at feeding points, in order to compensate for abnormal drop upon the distribution mains, is to be strongly condemned; the proper remedy for poor lighting at the extremities of distribution mains is to put down more copper.

Works which still give a low voltage supply—say, near 100 volts—are likely to reap a rich reward in the near future, as the consumers are in a position to obtain the economic benefits offered by the new metallic filament incandescent lamps. I have had lamps of this type in use in my house since July last, each giving a light equal to 32 British candles for an expenditure of under 40 watts. With current at 3d. per unit, and including the extra retail price of the metallic filament lamps as against the carbon lamps, and assuming an average life of 600 hours only for the metallic filament lamp, the relative cost comes out for 32 c.p. lamps for the 600 hours at 11s. in the case of the former as against 17s. 9d. for the latter. As far as I can judge from results in my house, the useful life of the metallic filaments will average at least double that of the carbon lamps, while they are not so sensitive to voltage variation, give a more brilliant quality of light, and the bulbs on normal voltage do not blacken, hence the candle power is more uniform throughout the life of the lamp.

The question of tariffs is one which might with advantage be considered by an organisation representative of the whole supply industry. The issue of recommendations by such an organisation would be useful to engineers, and especially to directors and committee-men, many of whom appear to be in utter ignorance of the fundamental principles governing the vital question of tariffs.

In my view the adoption of flat rates with graduated discounts, according to the number of units used during a specified period, is fundamentally wrong. Such a tariff cannot be fair and equitable; customers who are quite unprofitable may get a substantial discount, while customers who are exceedingly profitable to the supply may get a very small discount. Attempts to get over this inequality of treatment due to flat rate tariffs have been made by varying the discounts according to the class of consumer under consideration, but this is only lessening the evil and is in no sense a cure. Again, there is often a big discrepancy in the prices charged for lighting and for power, the former being frequently too high, while the latter is certainly often too low.

The present alternative to the flat rate is the well-known maximum demand tariff, which is a more equitable method. Inasmuch, however, as the method of calculating the amount due is unintelligible to many consumers, it has become unpopular; moreover, a customer charged on the maximum demand tariff does not know until the end of

the quarter, or the half-year, to what extent he will benefit, or, in some cases, he does not know if he will benefit at all, as this is not determined until the quarter or half-year has expired, whilst he is often nervous about switching on his lamps lest he should increase his maximum demand.

In some cases demand indicators have been used to develop business in its early stages, but when the business has reached a point at which the engineer can afford to make a change, a flat rate tariff has been introduced and the demand indicator system abolished. The change, while simplifying the accounts and appealing as it does by its simplicity to the consumer, has no serious prejudicial effect upon the receipts, as the losses made on some consumers are counterbalanced by the extra profits obtained from others. This condition of affairs is satisfactory so long as the supply authority is content to make excessive profits on some consumers and to apply a part of them in making up the losses on other consumers; but the former are likely to rest content with flat rate tariffs just so long as they remain in ignorance of the true condition of affairs, and no longer.

The chief factor in the cost of supply is made up by the "fixed charges"—exactly how much this is per annum is easily ascertainable in every case, and it probably varies from year to year. Each consumer should be charged his fair proportion of this sum. The exact amount to be paid should be recorded on each consumer's premises, not in the form of so many units chargeable for half-year at an increased price per unit, which is the clumsy and perplexing method of the demand indicator, but by an instrument which records the consumer's proportion of standing charges clearly in £ s. d.

The remaining item in the total cost of supply is the "generation cost," which is by far the smaller item and is practically a uniform and unvarying cost per unit, to cover which a very low flat rate can be fixed. Considerably less than 1d. per unit will cover this item, and also afford ample profit to the supply authority.

As illustrating the present wide divergence in the matter of tariffs, I set forth below the published scales of charges of four of the largest and most important municipal undertakings in the country, which may be taken as fairly representative of the whole kingdom. These I designate A, B, C, and D:—

A. The rate payable by a consumer for a supply of electrical energy for lighting in any one establishment (whether used direct from the mains or through a motor generator or other apparatus) shall be 3½d. for each unit up to 3,000 units per quarter, 3d. for each unit in excess of 3,000 units to 10,000 units per quarter, and 2½d. for each unit in excess of 10,000 units per quarter. A reduced rate shall be charged for electrical energy supplied for power or for cooking or heating when measured by a separate meter—namely, 2d. for each unit up to 3,000 units per quarter; 1½d. for each unit in excess of 3,000 units per quarter to 10,000 units per quarter; and 1d. for each unit in excess of 10,000 units per quarter.

B. Electrical energy used for lighting: For first 300 units per quarter, 5d. per unit; for any further quantity used in the same quarter, 3½d. per unit. Electrical energy used for long hour lighting: For first 300 units per quarter, 3d. per unit; for any further quantity used in the same quarter, 2d. per unit.

> NOTE.—This special low lighting rate has been fixed to encourage a day load, and is for the benefit of consumers who use their supply of electrical energy for lighting during a large number of hours per annum.
>
> In order to avail themselves of it consumers will be required to enter into an agreement in which they guarantee a minimum payment of £15 per annum for not more than 12 16-c.p. lamps, or their equivalent, connected to the circuit; a further guarantee of 25s. per annum for every additional 16-c.p. lamp, or its equivalent, connected to the circuit will also be required.
>
> Current reckoned at the above rate per unit can be used up to the value of the sum guaranteed, any additional current being charged at the same rate per unit.
>
> Consumers will be required to have all lamps so guaranteed connected to a circuit kept distinct from the ordinary lighting supply, and a separate meter will be installed upon this circuit.
>
> Should any alteration be made in the number or candle-power of the lamps so guaranteed, notice must be at once given to the Department, or in default the consumer will be liable to have the supply charged at the Department's current rates for ordinary lighting purposes.
>
> Electrical energy used for motors: For first 300 units per quarter, 2d. per unit; for any further quantity used in same quarter up to 3,000 units, 1½d. per unit. Where 3,000 units or over are used per quarter (after the first 300 units at 2d. per unit), 1¼d. per unit.

C. Private lighting: Maximum demand system, with initial charge of 4½d. per unit with daily average of 1½ hours and 2½d. per unit after, or flat rate of 4½d. with discounts from 2½ to 44½ per cent. Power and heat: Maximum demand system, 2½d. per unit for daily average of 1½ hours, and 1½d. after, or flat rate of 2½d. per unit with discounts from 5 per cent. to 65 per cent. Traction supply: 1¾d. per unit. Special purposes: Ship yards with 30 per cent. load factor, 1⅛d. to ⅞d. per unit.

D. Charge per unit: For lighting, 5½d., or a fixed charge of £7 per annum per kilowatt of maximum demand and 1¾d. per unit; for motors, 1d. to 1⅞d. per unit according to hours of user.

The perusal of these tariffs will, I think, be convincing proof that present-day methods of charging are unsatisfactory, and they must appear unintelligible and chaotic to the general public.

An ideal tariff would fulfil the following conditions:—

1. Protect all consumers from unfair or excessive charges.

2. Secure from every consumer a reasonable and readily assessable profit to the supply authority.

3. Be applicable without variation to all conditions and for all purposes for which electrical energy is used.

4. Be capable of easy explanation and its equity should be apparent to all persons of average intelligence.

In conclusion, may I express the hope that the Session upon which we are entering may be a progressive and beneficial one; as you are already aware from the published programmes, the meetings (with a view to extending the influence and benefits of the Section, and also to afford the distant members facilities for taking part in our proceedings) have been arranged to take place in several important cities and towns covered by our centre.

I trust the result will be an increase in the attendances and a full and free discussion upon the interesting and useful subjects which are to be brought before us.

MANCHESTER LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN,

THOS. L. MILLER, Member.

(*ABSTRACT.*)

November 20, 1906.

In his interesting address at the commencement of last Session, Mr. Pearce, in referring to the benefits of efficient technical training which students of the present day enjoy, pointed out that much of the benefit so derived would be lost unless it had a practical application. Without entering into a discussion as to the best method of education and training of engineers, I should like, as briefly as possible, to touch upon one or two points which I do not think have received the attention they merit.

The idea held by so many in the early days of the technical education movement that the various universities and technical colleges would be able to turn out engineers equipped not only with the necessary technical knowledge, but also with the works' experience gained in the college shops is now almost if not entirely dead in this country, although this system of training has been adopted to a considerable extent in the United States.

The function of a shop training is to make the student familiar with the operation of machine tools and appliances under actual conditions of work on a commercial scale, and to teach him how labour must be organised in order to produce the best possible results at the lowest costs.

To become a successful engineer something more is required than mere technical ability and workshop skill, for, unless a man is endowed with what I would term the commercial instinct, he is not likely to attain to great prominence in his profession. It is not enough to be able to design the most perfect machinery, unless such machinery can be produced at a price that will compete with the productions of rival manufacturers, and to do this it is not only necessary that we should know the cost of producing the machine, but also the cost of each step in the process, as it is only by comparing the actual cost item by item with the estimated cost that wasteful methods can be discovered and eliminated.

I am fully aware that many firms object to the members of their

technical staff having anything to do with the estimating department, but this, to my mind, is a very narrow view to take, as unless the man responsible for the design is enabled to follow the work in the shops and check the cost of each step in the process of manufacture, it cannot be expected that the most economical results will be attained.

There are, of course, considerable difficulties in the way of obtaining a knowledge of the business side of engineering, but much could be done, I think, if the various universities and technical colleges could arrange for courses of lectures, preferably in the evenings, dealing with, say, the Principles of Estimating. Lectures on commercial law, with special reference to drawing up of specifications and the making of contracts, would also be of invaluable assistance and would no doubt be the means of avoiding legal disputes which so often follow loosely drawn-up specifications and contracts.

As Mr. Pearce pointed out last year, there are many branches of the profession, and we cannot become specialists in each. In my illustrations I have laid particular stress on the "works" side, as that is where most men leaving the universities and technical colleges will go, but the same argument applies practically to all branches.

In promoting the advancement of engineering science it is gratifying to note that the various institutions do not limit themselves merely to the publication of the papers read at their meetings with the discussions thereon, but that, in addition, they have carried out, by means of committees of the institutions, research, and other, work of interest to the profession at large, from which most important results have accrued.

Our own Institution has done good work in preparing a model form of general conditions of contract, which although not generally adopted has been of considerable value in showing the lines upon which such general conditions should be drawn; and in drawing up a set of rules embodying the requirements and precautions to be adopted in wiring buildings to be supplied with electrical energy. These rules are at present under revision, and it is hoped that when issued the new rules will be adopted not only by electrical engineers but also by the fire offices, who are so greatly interested in the subject.

In this connection I would point out that the Institution represents the profession, and that it is the duty of each and every one of us, if we wish to see our profession take the position we consider it should do among the learned professions, not only to support it by our membership, but also by attending its meetings and by contributing the results of our experience or observations by reading papers or by taking part in the discussions.

Dealing now with the question of electricity supply, it may be remarked that the most serious problem confronting the central station engineer of to-day is the reduction of price at which electrical energy can be profitably sold to enable him successfully to compete for new business.

The cost of generation and distribution is now so low in the

majority of towns that it is only by an increase in the volume of the business that any substantial decrease can be looked for in the price at which the electrical energy can be profitably sold. This, of course, is due to the fact that the standing charges do not grow at the same rate as the generating costs.

Up to the present the attention of those in charge of electricity undertakings has been more particularly directed to the reduction of the working costs, the securing of new business being largely left to the wiring contractors, it being found impossible in the great majority of cases for the engineer, with the small staff at his disposal, adequately to canvass the district.

During the past year or so, however, a change has been taking place, and much more attention is now being given to the obtaining of new consumers, and a special department is being organised in some of the larger undertakings for this purpose. In the United States this side of the business has been developed to a much greater extent than over here, but the conditions differ so greatly that I fear the methods adopted there are hardly applicable to the conditions prevailing here.

The hiring out of motors, the free supply of incandescent lamps, and the (so-called) free wiring of consumers' premises, have each been adopted with varying degrees of success in this country as a means of increasing the volume of business, the hiring out of motors being probably the most successful.

The further methods now suggested are the arranging of permanent exhibitions of the most up-to-date electrical appliances; the advertising of the advantages to be derived from the use of electrical energy; the circularising of possible consumers; and the appointment of canvassers for new business. These are all excellent methods of dealing with the problem, and I have no doubt that their adoption will be attended with a considerable measure of success.

Another factor tending to keep up the price of electrical energy is that of the capital charges, which constitute such a heavy burden on most undertakings, and more particularly so in the early years of their existence. In some of the older works put down, when prices were much higher than they are at present, and where a purely lighting, or a lighting and small motor load, is dealt with, the effect of the capital charges is very severely felt, and the margin of profit is extremely small indeed. In addition to this, there is the vexed question of depreciation, which is continually cropping up, and upon which such very different views are held, and particularly so as to the necessity of providing such a fund in connection with municipal undertakings, apart from the statutory contributions to the sinking fund for the repayment of the principal of the money borrowed.

Dealing with this question, it may be pointed out that there is no statutory obligation to provide for depreciation, there being no mention of depreciation in either of the 1882 or 1888 Electric Lighting Acts, nor in the 1899 Electric Lighting (Clauses) Act.

In the 1899 Act, however, provision is made in Section 7 for the application of the money received by the Local Authority as undertaker, the disposition of the money so received being provided for as follows :—

1. In payment of working and establishment expenses and maintenance charges.
2. In payment of interest on loans, or
3. Providing sinking fund instalments.
4. In payment of all other expenses of executing the special order, not being expenses properly chargeable to capital.
5. In providing a reserve, if they think fit, by setting aside such money as they think reasonable, and investing the money and the resulting income thereof in Government securities, in which trustees are by law for the time being authorised to invest, other than stock or securities of the undertakers, and accumulating it at compound interest until the fund so formed amounts to one-tenth of the aggregate capital expenditure on the undertaking.

This reserve fund, it is further provided, shall be applicable to meet any deficiency at the time happening in the income of the undertakers from the undertaking, or to meet any extraordinary claim or demand at any time arising against the undertakers in respect of the undertaking, and so that if the fund is in any way reduced it may thereafter be again restored to the prescribed limit, and so on as often as the reduction happens.

It is also provided that the net surplus remaining in any year, and the annual proceeds of the reserve fund when amounting to the prescribed limits, must be carried to the credit of the local rates, as defined by the principal Act, or it may be applied to the improvement of the district or in reduction of the capital borrowed for electricity purposes. Should the surplus exceed 5 per cent. of the capital expenditure on the undertaking, it is further provided that the undertakers shall make such a rateable reduction in the charge for the supply of energy as in their judgment will reduce the surplus to that maximum rate of profit. On the other hand, any deficiency of income in any year when not met out of the reserve fund is charged upon and payable out of rates.

From evidence given by Mr. J. N. Kershaw, one of the Assistant Secretaries of the Local Government Board, before a Select Committee of the House of Commons in the 1902 Session of Parliament, which was appointed "to inquire into and report as to the statutory and other conditions limiting the period for the repayment of loans raised by the local authorities," it appears that the principles upon which the Board proceed are :—

1. That the loan period shall not exceed the life of the works, and
2. That the future ratepayers shall not be unduly burdened.

It is generally acknowledged that the probable life of certain portions of the plant is shorter than the period allowed by the Local Government Board for the repayment of the loans, and although twenty-five years has been fixed upon as the equated life of the security in buildings, machinery, mains, &c., still it is morally certain that some portion of the plant will require renewing before the end of this period, whereas the life of the other portions will probably be much in excess of the period named.

To provide for such renewals, and also for the purpose of satisfying any deficiency in the income of the undertaking, or for meeting any extraordinary claim, the Local Government Board, as I have already pointed out, permit the creation of a reserve fund, but limit it to 10 per cent. of the capital expenditure.

In the case of a company it is necessary to provide a depreciation fund sufficient to renew the assets at the end of their useful life, as unless this is done fresh capital will have to be raised for the purpose, or an undue burden will be put on the revenue for the particular years in which this occurs.

With a local authority, however, the case is somewhat different, as it is under an obligation to extinguish the whole cost of the plant within the period for which the loan is sanctioned. It would therefore appear that where the period for which the loan is sanctioned expires before the asset has been worn out the sinking fund instalments exceed the proper amount to be charged for depreciation, and that no further charge against the revenue will be necessary, as the money required for renewing the assets can be obtained by re-borrowing, the previous loan having been paid off. Unfortunately, however, as I have already pointed out, the useful life of certain portions of the plant is shorter than the period allowed for the repayment of the loan, and it is therefore necessary to provide a fund in order to renew the plant when such renewals are required. The contributions to this fund will, of course, depend on the useful life of the asset and its residual value at the end of its useful life, and this will again depend how thoroughly the plant has been maintained in efficient working order out of revenue.

As bearing on this subject it may be pointed out that the Local Government Board allows the following periods for the repayment of loans for extensions :—

Cables laid in ducts or on the solid system ...	25 years
Armoured cables laid direct in the ground ...	15 „
Switchboard and switches	25 „
Dynamos	20 „
Wiring inside buildings	20 „
Steam engines	15 „
Transformers	15 „
Motor transformers, boosters, balancing sets, rotary converters	15 „

Instruments	15 years
Motors for hire or in generating stations ...	10 „
Others in shops belonging to the Council ...	15 „
Lamp pillars and brackets for public lighting...	10 „
Batteries and accumulators	5-7 „
Meters and indicators	5 „
Arc lamps (*i.e.*, lamps themselves)	5 „

(Nernst and incandescent lamps and carbons for arc lamps: no loan allowed).

In the "County of London," on the other hand, the periods allowed for repayment of loans are :—

Land	60 years
Buildings	50 „

For all other purposes twenty years, or, as an alternative, in place of the last two terms, a uniform period of forty-two years is allowed for all loans, but this term is only allowed on the Borough Council passing a resolution to the effect that if any works fail to be renewed during the currency of the term, the cost of renewal shall constitute a maintenance charge, and shall not be made the subject of a fresh loan. It will be seen, therefore, that the determination of the proper amount to be set aside for depreciation depends on the type of plant and the care taken in maintaining it in efficient working condition, each case having to be considered strictly on its merits.

The item of rent, rates and taxes which figures so largely in the balance-sheet of most electrical undertakings, amounting as it does in some instances to one and a quarter times the coal costs, is another obstacle to the reduction of the price of electrical energy, the importance of which cannot be over-estimated.

Dealing with this question, it may be pointed out that there appears to be no settled practice amongst rating authorities as to the basis on which the assessment of electrical undertakings for rating purposes should be carried out, and we consequently find an extraordinary divergence in the methods adopted in different districts. In some districts the indicated horse-power installed in the electricity works is taken as a basis of assessment, while in others the assessment is based on the capital expended on the undertaking, the rates per indicated horse-power and the percentage to be charged on the capital expenditure as also the deductions allowed in order to arrive at the rateable value varying to an even greater extent than the methods of assessment.

More recently, however, the practice has been to adopt the "hypothetical tenant" principle of assessment for assessing the rateable value of electricity undertakings, and to take the receipts and expenditure as the basis of calculation.

In order to obtain a knowledge of the principles underlying this

method of assessment it is necessary to go back to the Parochial Assessment Act of 1836, Section 1 of which provides that "no rate for the relief of the poor in England and Wales shall be allowed by any justices, or be of any force which shall not be made upon the nett annual value of the several hereditaments rated thereunto; that is to say, of the rent which the same might reasonably expect to let from year to year, free of all usual tenant's rates and taxes, and deducting therefrom the probable average cost of repairs, insurance, and other expenses, if any, necessary to maintain them in a state to command rent."

From this it will be seen that the annual rent is the basis of assessment. As, however, the owner of an electricity undertaking is almost invariably the occupier, it is necessary, in order to ascertain the letting value, to assume an imaginary tenant, or what in rating phraseology is known as the "hypothetical" tenant.

In the Schedule of the Act of 1836 the term Gross Estimated Rental (or gross value) and Rateable Value were used, but no indication is given as to the method of arriving at these values. They have, however, been clearly defined in the Valuation (Metropolis) Act, 1869, which defines the terms as follows:—

Gross Value is "the annual rent which a tenant might reasonably expect, taking one year with another, to pay for a hereditament, if the tenant undertook to pay all usual tenant's rates and taxes, and if the landlord undertook to bear the cost of the repairs, insurance, and other expenses, if any, necessary to maintain the property in the state to command that rent."

Rateable Value is "the gross value after deducting from it the probable annual average cost of repairs, insurance, and other expenses, if any, necessary to maintain the property in a fit state to command the rate."

To arrive at the rent which the hypothetical tenant may be expected to pay it is necessary therefore to obtain the gross estimated rental or gross value. This is obtained from the latest accounts available, by taking the gross receipts (less bad debts written off), and deducting therefrom the ordinary working and management expenses, tenant's rates and taxes, and insurance. The net receipts having thus been obtained, it is necessary to arrive at a fair division of them as between the landlord and the tenant, in order to see how much the hypothetical tenant can afford to pay as rent, and for this purpose his working capital must be ascertained and a certain sum allowed him for interest, trade profits, risk, and casualties, and renewal of tenant's chattels, which sum, after being deducted from the net receipts, leaves the gross rental or gross value.

From this gross value certain further deductions must be made known as "statutable deductions," which are the deductions referred to in the definition of rateable value hereinbefore given, and the amount remaining after such deductions have been made is the rateable value.

Stated in this manner the assessment of the rateable value of an electricity undertaking appears to be a fairly simple matter, but difficulties will be found to arise as soon as the determination of the tenant's working capital and of the statutables is taken in hand.

Dealing first with the question of working capital, it will be conceded that in order to ensure the successful working of the undertaking a tenant must have ample working capital to meet the necessary charges for rent, wages, fuel, &c., incurred in running the undertaking, and as the consumers' accounts are usually sent out quarterly, it is customary to allow the hypothetical tenant five-twelfths of the working and management expenses plus tenant's rates and taxes and insurance to meet these charges. In addition to this, in order to provide against strikes or other unforeseen circumstances, the tenant is allowed to keep a stock of coal in his bunkers equal to eight weeks' winter supply, so as to avoid a shortage of fuel. Again, in letting the undertaking to the hypothetical tenant the landlord would in all probability make inquiries as to the financial standing of the tenant, and it will therefore be necessary for him to keep a certain amount of money in the bank to meet unforeseen demands.

It having been settled by the courts that the cost of meters appertains to the capital of the tenant, the hypothetical tenant would, in addition, have to provide the capital necessary for the purchase of the meters, and also for the necessary office fittings and other accessories, such as loose tools, lamps, &c. The total working capital would therefore be five-twelfths of the working and management expenses, plus tenant's rates, taxes, and insurances, plus eight weeks' stock of coal, plus cash at bank, plus cost of meters, office fittings, loose tools, &c.

On this the hypothetical tenant is allowed 5 per cent. for interest, 10 per cent. for trade profits, and 2½ per cent. for risks and casualties, making a total of 17½ per cent. which, deducted from the net receipts, leaves the gross rental or gross value.

To obtain the rateable value it is necessary to deduct from the gross value the amount of what is known as the statutable deductions, which consist of (1) the average cost of repairs and maintenance of plant, buildings, and mains; (2) the amount to be set aside to cover waste, obsolescence or supercession of plant, buildings, and mains, apart from the general up-keep which has been provided for under the head of repairs; and (3) an amount to cover the charges which might reasonably be made for insurances. This, briefly, is the general method adopted in the assessment of rateable value on the "hypothetical" tenant principle, which, owing to the importance of the subject, I have dealt with in some detail.

It may be pointed out, however, that this principle is hardly applicable to the early years of most electricity undertakings when the receipts are not sufficient to cover all expenses and leave a fair margin of profit, as although there is no pecuniary profit it by no means follows that there is no rental value; indeed, as has been pointed out by Boyle and Davies ("Principles of Rating"), "the capital sunk and losses made

during the first few years of an undertaking should be regarded more in the nature of capital than an item for the profit and loss account, making up what is called goodwill."

Before concluding my address I should like to refer to a recent decision of the Local Government Board with reference to the charging of the wages of permanent employees to revenue although they may have been engaged in carrying out work chargeable to the capital account.

The particular cases to which I refer are those in connection with the Bristol and Grimsby undertakings. In both these cases men employed on the permanent staff had been engaged in laying new mains, and on an inquiry being held by the Local Government Board in the matter of the local authorities' application for powers to borrow for extensions, the Inspector at the Bristol Inquiry stated that "it was a principle of the Local Government Board that the work of all permanent employees should be chargeable to revenue," a similar notification being sent by the Board to the Grimsby Council.

On the receipt of this notification the Grimsby Council wrote asking the Board to reconsider their decision, and pointing out that if such labour were charged against revenue it would be a heavy tax on the present consumers. In reply, the Local Government Board wrote adhering to their previous decision, and further stating that "In the opinion of the Board those wages, viz., the wages of permanent employees engaged in new mains are establishment charges, and should not be paid out of borrowed moneys, and it is the Board's rule to deduct such payments from the amount of the proposed loan. That rule in no way prohibits local authorities from employing their own workmen."

The curious part of this decision lies in the last sentence, for according to that, and it was admitted to be so by the Inspector at the Bristol Inquiry, the Corporation can contract for the labour and charge it to capital, but cannot use their own employees.

The effect of this decision will, I think, be severely felt by some of the smaller undertakings when there is hardly sufficient outside work to take up the whole time of the "mains" staff, and where by employing them in laying services and carrying out other extensions, chargeable to capital, a better and more efficient staff can be maintained than would otherwise be possible.

It will, of course, be conceded that the provision of new mains is a capital charge, and that as this is made up of the cost of the mains themselves and the laying of them, it is somewhat difficult to see the reason for the Board's action and more particularly so when by discharging the permanent men and then contracting with them for the laying of the mains the difficulties raised by the Board can apparently be overcome.

There is, however, a certain anomaly in connection with this question of loans, as while application has to be made to the Local Government Board for electric lighting loans, the Board of Trade is

the authority whose sanction is required for tramway loans, and as there is a duly appointed Electrical Department in connection with the Board of Trade it would appear as if considerable time would be saved if all electrical matters could be referred to the Board of Trade, who, before sanctioning a loan, could ascertain from the Local Government Board the extent to which the borrowing powers of the applicant had been exercised.

NEWCASTLE LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN,

H. L. RISELEY.

(*ABSTRACT.*)

November 19, 1906.

It is my first duty on taking the chair to-night to return thanks to the members for the honour conferred upon me by them in having elected me Chairman of the Newcastle Local Section, an honour which I assure you I appreciate most highly. In the next place I propose to present for your consideration a few questions of capital expenditure on power supply as likely to provide a suitable subject for an address.

In a power station one always assumes that the primary duty of the engineer is to ensure continuity of supply. The question then arises, to what extent capital expenditure is justified in order to secure such a result. Of course, the engineer in charge will, if he has his own way, spend unlimited money to ensure continuity of supply, and in order to add to the efficiency of his station he will also demand more or less expensive refinements to aid him in bringing down his costs. A happy mean has therefore to be struck in installing plant which will operate continuously and efficiently with the smallest possible amount of cleaning and repairs.

Starting with the purchase of land for the erection of a power house, the sites procurable will probably be limited in number, but, granting the price to be right, the following are the conditions which must govern the final selection of a site :—

1. That it should be in a suitable position in relation to the bulk of its business.

2. That it should be readily accessible for coal supplies and situated as near as possible to the coal-producing area, so as to reduce carriage charges to a minimum. Accommodation must also be available for storing four or five weeks' supply of coal in such a position as will permit of the stocked coal being got into the hoppers of the boilers with the smallest possible amount of labour.

3. Ample supply of good circulating water must be available at a low lift, so as to avoid useless pumping.

A point of view which was more often overlooked years ago than it is to-day is the effect of the choice of engines and dynamos on the load factor, taking into consideration the distribution of the maximum and light loads throughout the year. If the maximum load only lasts a few hours per year, say, five hundred, it is not good policy to spend a large amount of capital on highly efficient plant when with apparatus of a simpler kind the end sought for may be attained at less expense.

This is purely a question of load factor, which in a lighting station must be very low, as quite half the plant in the station will only be required for about three hundred hours per annum, consequently it is most important to keep down the capital expenditure on this plant, as the standing charges (interest and depreciation) on the peak load plant may very easily be four or five times the actual working costs.

Take the cost of a modern power-house equipment at £22 per k.w., and the standing charges (interest and depreciation) at 15 per cent., then the standing charges per kilowatt-hour as above work out at 2·6 pence, which is about five times the operating costs, taking these at 0·5 pence. This figure of course varies proportionately to the capital charges per kilowatt, and it is therefore more important to keep down capital expenditure on that part of the plant which is only going to be used on peak loads than it is to make the plant highly efficient.

Since the days of reciprocating engines and with the introduction of steam turbines the capital charge has been reduced in the ratio of about £17 to £5 5s., owing to cheaper apparatus and the smaller foundations required. This includes generation plant complete with condensers and engine-house equipment. At the present time a great reduction can be effected by purchasing plant with a large overload capacity, contractors being prepared to guarantee plant having an overload capacity for two hours of 50 per cent. This, of course, on the understanding that the apparatus will operate efficiently at normal loads, but not necessarily with equal efficiency at overloads, this latter point, however, being of minor importance in relation to the standing charges at peak loads.

As regards condensing plant, no important reduction of expenditure upon this is possible, as the apparatus has to work at the same rate whether the generating plant is overloaded or not, but assuming a case where the whole equipment (generator and condensing plant) will only be used on peak loads in emergency, a considerable saving can be shown. A first-class condensing plant, with air pumps, etc., will cost £1 per kilowatt, and would maintain a uniformly high vacuum, whereas an ejector condenser or one of the barometric type could probably be purchased for 5s. per kilowatt.

As regards the auxiliaries in a power station, I am of opinion that the majority of these should be steam driven, as they are cheaper in the first place, and the exhaust steam is usually used to heat the feed water. In some cases where the supply of exhaust steam from the auxiliaries is small, special steps have to be taken to heat the feed water. In at least one station the steam is being taken from the third stage of

a Curtis turbine, after it has completed some work, in order to raise the temperature of the feed water.

The next point I wish to touch upon is the question of reserve apparatus and spare gear necessary for continuous supply of electric power. Like all engineering problems, this resolves itself into purely a question of costs. From the consumer's point of view, the more spare apparatus the greater the certainty of continuity of supply, but from the shareholders' point of view, the less capital spent on spare plant (which is wholly unproductive) the larger the amount of revenue available for dividends. So the mean has again to be struck, and only sufficient reserve plant kept to preserve the supply from unnecessary interruption and prevent the prestige of the power company being injured.

As regards generators, at the present day units are so large that the standing charges on one large complete unit lying idle are enough to cripple the total costs of any moderate sized station, and, seeing that the machines of to-day are capable of doing large overloads, it seems that the correct thing is to regard the overload capacity of the various units as the reserve plant to the generating station.

The weakest point in any high tension generating station is between the generators and feeders, this being the only place in a power station where it is absolutely necessary to duplicate the apparatus. I refer more especially to the busbars, which are an absolute necessity, not only for the safety of supply, but also to ensure flexibility of operation.

The capital expenditure on switchgear alone appears to have increased by 50 per cent. during the last six years. This is due to the numerous safety devices installed and to the larger space now occupied by switchboards, as well as to their elaboration, in some cases the apparatus being so complicated as to add to the chances of error in operating the gear.

Unfortunately, many up-to-date power companies have to do a lot of pioneer work with untried apparatus, which does not always turn out a commercial success; therefore, without being too ambitious and overanxious to try new apparatus, engineers should aim at the simplification and reliability of plant, and due consideration should be paid to the installation of plant which will run continuously with a minimum of attendance and repair.

GLASGOW LOCAL SECTION.

SOME PHENOMENA OF COMMUTATION.

By Professor F. G. BAILY, M.A., F.R.S.E., and Mr. W. S. H. CLEGHORNE, B.Sc., Carnegie Research Student.

(*Paper read November* 13, 1906.)

In this paper will be described some experiments on the phenomena occurring in the process of the commutation of armature currents in direct-current dynamos and motors. It is a subject upon which much has been written, and already much experimental work has been carried out. With the theoretical writings the authors do not propose to deal, beyond an occasional reference where the experimental results corroborate or correct the assumptions of the writers. Some of the experimental work to be described is substantially a repetition of earlier work, but amplified to more exact determinations, or carried out in a different manner. The authors trust to be excused if they fail to make appropriate reference to the many papers which have been published in many languages.

The paper deals with the contact resistance of the brush, the value of the brush current, the currents in the short-circuited coils, and the electromotive force between the segment and the trailing edge of the brush.

In equations connecting the currents and E.M.F.'s under the brushes in direct-current machines it is frequently assumed that the resistance of the brush contact and brush is a constant, although it has been shown by Mr. A. H. Moore, Dr. E. Arnold, Mr. E. B. Raymond, and probably others, that this is by no means the case. Current density, speed, and pressure all exert an influence on the resistance, and the experiments to be described are a more exhaustive repetition of the work above mentioned. There were some discrepancies to be noticed in their results, and the precise effects of speed and pressure had not been examined closely, so that a full investigation appeared desirable. The influence of the character of the brush holder and the effect of lubricants were also points to be elucidated.

The mode of experiment was of the simplest character. A cast-iron pulley was covered with a heavy band of copper, turned and polished to a true surface with a diameter of 11 ins. This was mounted on

the spindle of an electric motor. The two carbon brushes were set on the horizontal diameter, and current from a battery was passed through a variable resistance and an ammeter and across the pulley from brush to brush. The drop in E.M.F. across the two brushes was read on a voltmeter by potential leads soldered to the brushes themselves, so that thermo-electric effects were eliminated, and no brush-holder resistance was included. The resistance of the copper drum was negligible, but the resistance of some ½-inch length of carbon on each brush was included, as representing probable working conditions, and this has not been subtracted in the following readings. It amounts to 0·04 volts on the two brushes with a current of 60 amperes per square inch.

The carbons were supplied by Le Carbone Co., the best quality, called X, being used. It was a very dense graphitised carbon with a specific resistance of 0·00193 ohms, or about 1,200 times the resistance of copper. Common grades of brush carbon have usually about three times this resistance. It was by no means a soft carbon, wore slowly, and took a high polish.

Preliminary experiments showed that vibration would play an important part in the resistance, and in order to simplify the first tests, brush holders were used in which vibration would be as small as possible. A rigid frame was fixed to the bed-plate, and the carbons were soldered into short brass tubes, sliding in other tubes fixed to the frame, and being pressed on to the drum by helical springs. The inertia was therefore little more than that of the carbon blocks themselves, while sideways vibration was prevented by the close fit of the tubes. As the wear during the tests was small, only a quarter inch of carbon projected from the tube, and the guide-tube extended almost to this point. The area was 1 sq. in. in all tests, the same pair of carbons being used all through.

I.—Determination of Contact Resistance of Dry Carbons and Copper.

The first experiments were devoted to the conditions of dry (*i.e.*, unlubricated) contact. The surface was kept perfectly clean by a polishing pad continuously pressing on the drum. There was an indication that a slight coating of carbon dust was beneficial, causing a slight diminution in both fall of potential and friction. But as the clean surface was more definite this was preferred, and the effect of the carbon lubricant was too small to modify the results appreciably.

Readings were taken at speeds from 860 up to 3,300 ft. per minute and with pressures ranging from 7 to 46 oz. per square inch. It would cumber the paper to quote all the numerical results in full, and in general these will be embodied simply in curves giving the mean values of several sets of readings under the same conditions. Fig. 1 shows the relationships obtained. There are some irregularities which refused to be eliminated, but the general trend is unmistakable. Down

to 18 oz. pressure the speed does not influence the result. With 12 oz. the effect is barely noticeable at 2,300 ft. per min., but is marked at 3,300. With 7 oz. the effect is seen at 2,300 ft. per min., but not at 1,430 ft. It is clear that the influence of speed is indirect, causing vibration, which reduces the efficiency of contact. This will be seen more clearly below, when vibration is purposely introduced.

Fig. 2 gives the mean values of these, eliminating curves affected by

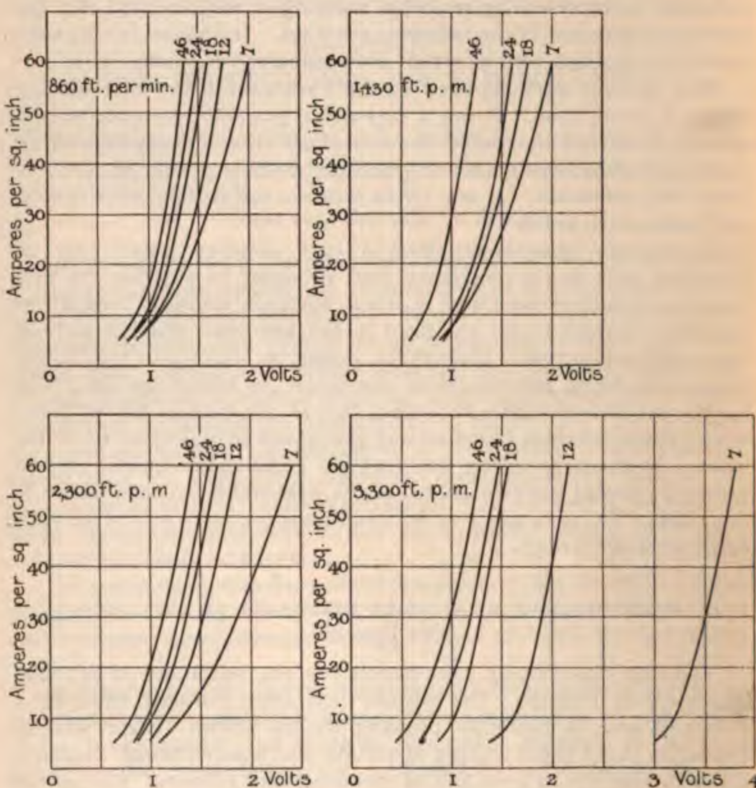


FIG. 1.—Effect of Pressure and Speed on Contact Resistances.

speed. Representing the mean of a large number of readings, it may be taken to portray the relation of E.M.F. and current for different pressures, when vibration is absent.

The curve may be expressed by the function $kE = i^{0.28}$. Assuming this index, and calculating the value of k for each point from 10 amperes to 60 at each pressure, the value of k given in Table I. is seen to be fairly constant for each pressure, and, except in the case of 7 oz., the irregularities show no regularity, so that this function expresses the

ratio of E.M.F. to current with considerable accuracy, assuming that there is no vibration. Each vertical column is derived from the mean values of a number of sets of readings, the numbers being given in the last line.

TABLE I.—VALUE OF RATIO $i^{0.28}/E$.

i.	$i^{0.28}$.	Pressure in oz. per sq. in.				
		46.	24.	18.	12.	7.
10	1·90	2·26	2·02	1·90	1·70	1·75
20	2·30	2·28	2·04	1·93	1·70	1·69
30	2·57	2·25	2·06	1·96	1·70	1·65
40	2·79	2·28	2·04	2·00	1·73	1·60
50	2·98	2·28	1·98	2·00	1·75	1·59
60	3·10	2·22	1·99	1·99	1·73	1·55
Number of readings for each value ...		11	16	10	7	4

It is clear that the constant only, and not the form of the expression, varies with the pressure. Therefore any accidental imperfect bedding, which will remain constant through one set of readings, will not affect the form of the curve. But it is necessary to use only readings from well-bedded brushes in calculating the effect of pressure, and therefore the most reliable sets have been selected, and the mean values of k for different pressures have been found to conform very closely with the expression $k = 1 + 0{\cdot}22\sqrt{P}$. In Fig. 6 is delineated this curve, with the ascertained values shown as points. The agreement is as close as can be expected. The full expression, between the limits 10–60 amperes and 7–46 oz., may be written with considerable accuracy—

$$E = \frac{i^{0.28}}{1 + 0{\cdot}22\sqrt{P}}$$

to represent the relation of E.M.F., current, and pressure with a well-bedded brush free from vibration.

Doubt may arise whether a plain copper drum really represents a commutator. Assuming that the surface of the commutator is smooth, there seems no reason to doubt this, were it not for the fact, as will be shown later, that the current from the brush at each part fluctuates as it passes over a segment, the amount of fluctuation depending on the position of the part in the brush and upon the nature of the varia-

tion of the current in the segment. This matter will be examined in the last portion of the paper.

It will be well known to all that the actual surface of contact is often only a small proportion of the total brush surface, and it is interesting to examine what effect will be produced by imperfect bedding. Let the area be reduced to $1/n$ of its nominal value. Then the pressure per unit area and the current density are increased n times. The E.M.F. is changed to the value—

$$E\frac{i_n^{0\cdot 28}}{i^{0\cdot 28}}\ \frac{1+0\cdot 22\sqrt{P}}{1+0\cdot 22\sqrt{P_n}}.$$

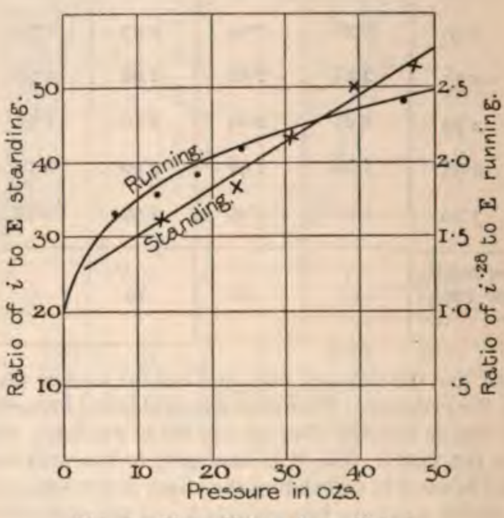


FIG. 2.—Mean Values from Fig. 1.

Within large limits this factor differs very little from unity. For example, if only one-fifth of the brush is bearing, the increase of E.M.F. is not more than 5 per cent. Hence we see that imperfect bedding has scarcely any influence on the brush losses. It follows also that with a given spring tension and current the loss will be much the same whatever size of brush be used within reasonable limits, and that there is no advantage in using low current densities. Moreover, a small, and consequently light, brush with equal total pressure will vibrate much less, and will therefore be less liable to spark. The frictional losses will be also unchanged, for it will be shown later, what is indeed quite normal, that the frictional loss is proportional to the pressure. There will be a certain ratio between current density and pressure per unit area at each speed, at which losses are a minimum, the ratio being kept low at low speeds and high at high speeds. The curves in Fig. 3 show the total losses in watts per ampere collected, plotted against

current density, for various pressures and speeds, the values being taken directly from the curves in Fig. 2, and the friction constant being 0·0005, for which see later. It will be found that when horizontal lines are drawn through, representing a constant loss, the ratio of current density to pressure is approximately constant. At a high speed, say, 3,000 ft. per minute, the 7 oz. pressure is the most economical with a c.d. of 30, or a ratio of 4 to 1, while at 500 ft. per minute we may use 7 oz. at 9 amperes, 18 oz. at 24 amperes,

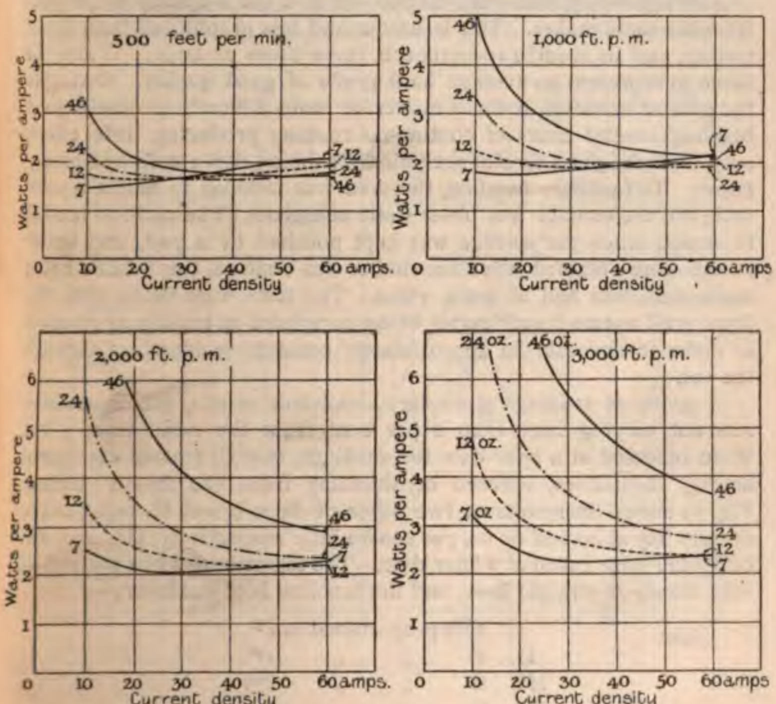


FIG. 3.—Total Losses per Ampere Collected, for various speeds, pressures, and current densities.

or 24 oz. at 32 amperes with nearly equal minimum loss, or a ratio of 1·3 to 1. Of course these conclusions will be modified by considerations of sparking, and it may not be possible at high speeds to use the most economical ratio.

Mr. Hobart ("Electric Generators") mentions 40 amperes as a safe limit, and 20 oz. as good practice, or a ratio of 2 to 1. If the curves of total losses are examined for the point corresponding to 40 amperes and 20 oz., it will be seen that there is not much wrong with these values, except at the highest speed, and here a lighter pressure and a greater current density might produce sparking. It is, indeed, satisfac-

tory to find that over a wide range of speed, pressure, and current density a low value of total losses can be obtained; but there is a heavy penalty if these limits are much exceeded. There is no advantage in using a low current density, and in order to obtain the best conditions over all loads of a dynamo, the current density at full load should be fairly high, a few well-designed brush holders being better than a number of cheap ones, especially at high speeds, where a low current density is extravagant.

Corresponding tests were carried out with the S quality of carbons from the same maker. This is harder and less graphitised than the X quality, and its specific resistance is three times as large. It may be taken to represent an average hard grade of good quality. Owing to the greater hardness, it was a matter of some difficulty to obtain good bedding, several hours of continuous running producing little effect, even after the most careful preliminary rubbing down with fine emery paper. To facilitate bedding, the area was reduced to half a square inch, but the contact was never quite complete. The carbons tended to scrape when the surface was kept polished by a pad, and better results were obtained when the rubber was omitted, the E.M.F. being more consistent and of lower value. The tests were taken with the drum well warmed, each series being completed as rapidly as possible in order to maintain an approximately constant temperature through the set.

A group of readings gave very consistent results, the successive sets not varying more than 1 per cent. from the mean value; but when repeated at a later date the readings, though equally consistent among themselves, differed considerably from the former values. Fig. 3A shows the results on two different days, I. and II. being taken on one day at 40 and 60 oz. per square inch respectively, III. and IV. being the same taken at a later date. The logarithms when plotted lie very closely in straight lines, and the function is of the form :—

I.	E is proportional to			$i^{\cdot 58}$.
II.	E	"	"	$i^{\cdot 56}$.
III.	E	"	"	$i^{\cdot 44}$.
IV.	E	"	"	$i^{\cdot 41}$.

On each day the curves agree fairly well, but some change in conditions has made a considerable alteration in the form of the curve. The mean of all the readings, some thirty sets in all, gives a curve of the form $E = k\, i^{\cdot 5}$, which is very different from that obtained with X carbons, and the difference is important in the process of commutation. More prolonged experiment is required on various qualities of carbon, but it is clear that the contact resistance does not vary much for a large difference in specific resistance, and that the law of variation of E to i is not the same for different qualities.

Reference must be made here to a paper by Professor E. Arnold*

* "Über die Untersuchung von Dynamobürsten," *Elektrotechnik und Maschinenbau*, July 29, 1906, abstract in *Electrician*, Vol. 58, 1906, p. 14.

which deals with the same subject. The materials examined were X and Z quality Le Carbone carbons, and except that the drum was of brass instead of copper, and that the E.M.F. was observed between a single brush and drum, the method of testing was similar. When a long period of time elapsed between each reading, so that the temperature of drum and brush was individual for each value of the current the E.M.F. for the two brushes reached a value of 1·1 volt for Z and 0·9 volt for X carbons at a current density of some 70 amperes per square inch, and as the current increased beyond this figure the E.M.F. rather diminished. We have not found this to occur in our experi-

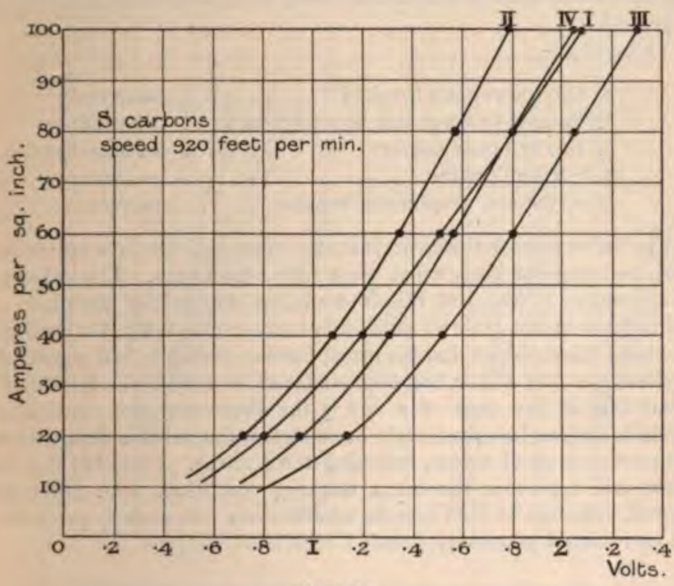


FIG. 3A.

ments, and while change of conditions often appeared to affect the readings, in the end, after repetition, little actual alteration was discovered. Professor Arnold also took sets rapidly, and obtained curves almost identical with ours. It is of interest that in these last readings he used a commutator instead of a drum.

Some other experiments and conclusions drawn from them by Professor Arnold will be discussed in the last portion of this paper.

II.—Friction of Carbon Brushes.

The friction was measured by reading the power absorbed by the driving motor when the brushes were on and off respectively, the readings being taken immediately after the E.M.F. readings at every

set. For convenience, the power wasted in friction has been expressed by the formula $W = \mu\, v\, P\, A$ watts, where—

v = velocity in feet per second.
P = pressure in ounces per square inch.
A = area in square inches of both brushes.

The co-efficient μ shows some variation, but the divergencies from the mean do not point to any modification of the above formula. The value obtained with the above-described brush holders and a polished surface, as a mean of 18 readings, was $\mu = 0{\cdot}00065$. Values were also obtained when other brush holders were used, which may conveniently be given here.

Value of μ—

1. Direct-pressure brushes ... ... $= 0{\cdot}00065$
2. Padded brushes and heavy holders ... $= 0{\cdot}00050$
3. Heavy brush holders ... ... $= 0{\cdot}00037$
4. Parshall's value ... ... $= 0{\cdot}00043$
5. Raymond, graphitised brushes ... $= 0{\cdot}00070$

The agreement between Raymond's value and the first one is fairly close, and possibly his brushes were softer than ours. The value with heavy holders is low, and this is doubtless due to the vibration. An examination of Mr. Moore's experiments, from which Mr. Parshall takes the above value, shows that his brush holders probably had a good deal of vibration, and hence the frictional loss is also low. It should be added that in the case of 2 and 3 the drum was not continuously polished, and, as has previously been stated, the carbon dust acts as a lubricant to a small extent, reducing the friction. Probably the value 0·0005 will represent the usual working conditions with fairly hard brushes, although at high speeds, where some vibration is probable, it will be reduced to 0·0004.

III.—Experiments with Lubricated Brushes.

It is a common practice to use some lubricant, which generally contains hard paraffin wax as the basis. To examine what influence the lubricating film exerts on the E.M.F., a set of readings was taken with the same brushes and holders as in Series I., but lubricating the drum with paraffin wax. It was not so easy to ensure uniformity of lubrication as uniformity of cleanliness, particularly with a solid or pasty lubricant. When first put on, the lubricant causes sparking and a rise in the E.M.F., and a consistent condition is obtained only after the wax has become softened and uniformly spread. Therefore continuous application was not possible, and some variations were inevitable in the quantity of lubricant on the surface. This made less difference to the E.M.F. than to the friction. The temperature of the surface affected the viscosity of the wax, and on a cold surface the effect was not satisfactory. This, however, is not likely to occur in

practice, and in the tests the drum and brushes were warmed up by a large current before readings were taken.

Fig. 4 shows a complete series taken at 2,300 ft. per minute, and it will be seen that the lubricant has produced very little change in either the shape or the value of the curve, until the pressure is reduced to 18 oz. Below 12 oz. the readings were irregular, and the E.M.F. rose rapidly, showing that there was imperfect contact. The speed exerts considerable influence on the E.M.F. Even at a pressure of 42 oz. there is a continuous change between 1,200 ft. per minute and 3,300. At 1,200 the E.M.F. was exceptionally low, whereas at 3,300 the value is nearly doubled. With a dry surface (Fig. 1) there was no change at all.

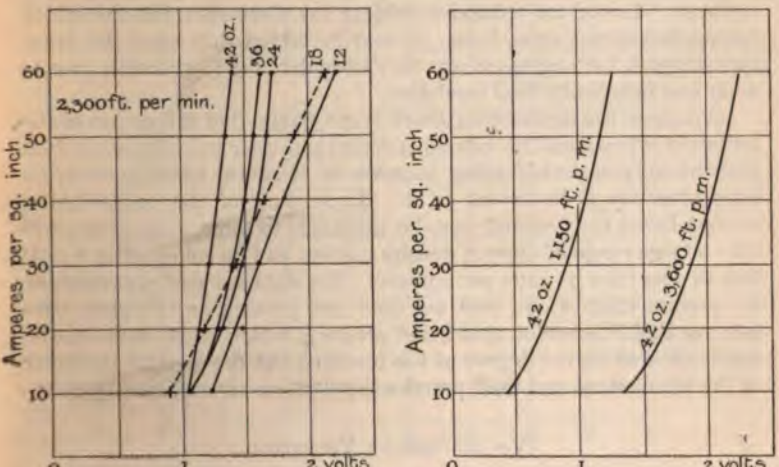


FIG. 4.—Tests with Lubricated Surface.

The frictional loss was determined at the same time, the following values being obtained :—

Pressure	12	18	24	36	40	42 oz.
$\mu =$	0·00013	13	15	074	065	10

or a mean value of 0·00011 watts per oz. per foot per min. per sq. in. The values given above show rather wide divergencies, due to differences in temperature and thickness of lubricant, but in all cases the value is much lower than that found with a dry surface.

Two special commutator lubricants were also applied, one a mixture of paraffin and graphite, the other apparently consisting of powdered paraffin with a little soapstone, coloured and scented with unimportant constituents. The results were much the same as those obtained with paraffin wax alone, though it is possible that with an ungraphitised brush the addition of graphite in the lubricant may be beneficial.

Several liquid lubricators were tried, applied continuously by a pad against the drum. Among them were light engine oil, paraffin oil, and toluol. None were advantageous. The thinner lubricants somewhat increased the E.M.F. and made little difference to the friction, while the engine oil rapidly clogged and increased the E.M.F. considerably.

The results of the tests with paraffin wax are remarkable, and were certainly not expected by the authors. The curve at 1,200 ft. per minute (Fig. 4) is the mean of six sets, all agreeing closely, so that no accidental error was possible, and it will be seen that the values are as low as any obtained with the dry surface. The lubricant, although an insulator of enormous resistance, permits the passage of large currents with absolutely no interference, and yet is present in sufficient thickness to reduce the friction to one-fifth of its value for dry surfaces. This is an attractive subject for discussion, but one which cannot be entered upon here. It may be added that when the drum was stopped, the resistance was very irregular, and it generally rose to what was practically total insulation.

Whatever the explanation, there is no doubt that the action of the lubricant is beneficial in reducing friction and wear and tear of brushes, without any counterbalancing increase in electrical losses, provided a little attention is bestowed on it. On account of the reduction in friction losses the pressure may be increased to some 30 or 40 oz., and over a large range of current density current can be collected at a total loss of less than 2 watts per ampere. The data are scarcely adequate for precise calculations, and we shall not attempt any formula connecting E.M.F., current, speed, and pressure, which would doubtless be much affected by the degree of lubrication; but the general character of the phenomena and their practical application are sufficiently clear.

IV.—Effect of Vibration.

The influence of vibration has already been noticed at high speeds. To examine this further, brush holders were made in which there was a considerable tendency to vibration—long arms pivoted at one end and possessing more inertia. The same brushes were used. There is no need to enter into details, as the tests only show what to avoid.

Fig. 5 shows the results. At 580 ft. per minute there was no vibration, and the values of E.M.F., even down to 12 oz., correspond closely with the previous values. At 1,600 ft. the effect is marked, at 2,300 still more marked, the increase showing at a pressure of 55 oz., while at 3,200 ft. the E.M.F. runs up to nearly 9 volts. Although the collection at this speed was not sparkless, it would scarcely have been deemed very bad on a dynamo. At these high speeds the collection is clearly almost entirely through the arc, as the E.M.F. is almost constant between 10 amperes and 40, and it is remarkable that the readings were exceedingly consistent, repeated sets not varying more than some 3 per cent.

The vibration was reduced by inserting pads behind the carbons,

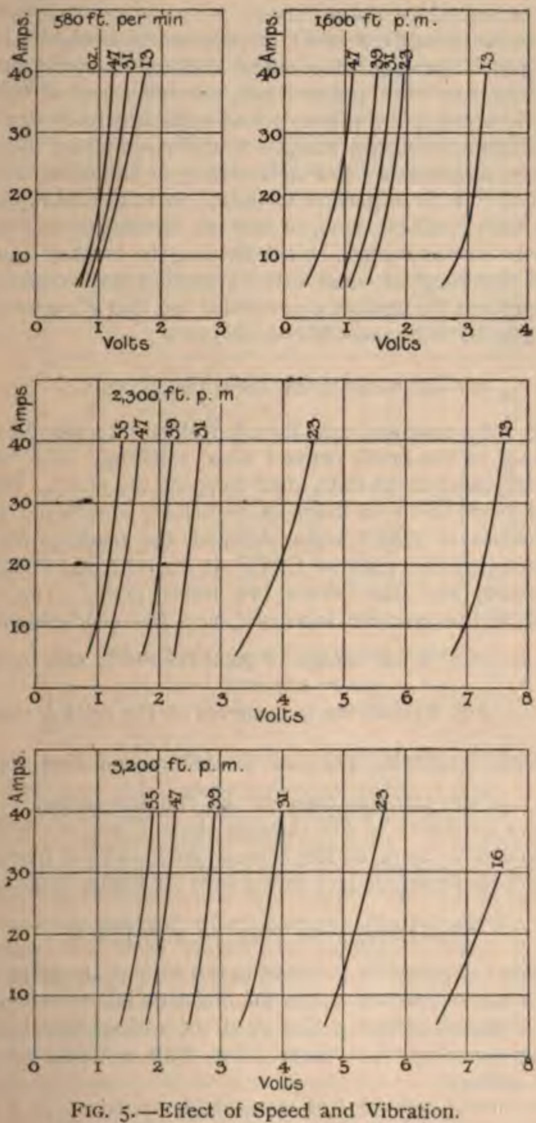


FIG. 5.—Effect of Speed and Vibration.

and a marked improvement was noticed. At 1,430 ft. per minute the E.M.F. was still normal, at 2,300 ft. it was normal down to 16 oz., but at 3,200 ft. it was barely normal even at 42 oz. It is scarcely necessary to reproduce these curves.

It will be noted that the effect of pressure is twofold. It reduces vibration, and it improves the actual contact independently of any vibration. As has been pointed out, the efficiency of collection is improved by keeping the pressure low, especially with dry surfaces; but the reduction must stop sharply at the point where the particular brush holders in question show a tendency to vibration, or losses will run up rapidly. It is important to notice that the E.M.F. maintains a high value with small currents, so that an increase in brush area will not eliminate the loss, unless, by subdividing the brushes, a probability is obtained that some of them will be making good contact. These results strengthen the opinion expressed above, that a few well-designed holders are better than a number of bad ones.

V.—Resistance of Brushes Standing.

Though not a practical condition, it will be interesting to examine the resistance of the brush contact when standing. This was determined during some of the tests, after stopping the drum. The current was raised from 10 to 60 amperes as before, and while occasional abnormal values of E.M.F. were obtained, the readings for the most part showed that the ratio of E.M.F. to current was constant for a given pressure, and that Ohm's law holds good. The resistance diminished as the pressure increased, and the conductance may be written $\frac{i}{E} = 0{\cdot}6\,P + 24$, though no great reliability can be placed on the formula. Fig. 6 shows the two curves of the ratio $\frac{i}{E}$ standing and $\frac{i^{0{\cdot}28}}{E}$ running, against the pressure, the latter producing considerably more effect on the standing than on the running values. With small currents the resistance of the running contact was the greater; but between 60 and 70 amperes the curves cross, and for higher current densities the stationary contact would have the higher resistance.

VI.—Effect of Time on the E.M.F.

To obtain some clue, if possible, to the curious shape of the curve E/i, some variations were made in the mode of taking readings, in the direction of ascertaining the E.M.F. at the earliest possible moment. For the previous curves were all taken with a liberal allowance of previous running.

1. The current was kept at zero, suddenly raised to a particular value, and the E.M.F. read as rapidly as possible.

2. The E.M.F. was read by a ballistic galvanometer and condenser when the current was switched on.

3. The E.M.F. was read by a ballistic galvanometer as in No. 2, but the galvanometer circuit was automatically opened a small fraction of a second after closing the main circuit, thus eliminating the effect of any subsequent change.

Readings were taken at 24 oz. pressure and 1,430 ft. per minute. To avoid cumbering this paper with too many curves, it may simply be stated that all three methods gave closely the same curve, and the mean of them all agreed almost exactly with the normal running curve at that speed and pressure. The curve of No. 3 method gave slightly lower values of E.M.F. than the others, but the difference was not sufficient

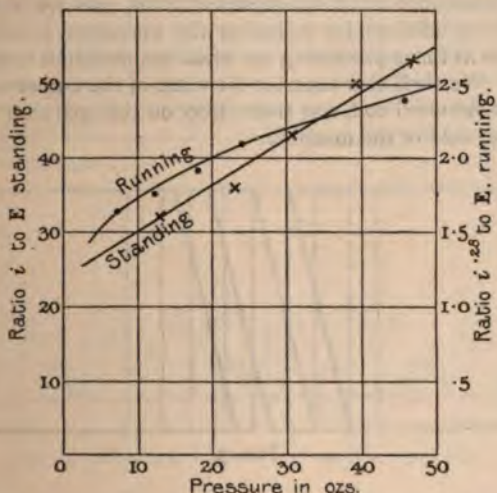


FIG. 6.

to bear any deductions. It may be taken, therefore, that the E.M.F. assumes this value in an exceedingly short space of time.

The form of the curve of E/i is strongly suggestive of the shape of the curve obtained when a glow lamp is heated, but the experiment with method 3 shows that any heating must be confined to an extremely thin layer of carbon, on account of the rapidity with which it takes place. There is also the difficulty that stationary brushes do not exhibit this phenomenon, but obey Ohm's law, so that the mere fact that the contact takes place at comparatively few points, with consequent enormous current densities for a short distance, does not seem a satisfactory explanation, for some similar effect should be shown in the stationary brush. We confess our inability to suggest a theory explaining the law of variation of the contact resistance.

VII.—DETERMINATION OF SPARKING E.M.F.

The next part of our experiments was devoted to determining the E.M.F. between the brush and the commutator segment at the moment

of separation, which we may call the sparking E.M.F., or the E.M.F. due to a sudden cessation of current in an inductive circuit, which tends to produce a spark at the point of separation. This has no connection with the reactance E.M.F. embodied in various formulæ, which deal with the value of L^{di}/dt before the break, and assume that the current has already become zero when the separation occurs. The sparking E.M.F. is therefore a measure of the failure of the machine to commutate its current in the correct manner. That this does not necessarily mean that the machine is commercially unsatisfactory is obvious from the fact that in forced commutation with fixed brushes the ideal procedure must be departed from, and we shall examine whether the conditions for reducing the inevitable sparking E.M.F. are the same as those producing an ideal commutation under the ideal conditions. We shall also examine the value of the currents circulating in the short-circuited coil, and their effect on the sparking E.M.F. and the magnetic field of the machine.

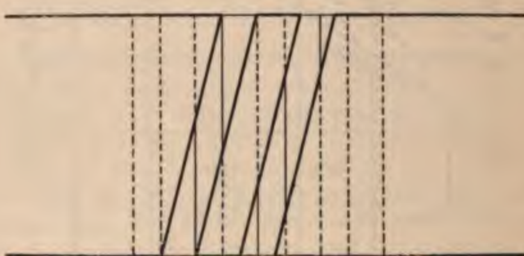


FIG. 7.

As interest is now particularly directed to the use of machines with commutating poles, this type was examined with the greatest fulness, and for comparison similar readings were obtained from a simple machine.

The machine was a 15-h.p. 4-pole enclosed motor with two-circuit wave-wound slotted armature. At 460 volts with the full exciting current it ran at 550 revolutions per minute. The armature has 1,384 bars and 173 commutator segments, with four turns per section, 29 slots with three outward and three return sections in each slot. The reactance E.M.F. calculated by Hobart's formula, if the effect of the commutating pole is neglected, is 3·5 volts. The four brush arms each carry two brushes 1 in. long and $\frac{7}{16}$ in. broad. The commutating poles had a narrow pole face $\frac{7}{8}$ in. broad, but both main and commutating poles slanted across the teeth, so that the total span was 2 ins., or nearly two slots and two teeth, as shown in Fig. 7. The makers were the Morris Hawkins Company.

The E.M.F. was measured on a high-speed falling-plate Duddell oscillograph, which was connected as a voltmeter between the brush and a trailing spring attached to the back of the brush, and separated

from it by a sheet of mica $\frac{3}{64}$ in. thick. While both brush and spring were touching the same segment the oscillograph registered the fall of E.M.F. from carbon to segment, but as the carbon left the segment, the induced E.M.F. caused a current to flow through the instrument to the segment just left. As the spring traversed the mica between the segments the circuit was broken and the deflection fell to zero, but in many cases the brush bridged this, and the E.M.F. merely dropped to the first value. The use of a circuit in parallel with the break tends to reduce the E.M.F., but in most cases the resistance was some 160 ohms, so that a very fair idea of the sparking E.M.F. was obtained. The direction of the E.M.F. indicates whether the current is from brush to segment or the reverse, and indicates whether the machine is under-compensated or whether over-compensation has set up a circulating current in the reverse direction. In the following diagrams the direction above the zero line indicates an E.M.F. in the direction of the main current, or under-compensation.

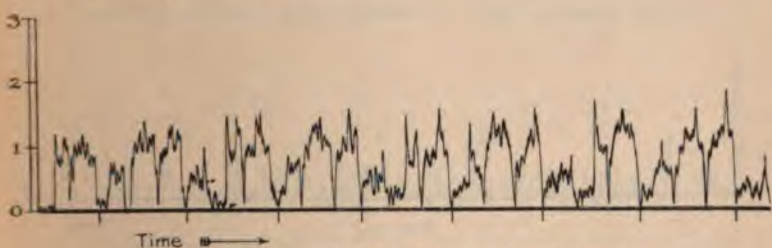


FIG. 8.—Running Light: Slow speed (150 revs.).

As the speed which was used in some of the tests, 870 revolutions per minute, involves 2,500 commutations per second at the brush, the resolving powers of even an oscillograph were severely taxed, and the waves are unavoidably much crowded. Further resolution by increasing the speed of falling was prevented by the weakening of the photographic trace. But for the most part it was only the height of the wave that was required, and a large number of waves gave a better value of the average E.M.F. required. In order to make sure of the action of the apparatus a slower speed was adopted at first. Fig. 8 shows the action at a speed of 150 revolutions when the motor was running light. The waves are quite distinct, falling into groups of three, the number of coils in a slot. The E.M.F. rises as the slot travels to the approaching pole piece out of the field of the commutating pole. The zero values show the mica separators, after which there is a rapid rise to the E.M.F. between brush and segment, followed by another rise as the brush leaves the segment. There is much irregularity of detail, and it can scarcely be expected that the currents will repeat with exact regularity. There are six ripples on each wave for which more than one cause can be suggested, and it would be

unprofitable to follow them out in detail. The machine is under-compensated, and the sparking E.M.F. rises through the group of three coils as the compensating pole becomes weaker. The maximum E.M.F. is only 1·7 volts, as the speed is very low, the E.M.F. between brush and segment being about 0·8 volts. The compensating poles were excited with more current, the motor still running light,

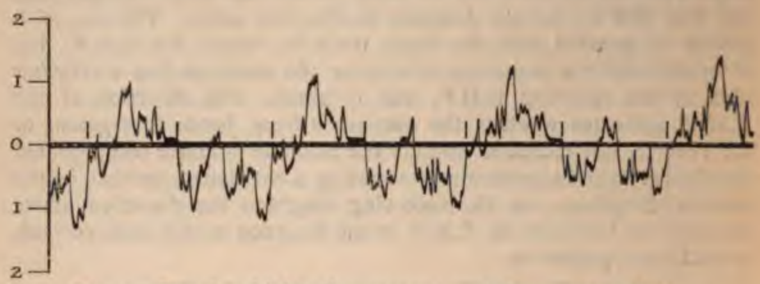


FIG. 9.—Running Light: 6 amperes round Commutator Poles.

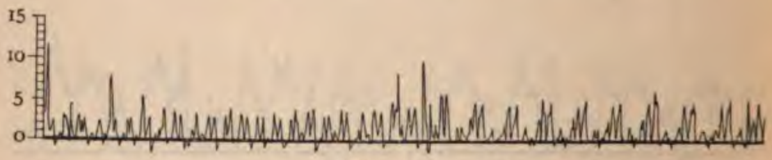


FIG. 10.

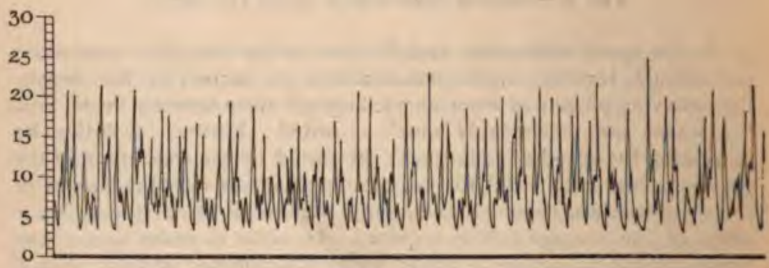


FIG. 11.

and the curves showed a gradually increasing over-compensation. Fig. 9 was taken with 6 amperes, the armature current being 2 amperes, and much of the E.M.F. is now in the reverse direction. In fact, the waves group themselves in two sets of three, and in alternate sets the current is entirely in the reverse direction, the other parallel brush arm probably taking the driving current during this interval.

Repeating with different loads and the proper compensating

current for each, the E.M.F. showed always under-compensation. This will be examined in more detail at a higher speed.

The motor was intended for a large range of speed, and we may therefore pass over the tests at normal speed, to examine its behaviour under the more trying conditions of a high speed and a weak main field. A speed of about 870 revolutions per minute was arranged, and Fig. 10 gives the light load values with an E.M.F. of 6 volts, Fig. 11 the full load values, in which the E.M.F. rises to 20 volts. The motor is considerably under-compensated, but nevertheless the brush dealt with this high E.M.F. with very little visible sparking, the mass of cold copper and the speed of separation doubtless contributing to this

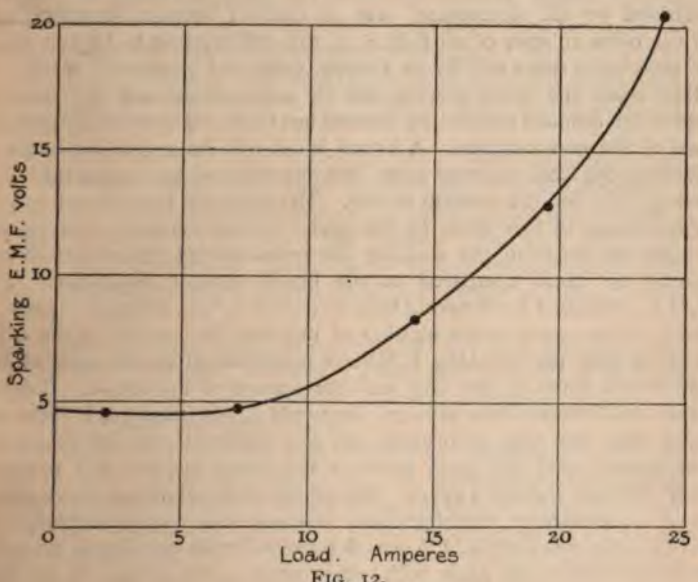


FIG. 12.

result. Fig. 12 shows the set of values obtained at these and at intermediate loads.

Further tests were made with no current round the compensating poles. The sparking was considerable, and even on half load the sparking E.M.F. rose to 32 volts. By shifting the brushes the current was taken up to full load with a sparking E.M.F. of about 32 volts. This is, of course, an abnormal condition, for the commutating poles were not removed, and their presence over the coil under the brush produces a strong magnetic field, due to the armature, which passes through the coil and naturally produces large circulating currents. The value of these will be examined later, and their magnitude easily explains this great E.M.F.

It had been in the mind of one of us that a brush with a trailing

edge in the form of a very blunt V would act beneficially in reducing the current before the final break, and this seemed a suitable method for testing the device. The result, however, was not favourable, for the sparking E.M.F. was very little diminished. We have not followed up this method of testing different qualities and shapes of brushes, but it appears to be capable of much useful employment.

The foregoing experiments indicate that where the conditions assumed in the reactance formulæ are not fulfilled, it is important to determine whether the sparking E.M.F. will be large. At the same time they show that it need not be very small. In all cases of forced commutation, *i.e.*, where the current does not die down to zero by the action of a suitable E.M.F. in the short-circuited coil, but is merely throttled by the decreasing area of contact between segment and brush, often in spite of an E.M.F. in the coil tending to keep it up, in all such cases there will be an abrupt break and a possible spark. In many cases the main current will be unimportant, and the value to which the induced circulating current has risen will control the smoothness of the commutation. A broad brush will be a positive harm, as allowing the coil to come more into the field of the wrong pole, and giving time for the current to rise. The value of the current will be proportional to this field, to the speed of the machine, and to the number of turns in the coil, for the resistance of the coil itself will usually be small compared to the brush contact resistance. The E.M.F. produced by breaking this current $= L\,{}^{di}/_{dt}$, where L is proportional to the square of the number of turns (if they are all in the same slot), so that the sparking E.M.F. is proportional to the cube of the number of turns in the coil and the square of the speed. But the whole coil between two adjacent segments is not involved, if there are more than two sets of brushes, for the different sets break circuit successively, and the parts between the other brushes will readjust their currents without a spark. Hence the commutation is more easily forced in multipolar machines than in those with only two poles. To test this, the sparking E.M.F. was determined with one brush on each of the four arms, and again with two brushes on two arms. In the first case the E.M.F. was 8 volts, in the second 15 volts, or nearly twice as great, the length of bar in which current is stopped being twice as long.

The same result is shown in the oscillograph curves of the current in the short-circuited coils shown by J. K. Catterson-Smith.* Using several brushes, he found the current change its value by successive small steps instead of one large one, from which it may be concluded that there will be less tendency to spark.

The above estimation of the sparking E.M.F. assumes that the decrease of current, due to the diminishing area of the segment in contact with the brush, is sufficiently rapid to produce a sensible E.M.F. in the coil. But if this is not the case, and the self-induced E.M.F. does not appreciably influence the current, then the sparking E.M.F. will be proportional to the current at the moment of breaking,

* *Journal Institution of Electrical Engineers*, vol. 35, 1905, p. 430.

to the coefficient of self-induction, and to the speed or Lni, which is similar to the reactance voltage except that i has only a remote connection with the armature current, and depends on the resistance of the brush contact and the E.M.F. induced by the stray field. If it is possible to find a quality of carbon in which the resistance is fairly independent of current density the use of such brushes should sensibly decrease sparking.

For forced commutation it is preferable to have as small a magnetic field as possible in the interpolar space, to diminish the circulating currents. Hence a narrow air-gap and a large interpolar space are beneficial. Distortion of the field is then of little consequence, and weakening or even reversing the pole tip will not matter, so long as the field from the strengthened pole tip does not come down on the coil. The ideal machine with commutating poles does not experience forced commutation, but with incorrectly adjusted poles some forcing of the current is inevitable, and the larger inductance will cause sparking.

As a comparison with the foregoing curves, some tests were made on a simple motor without anti-sparking devices. The machine was a 6-pole, 55-h.p. motor made by Mavor and Coulson, running normally at 500 revolutions with 460 volts on the brushes. The pole shoes were square and the air-gap rather small for the size of the armature, being 3·9 mm. The slot breadth was 9 mm. and the breadth of the top of the tooth was 10 mm.

There were 282 turns with 142 commutator parts, or two turns per coil, wound in a two-circuit winding. There were six pairs of brushes, each of $1\frac{1}{4}$ sq. in. area. The nominal maximum current density was therefore 15 amperes per square inch.

As this machine was fitted with additional testing devices, which included a pair of spring contacts on the commutator, the contact resistance of the brush was eliminated by setting the two contact makers a short distance apart, the one just in front, the other behind the trailing edge of the brush. When they both touched the same segment the E.M.F. fell to zero, and the peaks of the curve read the E.M.F. in the coil as it left the brush and received the main current from the new side. The potential brushes were set at a distance little more than that of the breadth of the mica between the segments, so that the reading just included the spark and no more.

Setting the brushes in the most favourable position for running light, the curves gave an E.M.F. of 3·5 volts at this load, which rose to 4 volts at half load. It was found that if the potential leads were set with a very small gap, so that the trailing contact moved on to the mica after a contact lasting only $\frac{1}{32}$ in., the E.M.F. curve rose abruptly and dropped to zero (Fig. 13). On increasing the time of contact, the zero drop was only momentary, with a rapid rise as before, but there followed a momentary dip as the sparking E.M.F. ceased, and a further rise showed the E.M.F. induced in the coil by the stray field (Fig. 14), which was cut short by the contact coming on to the mica. The first

rise in Fig. 14 is the same as the rise in Fig. 13, and that this is smaller than the E.M.F. induced by the stray field shows how thoroughly the current is controlled by the diminishing area of brush contact.

The following values of the first rise were obtained :—

		Armature Current.		Sparking E.M.F.
Brushes set for light load	...	7 amp.	...	3·5 volts.
,, ,, ,,	...	23 ,,	...	3·5 ,,
,, ,, ,,	...	46 ,,	...	4·0 ,,
,, ,, 46 amp.	...	46 ,,	...	3·0 ,,
,, ,, 46 ,,	...	7 ,,	...	2·0 ,,
,, ,, 37 ,,	...	7 ,,	...	2·0 ,,
,, ,, 37 ,,	...	37 ,,	...	3·0 ,,
,, ,, 37 ,,	...	75 ,,	...	3·5 ,,
,, ,, 75 ,,	...	75 ,,	...	2·5 ,,

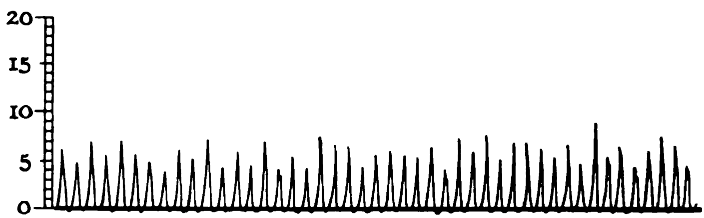


FIG. 13.

[redacted]

Additional readings were taken by the earlier method, from brush to contact spring, under the same conditions as those for Figs. 13 and 14. The value of the first rise, so far as can be determined, is much the same as before, but the inclusion of the contact resistance has complicated the curve. During this time the zero value was rather changing, and the value of the contact resistance drop is probably exaggerated.

Without claiming any great exactness for these numbers, it is evident that the E.M.F. of self-induction, which tends to produce a spark, is very small in this machine, and variation of load and position of the brush does not create large changes. The machine has a very weak field in the space between the poles, for the air-gap is small and the distance between pole pieces is large. The number of turns per coil is small, the complete coil is divided into three parts by the brushes, and the speed is not high. There are therefore all the conditions for a very moderate sparking E.M.F. or good forced commutation, although the value of the reactance voltage is not especially low, being 3¼ volts. A further examination into the process of commutation of this machine will be considered below.

VIII.—Distribution of the Magnetic Field, and Influence of Circulating Currents in a Motor with Commutating Poles.

Readings of the magnetic field of the machine were taken by means of a search coil on the armature, connected through slip-rings to the oscillograph. Fig. 16 shows the field due to the main poles, and Fig. 17 represents the influence of the commutating poles, with the full current round the coils. Exciting these as for a motor, a series of curves was taken with increasing currents, and the resulting total areas, representing the magnetic flux, are shown in curve A (Fig. 18). It is clear that by half load the commutating poles are saturated, and the rapid rise of the sparking E.M.F. in Fig. 12 has already indicated that this was probable.

It has been shown by Messrs. Walls and Smith (*Electrician*, April 6, 1906) that in a stationary armature the magnetic flux of the commutating poles is independent of that due to the main poles, and it may be regarded as crossing the latter at right angles. These curves indicate that this is the case, for the effect is merely a hump on one side, the main portion being unaffected. The brushes were put down in the central position, and similar curves taken at different loads, the load current exciting the commutating poles. The brushes being central, there is no demagnetising action exerted directly by the armature current. The commutating poles are demagnetised by the magneto-motive force of the armature. There are powerful currents in the short-circuited coils, which produce violent fluctuations of magnetic field under the commutating poles, and which would tend to magnetise the main field. But the areas of the curves, which are plotted in line C, are almost constant, showing that the increased

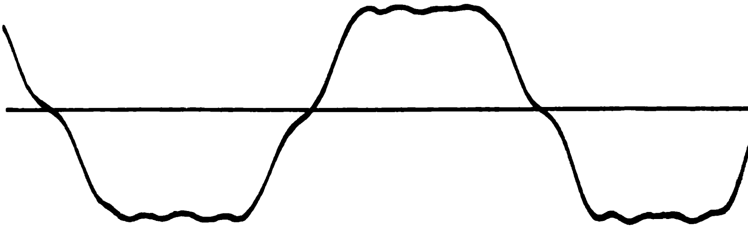


FIG. 16.—Armature driven externally. Brushes lifted. No current round Commutator Poles.

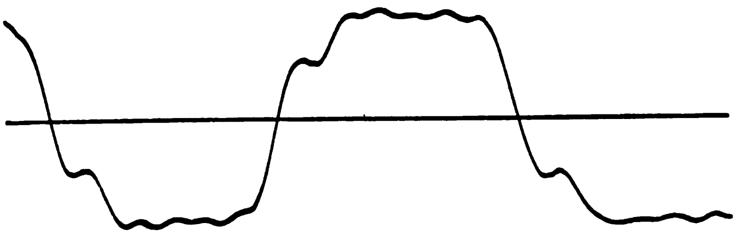
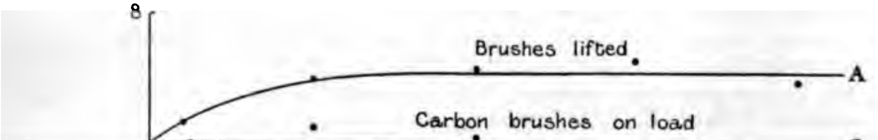


FIG. 17.—Armature driven externally. Current round Commutator Poles 28 amperes (full load current). Brushes lifted.



[redacted]

reluctance of the main circuit due to the distortion of the field has counterbalanced the increased M.M.F. Figs. 19, 20, 21 are examples of the curves obtained at light load, half, and full load respectively. The circulating currents steadily increase with the increase of load, as the reversed field under the commutating poles increases.

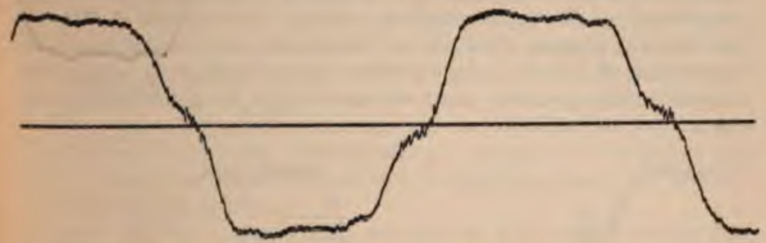


FIG. 19.—Field on Load. Running light: 2 amperes.

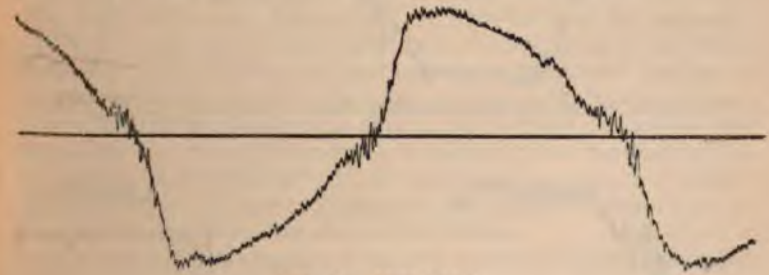


FIG. 20.—Half Load.

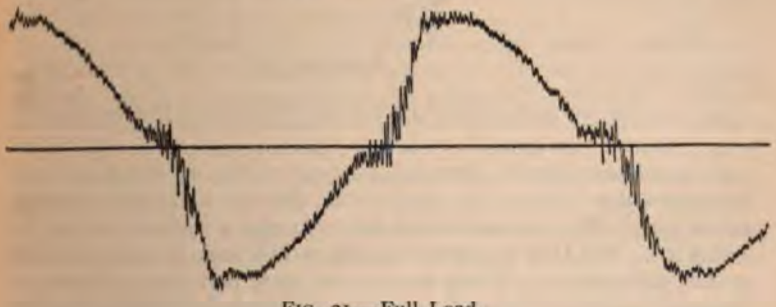


FIG. 21.—Full Load.

The same tests were carried out with copper brushes, but the sparking prevented the trial of heavy loads. The results were much the same as with carbon brushes, but the circulating currents were greater, as would be expected.

To examine the influence and magnitude of the circulating current apart from armature distortion, a series was taken with the armature

running light and a separate current round the commutating poles, excited as for a motor. This gave a gradually increasing over-compensation. Figs. 22, 23 give the results with 14 and 28 amperes, and the magnetic flux for each is plotted in curve B, Fig. 18. The commutation being over-compensated, the short-circuit currents are in the reverse direction, and the total field is considerably diminished. As a check on the figures derived from the areas of the curves, the line B, was plotted from values calculated from the speed of the motor, and it will be seen that the correspondence is fairly close. Copper brushes gave very much the same results, the points lying practi-

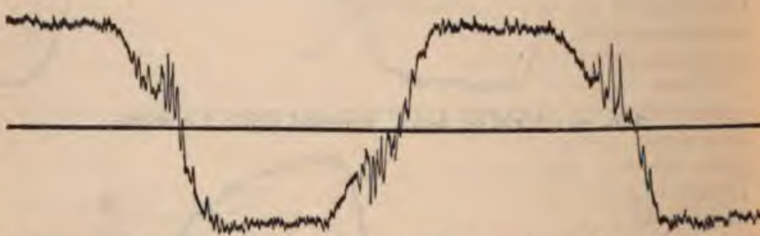


FIG. 22.—Current round Commutator Poles 14 amperes: Brushes down.

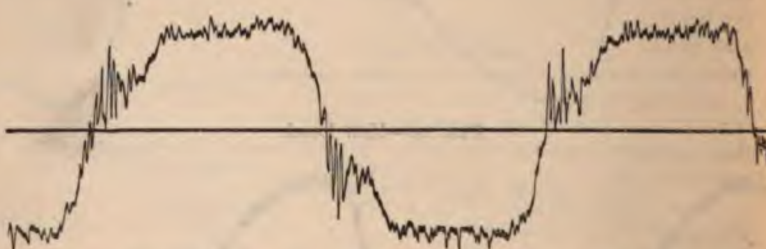


FIG. 23.—Current round Commutator Poles 28 amperes: Brushes down.

cally on the same line B. Examining the curves (Fig. 23) in detail, it will be noticed that in places the short-circuit currents completely demagnetise parts of the commutating poles, from which their value may be estimated. The M.M.F. across the gap at full load is 1,700, and the ampere-turns 1,350. There are for the most part two coils short-circuited under the brush, containing 24 turns, so that the current must amount to some 60 amperes, and its maximum value is probably much more in one of the coils. Their influence on the main field was determined by measuring the area, and the demagnetising effect was found to be some 10 per cent. Calculating from the characteristic curve, this represents 350 ampere-turns, the average value given by this method being much less than the maximum value given above. No doubt such large currents, several times the normal current, are not

to be anticipated in a well-designed machine, but it is clear that there are great possibilities if the design is incorrect. The sparking E.M.F. obtained under the same conditions was found to be very large.

An attempt was made to use copper brushes, but the sparking and disturbance of the field were so violent that the motor began to hunt, and readings were impossible above a magnetising current of 7 amperes.

Another series was taken with the commutating poles excited as for a dynamo, *i.e.*, in the wrong direction. With the brushes lifted, so that no disturbance could take place, the values of line A were obtained again, as shown in D.

The brushes were then put down and the machine run as a motor. Line E gives the results, corresponding to line B. The short-circuit currents magnetise the main field, and the total flux rises as the current round the poles increases.

These curves in Figs. 19, 20, 21, confirm the deduction from the curves of sparking E.M.F., that the commutating poles are not sufficiently powerful, and they further show the great risk of such poles when incorrectly designed, for the reluctance of the magnetic circuit, in the air-gap of which the short-circuited coils lie, is small, and a small want of balance of magneto-motive forces will produce a considerable magnetic field. By using such poles the maker expects to be able to allow a large number of turns in the armature coil, and the liability of sparking is increased, in addition to the disadvantage of the heating effect of the short-circuit currents on armature commutator and brushes, and the loss of power. In the simple machine there is much less danger of unsatisfactory commutation.

It is only fair to the makers of the motor to state that this particular machine was one of the first they had made, and it should be added that, notwithstanding the errors revealed in these tests, the motor runs with little sparking even at high speeds.

The risk of using too broad a brush is also clearly brought out. These should be as narrow as possible, in order to curtail the time during which extra currents can be produced. Whether under or over compensated, the motor will tend to spark if the brush is broader than is absolutely necessary, and as we have seen in the first part of the paper, a high current density makes little difference to the commutator losses.

In Mr. Creedy's paper (*Journ. Inst. El. Eng.*, April, 1905), among experiments on an alternate-current series motor, is one on a direct-current series motor, in which he measures by a falling-plate oscillograph the fluctuations in the magnetic field and the armature current, finding ripples in the magnetic field and the current. He attributes this, in part at least, to variations of brush resistance, and with a series motor such an explanation is possible ; but it was much more probably the same action that has been noted above. Mr. Punga, in the discussion of the paper, suggests that short-circuit currents may be the explanation.

IX.—Measurement of Current in the Segment under the Brush.

The foregoing experiments gave only indirect information concerning the current flowing into the brush from a segment, and so far as we know, no direct measurements of the rise and fall of the current in a segment have been made. As the current in a segment endures for an extremely short space of time, either an oscillograph or a contact-maker must be used. The resistance of the lug itself is too small to permit of a reading of the fall of potential, but a resistance inserted for the purpose would tend to divert the current into adjacent lugs also in contact with the brush. Accordingly three consecutive segments were provided with a resistance of 0·018 ohms, and readings were taken from the central one. The current is then unaffected, except that the total armature resistance is momentarily increased by some 5 per cent., which will not sensibly influence the result. Currents in the short-circuited coils will be reduced, but the dimensions of these will vary so much between one machine and another, and with the width of the brushes, that their exact value in the particular motor examined is not of great importance. The resistance was arranged primarily for use with an oscillograph, which method was abandoned, and it was unnecessarily large for the method finally adopted.

The contact-maker method consisted in charging a condenser with an E.M.F. at a particular instant by means of a pin and spring. On the terminals of the condenser was a galvanometer, which with a very small consumption of the charge gave the E.M.F. The loss of charge was only 6 per cent., or the average E.M.F. 3 per cent. below the value to be measured. The readings were standardised in two ways: (1) by placing a standard cell directly in the galvanometer circuit; (2) by placing the cell in the contact-maker circuit in lieu of the potential to be measured. The two readings agreed to 0·2 per cent., showing that no errors crept in at the slip-rings. For measuring the current in the lug, potential leads were taken off to a slide-ring and to a contact pin, and from these to the condenser. The E.M.F. between brush and segment was read in the same way, the pin being fixed in the segment, and a change-over switch brought either into action. The positions of the contacts were adjusted to give readings at nine points, dividing the distance through which the segment was in contact with the brush into eight equal parts. The whole circuit was carefully tested for leakage and found to be perfectly sound.

The machine examined was the 55 H.P. motor previously used. Although this has six poles and three brush arms in parallel, it was thought desirable to avoid complications, and only one brush arm was employed. Otherwise nine lug resistances would have been needed, and three simultaneous readings of current by three complete sets of apparatus. Though much interest would attach to the determination of the respective currents in the three parallel circuits, this part must be left to the future.

The use of a single brush arm made advisable a restriction of the current to half load, although with the brush in the most favourable position a greater load could have been carried. The brush area was 2 ins. axially and nominally $\frac{3}{8}$ in. circumferentially, which was reduced actually to $\frac{5}{16}$ in. The brush width was barely more than the width of a segment and mica strip, so that not more than two segments were active together. The brush pressure was 40 oz. per square inch, the high pressure rendering steady readings more probable. The full exciting current was used, and a pressure of 460 volts gave a speed of 480 revolutions per minute, which was maintained all through. The brush examined was the negative, current passing from segment to brush.

Readings were taken with the brushes central, set back, and set forward, with currents of 7, 25, and 45 amperes at each position. Repetitions of readings showed some changes in the form of the curve, which would be to some extent influenced by changes in the brush contact, and as each set of readings occupied about an hour, it is probable that such changes occurred even during a single set. The most consistent examples are shown in Figs. 24, 25, and 26. [In Fig. 24 the current is 35 amperes, not 25.] It will be noticed that the current does not start until after the first division, due to the leading edge of the brush being slightly bevelled, as was found afterwards. The upper curves show the three currents, the lower curves the corresponding E.M.F.s between brush and segment, and the central diagram gives as ordinates the contact area between brush and segment at each point. For a short space in the middle the whole segment is in contact with the brush, reducing to zero on either side, where the current begins or ceases.

The first rise of the current is extremely rapid, amounting in the curve 45 in Fig. 24 to a rate of increase of 200,000 amperes per second. If this is multiplied by the inductance of the coil in which this change takes place, the result is an E.M.F. of 5 volts, the counter E.M.F. in the short-circuited coil. Referring to the E.M.F. curve, it has a value in the coil just before contact of 7 volts. This is the E.M.F. due to the leakage field. The E.M.F. drops promptly to about 1 or $1\frac{1}{2}$ volts, the rest of it being used to overcome the counter E.M.F. in the coil, so that the slope of the current curve is closely in accordance with this E.M.F. In Fig. 25 the E.M.F. and rate of increase of current are almost as great, but in Fig. 26 the E.M.F. is very small, and the rate of rise of the current is much slower. But this E.M.F. is not the only cause of the current entering the new segment, for in that case the rate of rise would be the same for all currents. The resistance of brush contact in the previous segment forces the current into the new one with an E.M.F. which increases with the current, and hence the rate of rise is greater, the larger the current. In Fig. 24 this effect is small, but in Fig. 26 it is the principal factor, and the rate of rise differs markedly for the three currents.

Examining the next parts, it is notable that the current has not only

reached its maximum before the full contact, but has even begun to fall either before or soon after full contact is attained. On light load there is an excess of reversing E.M.F., which causes a reverse or circulating current, large in 24 and small in 26. In the former it is

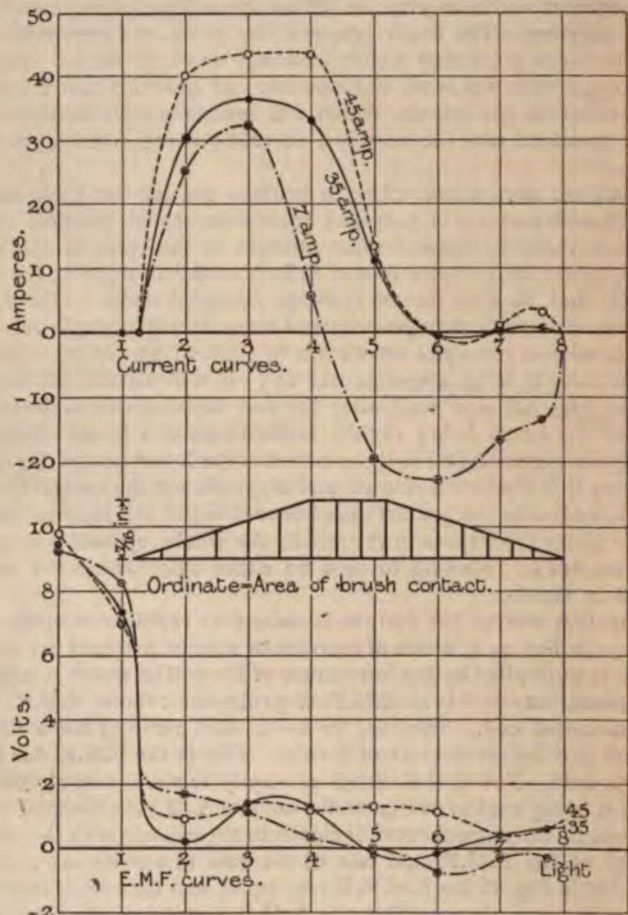


FIG. 24.—Current and E.M.F. in Segment, Brush behind Centre.

dying away slowly, as the coil moves out of the field, when the diminishing brush area cuts it off abruptly, with a corresponding rise in the E.M.F. curve. With 35 and 45 amperes the current does not rise again to any appreciable extent, and commutation is evidently perfect. Owing to the distortion of the field by the larger currents, there is a slight reversal of the field, causing the current to start in the wrong

direction, until cut off by the brush resistance. The phenomena in Fig. 25 are very similar on a reduced scale. In fact, up to half load this central position of the brushes is better than the previous one. Probably an intermediate point between 7 and 8 would show the half-load current rising again, as indicated by the E.M.F.

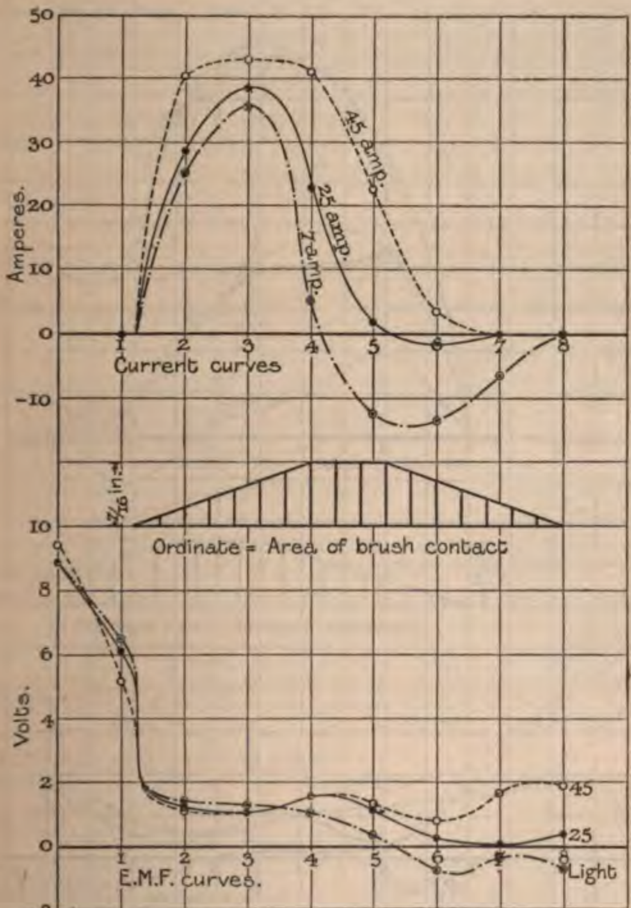


FIG. 25.—Current and E.M.F. in Segment, Brush in Neutral Position.

Fig. 26 shows the effect of insufficient reversing E.M.F. at first, which does not matter, and of the wrong E.M.F. at the end, which is more important. Without this E.M.F. the current of even 45 amperes would evidently have risen and died down by the action of the brush resistance alone, finishing easily by the end of the contact. It may be

of interest to say that, taking t as the half-time of contact, and R as the resistance of coil, lug, resistance, and brush contact (at the full), the product Rt is twice the inductance of the coil, a condition which on some theories of commutation should insure satisfactory results. In the present instance the E.M.F. from the fringe of the approaching

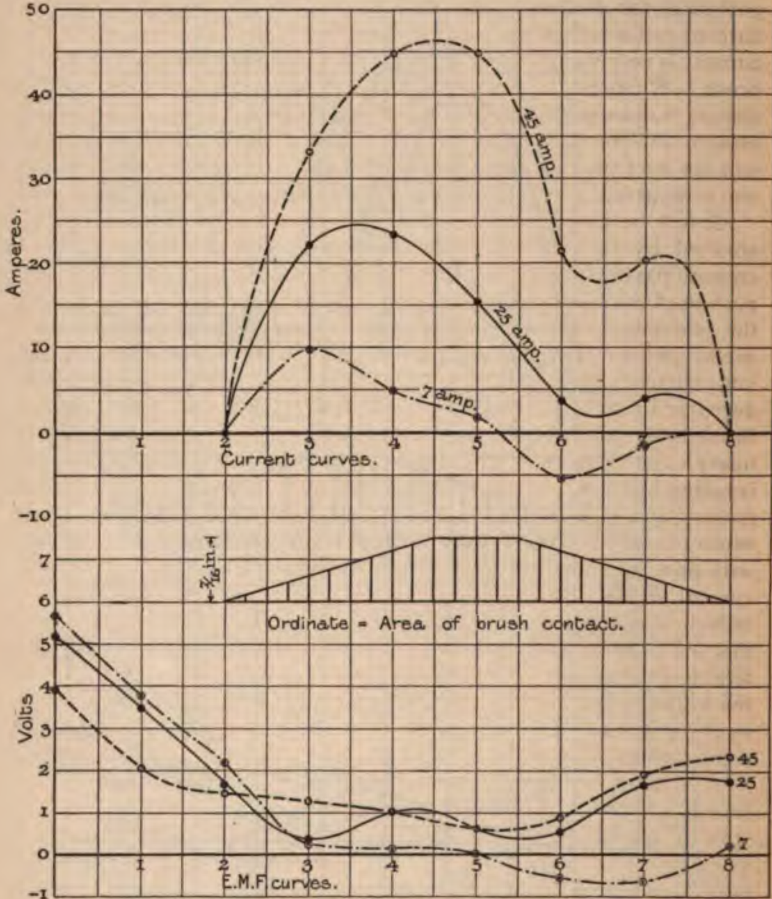


FIG. 26.—Current and E.M.F. in Segment, Brush in front of Centre.

pole keeps up the current, which is abruptly brought to zero by the increasing brush resistance, the E.M.F. rising to correspond.

The E.M.F. curves during contact show that this varies between 0·5 and 1·5 volts, which may be taken as about 1 volt. The current density is about 90 amperes per square inch with the 45-ampere curves when there is full or nearly full contact, rising to 150 amperes in the earlier

parts of the curve. This corresponds fairly well with the values given in Part I., for the brushes resembled the S quality, and this value is for one brush only. The current density at the leading edge is very high, falling rapidly until at the trailing edge it is in many cases zero or in the reverse direction. But over the first third of the brush, where most of it gets through, the density is fairly uniform. This is of some importance, for Professor Arnold (*loc. cit.*) has shown that when the current varies with great rapidity the ratio of instantaneous E.M.F. to current is nearly constant, and he applies this to the case of a dynamo brush. Whatever the value of the resistance may be under these conditions, it does not apply quite so strictly to the case of a brush as he assumes. Even at the trailing edge with a reversed current, the value will not fluctuate very much, unless the brush is even narrower than the one used here.

It will be noted that the current density under the brush is little affected by the circumferential size of the brush. The whole line current passes through the leading segment, even though the larger portion of the brush is touching the segment behind. We may recall the statement made in Part I., that excessive brush area serves no useful purpose, and we see that the case is really stronger against low current density than appeared before. For the anticipated decrease in the E.M.F. will not be obtained, while the idle part of the brush is at best wasting power in friction, and may also be the seat of heavy circulating currents. This machine, for example, in these tests is taking half load with one-ninth of the brush surface supplied by the makers, and we have taken it up to two-thirds of full load with a single brush of one-eighteenth of the full brush area in a special brush holder, with no sparking and with a pressure of 30 oz. per square inch. The current density must have been very great, and possibly this extreme reduction of brush area was not economical, but it is instanced to show that high current density alone will not cause sparking. In fact, it will tend to increase the brush resistance effect in forced commutation at the trailing edge, and to lower the sparking E.M.F. The only risk lies in overheating, if sparking should occur, for it will be concentrated over a shorter line with less cooling surface.

The curves of current were obtained with very steady readings, though successive tests showed some irregularity of outline. But in spite of probable changes during a single set of readings, the mean value of a succession of waves gives a height closely equal to the line current. The curves of E.M.F. were not so reliable, and it is extremely probable that the contact resistance at any one point under the brush will fluctuate, causing a corresponding fluctuation in the E.M.F. There are scarcely enough points for accurate plotting of the bends, but an increase in their number would have protracted the duration of an experiment, and would have increased the probability of change of conditions. There is a certain uniformity in the undulations which tends to show they are not simple irregularities, but an analysis of the current density and E.M.F. at each point would not be safe.

While the results of these last tests do not bring out any new phenomenon which has not been, or could not be, conjectured beforehand, they emphasise the fact that the leading edge of the brush is really the important part from the current-carrying point of view, and that the trailing edge should be reduced to its narrowest limits. The leading half should be of the best and most heavily graphitised carbon, and possibly the metallic impregnations sometimes used would be still better. But the trailing half should have a much higher resistance, preferably obeying Ohm's law as nearly as possible. The old carbon-fronted gauze brushes carry out this idea, but a single composite brush of uniform wearing qualities throughout will be more easily applied, and will require less attention. There is no need for several laminations, for these circulating currents cannot get through the leading part of the brush, and low resistance laminations at the trailing half will be harmful. The high resistance at the trailing edge will force the current to the new segment in front, which is all that is required.

The second point to which attention may be drawn is that the brush has far more power to commutate the current than is usually believed, at least by writers on dynamos. A low inductance is necessary, but a uniform high contact resistance is not advantageous, for while it hastens the fall of current in the trailing segment it also checks the rise in the new one. A reversing E.M.F. is desirable, but not necessary, as is shown by Fig. 26, where commutation takes place without its aid, and it must be noted that, while the machine is only on half load, yet it is working with only one brush arm, and all three coils are commutated at once. The conclusions drawn from the curves of sparking E.M.F. are therefore substantiated, that the less stray field there is the more safely the machine will commutate. So long as the armature field is kept away from the short-circuited coil, the strength of the main field may be reduced to any limits. If the armature coils have reasonably small inductance, then a wide interpolar space, a narrow air-gap, good brush holders and brushes not too broad, will hardly fail to produce sparkless commutation and a cool commutator.

In conclusion, we wish to express our thanks to Mr. W. G. Griffith and to Mr. H. J. Ireland for their assistance in the work on the resistance of brushes, and to acknowledge our gratitude to the Carnegie Trust for a grant in aid of this research. Their bestowal of a research scholarship has given leisure to one of us for the somewhat laborious experimental work which has been entailed, and the machines used in the tests form part of the new equipment of the electrical engineering laboratory in the Heriot-Watt College, to the purchase of which they subscribed a generous portion.

Discussion.

Mr. Mavor

Mr. H. A. Mavor: This investigation on commutation has been much required. We have here not only a very careful scientific investigation of the subject, but we are presented with valuable practical results in the form of definite advice. I think I may say from practical experience that Professor Baily's conclusions are not only fully justified by the

experiments which he has carried out, but that the hypotheses which he has laid down are quite as fully established by experience in the use of machines. It has long been evident to most of us that the empirical calculations of reactance voltage and other things have only an indirect bearing on commutation. While it is quite true that we can calculate and establish by experimental results the fact that a high reactance voltage is exceedingly unfavourable to commutation, it is also true that we may design our machines with everything that can be desired in the way of satisfactory calculations as to the reactance voltage and yet find a machine which is quite unworkable. This point is clearly brought out by the fact that most designers have different limits for the reactance voltage for small machines and large ones. Professor Baily has come to the conclusion that while we must always, as it were, keep the question of reactive voltage at the top of our heads, what we want to keep at our finger-tips is knowledge of the effects of the very things to which he has been directing our attention. One of the most important of these is vibration of the brush on the commutator. This vibration may arise from many causes, from the commutator being out of truth, rough, or made of unsuitable metal. I have recently come to this conclusion from the fact that there is now a general superstition against making commutators of any cast metal because of the risk of impurities rendering the friction inconstant and uncertain. I do not think that friction in itself is a very serious matter. It produces vibration and brings about very uncertain results. Having obtained a commutator made of a homogeneous metal whose properties are known and which can therefore be dealt with by means of a suitable brush, we are again face to face with the condition of the surface of that material, and we are quite aware now that a hard-drawn pure copper commutator with a suitable surface may become very troublesome in presence of certain matters which may accumulate whether it is running or at rest. For example, the dust of textile fabrics is most injurious to the running of the commutator. In that connection the well-known empiric use of paraffin wax and such matters referred to by Professor Baily is interesting as giving an indication that lubrication is very important. After having considered the question of brush resistance, lubrication, and other matters, I must confess to a hankering desire to go back from the carbon brush to the copper brush. We know how, with some well designed old machines with copper brushes, the loss is minimised by the use of the copper brush, and how it is possible to get effective lubrication on the commutator by the mere accidental leakage of oil on it which occurs on all old machines. We can see machines that have been in use for twenty years with copper brushes, changing loads, and all the vicissitudes they have come through, with the commutator in perfect condition. There does not seem, after all, to be any essential reason why we should incur the loss by the use of a carbon brush at all. Mr. Mavor.

Mr. W. B. SAYERS: I am much interested to hear that Mr. Mavor is again thinking of copper brushes. I have had the experience of Mr. Sayers.

Mr. Sayers. seeing my experiments to a large extent extinguished by the advent of the carbon brush. I had been experimenting at Messrs. Mavor & Coulson's works with a view to obtaining sparkless commutation with fixed brushes by means of various devices, and I had attained a certain amount of success in that direction when the carbon brush came into the field and seemed to do very easily what I had found a troublesome and difficult thing. However, it has long since become clear that the carbon brush is not the complete solution of the matter which it was said at first to be, inasmuch as we see now many kinds of commutating poles and similar devices being introduced which were said by many at the time to be quite unnecessary. The carbon brush is said to do all that is required.

Mr. Nicholson. Mr. J. S. NICHOLSON : The paper would have been more interesting and instructive, especially to those who have had an opportunity of carrying out similar experiments, if diagrams of connections and apparatus had also been given. In the experimental determination of the brush contact resistance the fall of potential is measured across both brushes. The experiment would probably have been more complete if, in addition, the fall of potential between each brush and the copper drum had also been measured. That has already been done in previous researches, and I understand that the fall of potential is different at the two brushes.

Mr. Robinson. Mr. E. LEWIS ROBINSON : As regards Mr. Nicholson's remarks about the difference in the volts lost between the commutator and the positive and negative brushes respectively, experiments have been carried out, and it can generally be taken that the drop is twice as large at the negative compared with that at the positive brush. This can be measured by using an ordinary brush insulated from the holder and pressed on to the commutator so that it carries no current. By connecting this brush and the arm to a voltmeter, the lost volts can be measured. With regard to paraffin wax, care should be taken in using this on commutators. If the commutator is hot the wax immediately disappears ; again, if the commutator is at a temperature lower than the melting-point of the wax, the commutator becomes sticky, the contact resistance goes up, the brushes chatter, and sparking results.

Mr. Kelsall. Mr. A. H. KELSALL : With regard to the last curves showing the rapidity with which the current is transferred from the "leaving" commutator segment to the "making" commutator segment, I have been wondering whether Professor Baily eliminated the possibility of error due to microscopic differences in the level of the bars, seeing that he was working with only three bars fitted with resistances and taking his readings on the centre one. Differences of level must enter enormously into the question, and Professor Baily has already obtained some results showing a tendency to vibrate. I suppose I am right in concluding that Professor Baily is distinctly in favour of high densities, and I am wondering whether there is any special reason why he did not go beyond the 60 amperes in his density experiments. I would have liked to have seen the curves traced out for higher densities. In the

case of the curves where the pressure seems to be ample for vibration, the curve is still dropping rapidly at 60 amperes. With regard to Professor Baily's figures for friction for different makes of brushes and holders, it would be interesting to know whether these are all of the type known as the box-type holder, or whether any of them are of the hammer type. I think Professor Baily, a couple of years ago, expressed a preference for the hammer type, and I should be interested to know whether these recent experiments have modified his opinion on that point or not. For instance, the pressure on these brushes, from which the coefficient of friction was determined, is, I suppose, the measured pressure due to the spring, but in hammer-type brushes the friction itself has a tendency to increase or reduce the actual pressure between the brush and the commutator, according to the direction of rotation, and whether the tangent passes between the fulcrum and the commutator or outside the fulcrum. In some types of brushes I believe that this augmentation or reduction of the actual pressure is quite an important feature. I think also that there is some question as to the distribution of pressure over the surface of the brush, so that a tangent at the centre of the arc of contact may not be the mean effective tangent. Mr. Kelsall.

Dr. J. T. Bottomley (*communicated*) : The first subject dealt with in the paper is contact resistance and the effect of lubrication. This is a most interesting inquiry, but I wish that the experiments had been more judiciously planned out, so as to obtain the maximum of information, and guidance towards some sort of laws, if these are to be found. The pressures, which, by the way, are measured in a quite unrecognised unit, in ounces (what sort of ounce is not stated), are said to range from 7 oz. to 46 oz., but the numbers chosen were 7, 12, 18, and 46. I can think of no relation between these numbers, and if we look at the curves, they are spaced so irregularly, and in a manner so peculiar, that one can scarcely help thinking that there must be some factor concerned in the result which has not been taken into account. Let us compare, for instance, the speeds 1,430 and 3,300 feet per minute, and note the rise in pressure from 46 oz. to 24 oz. at the two speeds and that from 18 to 7, at 1,430, and 18 to 12, and 12 to 7 oz., at 3,300 feet per minute. Further, the speeds chosen for experimenting have no simple relation with each other. It seems to me that it would have been much more instructive had the pressures and speeds been raised, in the successive experiments, either by successive equal increments or else by successive doublings. In spite of the difficulties thus introduced, and in spite of the great difficulties of the inquiry, the authors have obtained results which are wonderfully concordant. The gist of these is given in Table I., and in an empirical formula connecting electromotive force, current, and pressure. It is here that the inconvenience of having the pressure expressed in ounces presents itself in an unfortunate way. The results obtained with lubricated brushes are highly interesting. It is not, perhaps, quite generally known that even in the case of the plugs of a resistance box the Dr. Bottomley.

Dr. Bottomley.

resistance at the plugs is markedly reduced if the plugs are thinly smeared with light paraffin oil. I believe this was first pointed out by the President of the Institution, Dr. Glazebrook, who found this to be the case in the course of his work at Cambridge and at the National Physical Laboratory. Probably the paraffin helps to clear away the film of air which invariably covers the brass surfaces, no matter what trouble is taken, by pressing the plugs into the holes, to get rid of it. The film of paraffin is more easily pressed aside from between the brass surfaces which come in contact than the corresponding film of air. In any case, the result is certain, a fact which I have verified. At the same time I must say that my experience is altogether against lubricated brushes. The copper, or other metal, of the commutator segments does not and cannot wear down at exactly the same rate as the mica which insulates the segments, and the mica gets wetted by the lubricant. The particles of metal or carbon from the brushes tend to stick to the mica, instead of being blown away, as is the case when the surface of the commutator is quite dry, and a tendency to spark is the invariable result. The results of the authors with regard to commutating poles seem to me to be of considerable value. It is to be hoped that they will be able to push this work further, and with the assistance of other types of machine. Sufficient experimenting has not, up to the present, been carried out to warrant safe generalisation.

Professor Baily.

Professor BAILY (*in reply*): It has been pointed out by Mr. Mavor that a large machine can be made to commutate smoothly under a higher reactance voltage than would be safe with a small one, and possibly the reason may be obtained in our paper. For while the air-gap and the magnetomotive force in it are not much greater than in the small machine, the linear interpolar distance is considerably larger, and hence the stray field at the coils under the brushes is smaller. Therefore, when running with fixed brushes the sparking E.M.F. does not rise above a moderate value, and the brush resistance alone is capable of checking the current, even against a high reactance. Considerably smaller air-gaps, similar to those of induction motors, might be used with advantage, and with no increase in exciting current a pole shoe of less breadth would then be possible. At the same time sparking is a phenomenon not always easy to explain, nor is it due to a single cause. As an instance a particular motor was made with a commutator of cast copper, which on test ran without any sparking. But after a continuous run of some twenty-four hours it would begin to spark violently. The commutator was changed and the trouble disappeared. Some small difference in the surface may be the only variant between a good machine and an obviously bad one.

Mr. Mavor has a tenderness toward the old copper brush, and, indeed, for low electromotive forces the losses in carbon brushes may rise to extravagant values for resistance and friction. But for anything like 400 or 500 volts the loss is not important, and the advantages are great. It must be remembered that not only is the reactance of a coil

Professor Baily.

in a slot high, but in cases where there are more commutator segments than slots, two or more adjacent coils lie in the same slot, and it is not possible to give a correct position to the brushes such as would be required on the ordinary theory of current reversal by an induced E.M.F. Hence there must be a good deal of forcing, or trust in brush resistance, however carefully the brushes are adjusted, and the gauze brush will not be working under the conditions possible in smooth-cored machines. Even with commutating poles a very nice adjustment of field will be necessary. For example, in our tests on the 15-H.P. motor, we found that the three coils in one slot produced markedly different sparking E.M.F., and the increase in circulating currents and sparking was alarming when gauze brushes were used. It is possible to use fixed gauze brushes under suitable conditions, for the motors made in 1891 for the City and South London Railway locomotives ran with fixed central brushes under a speed variation of 2·5 to 1 with full load and practically no sparking. But the speed was low and the field magnets very powerful, so that the distortion was small, while the armatures had smooth cores and only one turn per segment. The E.M.F. set up in the coil would be very small, and with so small an inductance the current would be broken quietly. But they were not cheap machines.

The question has been raised whether there is a difference between the drop of potential at the positive and that at the negative brush. Beyond noting that there is a difference, we did not pursue the subject, for the difference appeared to be probably due to thermo-electric forces between the surfaces of copper and carbon, and as the temperature of the carbon surface was likely to be variable, it was considered preferable to eliminate the effect by reading across both brushes. Professor Arnold, *loc. cit.*, has examined this matter more fully. The method of testing this and the drop of E.M.F. generally, which was proposed in the discussion, viz., to fit an insulated brush by the side of a working brush, with a voltmeter between, does not yield very definite information when used on a commutator, for as the current density in each part of the brush varies at different times when a segment passes under, the resulting E.M.F. refers only to an average value of the current. But it is a useful practical test. We tried taking readings between the brush and a point contact pressed against the commutator, placing it at the centre and near the two edges of the brush. But the values, though quite definite and regular, had no very definite meaning, since they afforded only an average over the whole segment breadth, and we accordingly arranged the more troublesome contact-maker method.

Some surprise has been expressed at the very trifling amount of increase of E.M.F. produced by a lubricant, and we certainly anticipated quite different results. Without proper care, however, a lubricant may cause a good deal of trouble, and its use is more adapted to central station dynamos than to small motors which receive little attention. I am informed that carbon brushes boiled in paraffin give good results, but I have not tried them.

In reply to Mr. Kelsall's question, we did not experiment on more

Professor Baily.

than one segment. The surface of the commutator was quite smooth, and we had no reason to suspect any irregularity. If there had been any depression or undue elevation of the segment in question, the effects would have destroyed all hope of consistency in our readings. It would have been impossible to put resistances in all the lugs, because there was not room for them; but it would have made no difference to the current values in the segments under the brush at the instant of taking a reading. All of these lugs had resistances, and the reading ceased until they came round again.

We have been criticised for limiting the current density to 60 amperes per square inch in the first part of the paper, and certainly, if the last part had been done first, we should have carried the values higher. At the time I thought 60 amperes a liberal allowance, and was pleased to find it a safe value. But in reality the current density was much higher on occasions, for the bedding of the brush was not always perfect. In the tests on S carbons, undertaken quite recently, the densities were increased, and no change of behaviour could be detected. The brush holders were chosen in order to eliminate vibration as much as possible, for vibration, although a very usual concomitant, is too variable to yield comparative and consistent results. The plain butt brush represents also a large class of commercial patterns, though I believe it to be inferior to the hammer type, in that the latter preserves a much superior bedding. The butt brush had for our purpose the additional advantage that the pressure was definite, and was not influenced by any frictional tangential force, whereas with hammer or arm holders there is usually a component tending to modify the pressure unless the brush arm is exactly at the correct angle.

Dr. Bottomley asks upon what principle the increase of speed values was chosen. In the tests with butt brushes, Fig. 1, he will find that each successive speed is about 50 per cent. greater than the preceding. In Fig. 5 a very low speed was substituted at the beginning to make sure of freedom from vibration, and the speed was then increased until some effect was noticed, subsequent increase being rather less than 50 per cent. each time. But in choice of both speeds and pressures we endeavoured to make such increase as would bring about a readable, but not too great change in the function sought. In Fig. 1 he has not taken the meaning of the sets of curves. The spacing increases in each successive set, owing to the increase of speed, and as the brush pressure rises, the influence of vibration comes into play only at higher speeds. Our unit of pressure was the ounce *avoirdupois*.

No examples of the Morganite brush were examined. The manufacturers have made similar tests themselves, as they have informed me since the reading of this paper, the results of which are in general agreement with ours. There is undoubtedly much to be done in the comparison of different brushes, particularly under working conditions, *i.e.*, commutating a current; but the investigation will be extremely laborious, for the difficulty of obtaining

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consistent results and satisfactory bedding is extraordinary. If I am able to extend these experiments I shall adopt some form of hammer brush, and shall use small areas of contact to hasten the wearing-down process and to retain it when obtained. It may be pointed out now, however, that the specific resistance of a brush, so far as we have gone, seems to exert small influence on the contact resistance. Thus the specific resistances of Morganite brushes, Le Carbone X, and Le Carbone S, are roughly in the ratio 1, 3, 9, while their contact E.M.F.'s at 60 amperes per square inch are about 1·2, 1·4, 1·6, and even this difference is partly accounted for by the resistance of the short length of carbon between the surface and the potential contact. This is suggestive, but requires more examination before any conclusion can be drawn. I am inclined to believe that the mechanical qualities of a brush, such as a low friction coefficient and freedom from chattering, are more important than its resistance, as brushes are made at present. And more important than the brush is the brush holder; but this we have advocated at ample length in the paper itself.

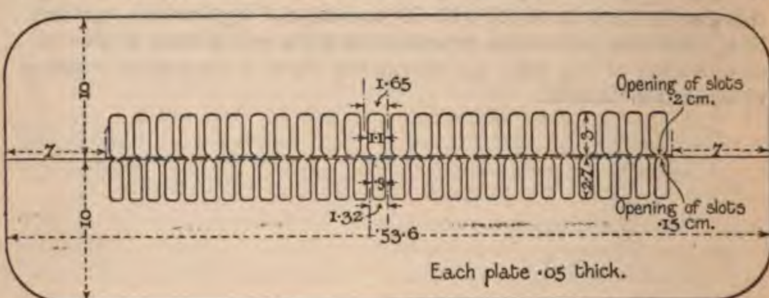
We wish to express our appreciation of the interest which the section has displayed in the subject, and in closing we have pleasure in referring to the benefit to science that is conferred by the Carnegie Trust through their research studentships and scholarships, and to express a hope that a not inconsiderable portion of these will be devoted to researches in the problems of applied science. Those who are occupied in teaching a subject which is perpetually changing, and who desire not only to keep abreast of its scientific developments, but also to keep in touch with its commercial applications, can find little leisure for continuous experimental work, and to such an one the co-operation of one who can devote the whole of his time to a piece of work is invaluable.

ORIGINAL COMMUNICATION.

THE ANALYSIS OF THE MAGNETIC LEAKAGES IN INDUCTION MOTORS.

By ARTHUR BAKER, Whit. Sch., and JOHN T. IRWIN, Associates.

There is still a considerable amount of uncertainty as to the proper value to be assigned to the leakage factor in the design of induction motors, even taking into consideration Dr. Behn-Eschenburg's valuable paper.* The experiments herein described were undertaken with a view to check his results by entirely different methods, and, if possible, to arrive at new methods of separating out the various components which go to make up the leakage factor. The experiments were carried out at the Central Technical College, and, of course, it was impossible to get such a variety of machines as Dr. Behn-Eschenburg



All dimensions in centimeters.

FIG. 1.

had; but, on the other hand, some of the measurements made would be applicable only in a laboratory.

Description of Experimental Apparatus.—In the ordinary induction motor it is impossible to vary the air-gap without altering the openings of the slots, and, of course, altering the magnetic leakage conditions.

In order to overcome this difficulty in the present case the experimental motor or transformer was made with the tips of the teeth in both stator and rotor in one plane, *i.e.*, as if a 2-pole motor were

* *Journal Institution of Electrical Engineers*, vol. 33, 1904, p. 239.

developed out flat. The air-gap was made and adjusted by projections at the ends of one set of stampings butting against the flat surfaces of the other set as shown on Fig. 1. By filing these projections any desired air-gap under 1·5 mm. could be obtained (a greater air-gap could be obtained by inserting a distance-piece of any non-conducting material). In order to measure the leakage that takes place across the slots, the windings in both the stator and the rotor were arranged in two portions, *i.e.*, the conductors in the bottom half of the slot were insulated from those in the half of the slot nearer the air-gap, the terminals of each set of windings being brought out to separate terminals.

Although the apparatus is really a transformer, we shall speak of it as a motor having a stator and rotor, so as not to confuse the subject by the introduction of the terms primary and secondary circuits, especially as in some of the tests each winding in turn is used as the primary. The motor was designed for 3-phase working, and the number of pole pitches was two, so as to make the reluctance of the circuit practically independent of the reluctance of the two supporting pieces. The motor was excited from a rotary converter, which gives practically a sine wave of E.M.F. The D.C. side was supplied at 200 volts, and the voltage on the A.C. side between the 3-phase slip-rings was about 122. It is known that the leakage factor varies with the shape of the wave of E.M.F., and it was therefore necessary to carry out the tests under properly specified conditions.

The symbols used are the same as those used in Dr. Behn-Eschenburg's paper, and the more important are here given :—

E_1 = E.M.F. per phase per pole.
f = frequency.
δ = air-gap in cms.
b = axial length of core-body in cms.
T = pole pitch.
χ = mean width of opening of slots in cms.
n = number of conductors per phase.
n_s = number of conductors per slot.
N = the mean of the number of primary and secondary slots per pole pitch.
B = maximum density in lines per sq. cm. in the air-gap.
ϕ = main flux.
ϕ_l, ϕ_{slot} = leakage flux, slot flux, etc.
D_s = depth of slot.
W_s = width of slot.
C_o = magnetising current.
C_F = full load current.
σ = leakage factor.

The motor was designed, made, and wound at the College. The ends of all the phases were taken to a common terminal board screwed on to the end plates, the wires being led in through holes at the top

and the bottom of the board and fixed inside. It is evident that in winding a 3-phase motor developed out flat as described the windings will not be symmetrical with regard to each other, and that the wires forming the end connections of the three phases must cross one another in the centre, and hence a good deal of overlapping necessarily occurs. The end connections of the central phase were turned well back on the end plates in order that the last phase wound might not have to project too far. This arrangement, of course, had the disadvantage that the lengths of wire for the three phases were different, and, of course, the self-induction of each phase was slightly different, and the mutual induction between the end connections of the phases varied considerably owing to the arrangement of the end connections. Great care was taken to get the air-gap uniform over the whole surface, the measurements being made in several places by the aid of feelers and a stout straight-edge before coupling together and again checked with feelers when ready for experimenting.

There was never a greater variation in different parts of the gap than 0·05 mm., the mean of eight readings always being taken, the accuracy of the feelers being afterwards checked with a micrometer screw.

When delta-connected and with a voltage of 122 across the mains the maximum induction in the air-gap was as follows:—

Frequency	30	40	50
Maximum induction ...	7,900	5,900	4,750

Constants of the Motor when Completed.

	Stator.		Rotor.
Pole pitches	2		2
Slots per pole pitch per phase	4		5
Conductors per slot	16		12
Conductors per pole per phase	64		60
Width of slot in cms.	1·10		0·90
Depth of slot in cms.	3·00		2·70
Opening of slot in cms.	0·20		0·15
Width of tooth in cms.	0·55		0·42
Thickness of iron above slot	6·85		7·30
Mean resistance per phase	0·09		0·08
Air-gap in cms. (to commence)		0·1397	
Pole pitch in cms.		20	
Breadth axially in cms.		16	
Width of end contact surfaces		7	

Theoretical Considerations.—In the first place it was thought that the leakage across the slots might be an appreciable factor, and if this turned out to be the case, then there would have to be a further term added to Dr. Behn-Eschenburg's formula—

$$\sigma = \frac{3}{N^2} + \frac{\delta}{X.N.T} + \frac{6\delta}{b}.$$

This would increase the theoretical value obtained by its use, for it will be observed by reference to his paper that the values given by the existing three factors were practically always too low, the average discrepancy for thirty-three motors experimented on being as much as 23·4 per cent. Again, in the above formula, the winding or zigzag coefficient also seems too low, for in its determination the mean value was assumed to be one-half the maximum.

Dr. Behn-Eschenburg himself drew attention to this fact, and Mr. Hobart thought that in some cases, as the minimum was never zero, this winding coefficient ought to be doubled, making it $\frac{6}{N^2}$.

In what follows it will be shown that the best value is about $\frac{4}{3}$ of Dr. Behn-Eschenburg's coefficient, making it $\frac{4}{N^2}$.

Slot Leakage.—The conductors can be assumed, especially in low-tension machines of small output, to be equally distributed along the depth of the slot. In this case we have the M.M.F. across the slot varying directly as the distance from the bottom of the slot, and as the

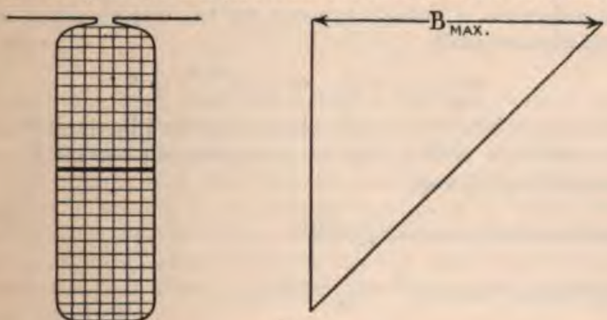


FIG. 2.

FIG. 3.

reluctance is practically that of the slot itself, the flux density varies from zero at the bottom to a maximum at the tip of the slot. Let n_s = No. of conductors per slot, N = No. of slots per pole pitch, ϕ_l. slot = total leakage across slot.

Figs. 2 and 3 show how the leakage flux varies along the depth of the slot.

The total E.M.F. generated by the leakage flux in one slot

$$=4 \cdot 4 \cdot f \cdot n_s \cdot \phi_l \cdot \tfrac{2}{3} \cdot 10^{-8}.$$

To confirm this experimentally, the winding in the slots was arranged in two halves as mentioned above, and the only way to measure the slot leakage was by observing the difference in voltage across the two halves when the same alternating current is sent through the two in series. The E.M.F. generated by the flux that cuts the half nearer the air-gap

$$=4 \cdot 4 \cdot f \cdot \frac{n_s}{2} \cdot \frac{3\phi_l}{4} \cdot \tfrac{5}{9} \cdot 10^{-8}.$$

The E.M.F. generated by the flux cutting the half at the bottom of the slot

$$=4 \cdot 4 \cdot f \cdot \frac{n_s}{2} \cdot \frac{3\phi_l}{4}\, 10^{-8} + 4 \cdot 4 \cdot f \cdot \frac{n_s}{2} \cdot \frac{\phi_l}{4} \cdot \tfrac{2}{3}$$

$$=4 \cdot 4 f \cdot \frac{n_s}{2} \frac{11\,\phi_l}{12}\, 10^{-8}.$$

The difference of voltage between the two halves which can be observed experimentally

$$=\mathrm{E}_{\text{bottom}} - \mathrm{E}_{\text{top}} = 4 \cdot 4 f \frac{n_s}{2} \frac{\phi_l}{2}\, 10^{-8},$$

which is seen to be $\frac{3}{8}$ of the total voltage generated by the slot leakage.

The maximum M.M.F. due to a magnetising current C_o in the windings $= C_0 \sqrt{2} \,.\, 4\pi n_s$.

Maximum density $= \dfrac{C_0 \sqrt{2} \,.\, 4\pi n_s}{W_s}$.

Total flux per slot $= \frac{1}{2} \dfrac{C_0 \sqrt{2} \,.\, 4\pi n_s}{W_s} D_s\, b$.

∴ Total E.M.F. leakage per slot

$$=4 \cdot 4 \cdot f \cdot n_s \left\{ \tfrac{1}{2} \frac{C_0 \sqrt{2} \,.\, 4\pi n_s}{W_s} D_s b \right\} \tfrac{2}{3}\, 10^{-8}$$

$$=2.6 f n_s^2 \frac{D_s}{W_s} C_0\, b\, 10^{-9}.$$

In a 3-phase machine total E.M.F. leakage per pole pitch

$$=2.6 f n_s^2 \frac{D_s}{W_s} C_0 \frac{N}{3}\, b\, 10^{-8}. \qquad \ldots\ldots\ldots \quad (1)$$

The total E.M.F. generated per phase in a 3-phase motor per pole pitch

$$=2.22, \tfrac{5}{6} \,.\, T \,.\, f \,.\, n_s\, b\, B \frac{N}{3}\, 10^{-8}.$$

$$B = \frac{\sqrt{2}\, C_0\, n_s\, N \times .4\pi}{3\,\delta}.$$

$$\text{E.M.F.} = 2.22 \left(\tfrac{5}{6}\right) . T . f . n_s\, b \left[\frac{\sqrt{2}\, C_0\, n_s\, N\, (.4\pi)}{3\,\delta} \right] \frac{N}{3}\, 10^{-8}.$$

The ratio of the E.M.F. due to slot leakage to that due to main flux

$$= 2\frac{D_s\,\delta}{T.W_s\frac{5}{9}} = 3.6\frac{D_s\,\delta}{T.W_s.N}$$

—*i.e.*, the ratio of the E.M.F. due to slot leakage to the total E.M.F. generated is equal to $3.6\frac{D_s\,\delta}{T.W_s.N}$, where δ is the *equivalent* air-gap.

$\therefore$ Since $\frac{\text{E slot leakage}}{\text{E total}} = \frac{L-M}{L}$, neglecting the other leakages in L,

$$2\frac{\text{E slot leakage}}{\text{E total}} = 7.2\frac{D_s\,\delta}{T.W_s.N.} = \sigma_{\text{slot}} \quad . \quad . \quad . \quad . \quad (2)$$

where σ_{sl} = portion of total leakage coefficient due to leakage of lines across the slot.

Where the voltage is high and the output of the machine small, the conductors, owing to the high slot insulation, instead of being uniformly distributed along the depth of the slot, will more nearly approach the case of a bunch of conductors concentrated at the middle of the slot. In this case it can be easily shown that the value of the leakage coefficient, which depends on the slot leakage, becomes approximately—

$$\sigma_{sl} = 10\,\frac{D_s\,\delta}{W_s.N.T.}$$

On the other hand, when the voltage is low and there is only one conductor per slot, and that very lightly insulated, as in a squirrel-cage rotor, or where the machine is of such a large size that there is only one conductor per slot, then the slot leakage will produce unequal distribution of current in the conductor, tending to make the total slot leakage less, and the voltage at the terminals of the conductor due to leakage might be taken as half the mean of what would be induced per conductor if a large number of conductors were placed in the slot. In this case—

$$\sigma_{sl} = 3.6\,\frac{D_s\,\delta}{W_s.N.T}.$$

It cannot be greater than this, since there is a reduction of the total leakage, and also since there is now a greater ohmic drop in the conductor, part of the P.D. due to leakage across the conductor should now be taken as an additional C.R. drop.

Zigzag Leakage or Winding Coefficient.—It is very difficult to obtain an expression mathematically for the zigzag leakage. It has so far been determined by observations on motors differing in certain particulars. Dr. Behn-Eschenburg obtained an expression for it by observing the variation of the leakage factor, (*a*) when the number of poles on the same motor was varied; (*b*) when the number of slots per pole pitch was varied, all other dimensions remaining the same.

It is very easy to see, however, what the ratio of the average to the maximum zigzag will be by considering one tooth, say, in the

rotor bridging a slot in the stator, as shown in Fig. 4, and assuming the rotor and stator to have the same number of teeth. Let w be the

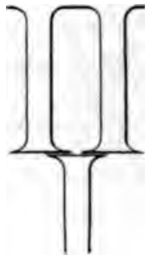


FIG. 4.

width of a tooth. Then when the rotor tooth is opposite the stator tooth, as shown in Fig. 4, the leakage flux—

$$\phi_l = K\frac{1}{\frac{1}{\frac{w}{2}}+\frac{1}{\frac{w}{2}}} = \frac{w}{4}$$

Now suppose the rotor tooth to move over a distance x, leakage flux

$$= K\frac{1}{\frac{1}{\frac{w}{2}+x}+\frac{1}{\frac{w}{2}-x}} = \frac{w^2-4x^2}{4w}$$

Average leakage when rotor moves over a distance $\frac{w}{2}$

Determination of the Zigzag Leakage.—It is always possible to determine the zigzag leakage on a finished machine as follows: A coil of very fine wire is wound so that it spans out the same pitch and has the same distribution of conductors as one of the stator windings on the actual machine, and so that the end connections follow the curvature of the end connections of the coils on the rotor, and are bent back to the same angle as the rotor coils. Remove the rotor and place the coil over one of the phase windings so that the wires on the test coil lie immediately over the corresponding conductors of one of the phase windings on the stator, *i.e.*, it will have to be placed immediately over the slots. Now, one can measure the difference in voltage between the phase winding and the test coil when an alternating current is passed through the winding. This difference is due to slot leakage, peripheral leakage, and leakage due to the end connections. Ohmic drop will also produce a small difference, though in most cases that can be neglected, and if its effect is appreciated it can easily be allowed for.

The total voltage due to the stator leakage, if the rotor and stator leakage can be assumed to be the same, is equal to $\frac{\sigma}{2} \times$ Primary Voltage; where σ is the leakage coefficient obtained either by the voltmeter method or by the ratio of the magnetising current to short-circuit current.

Therefore—

$$\begin{aligned}\frac{\sigma}{2} \times V_P &- \text{Difference of voltage between phase winding and test coil}\\ &= \text{Voltage due to zigzag leakage,}\end{aligned}$$

and therefore the value of the zigzag can be found. In the present experiments we have a second method of checking the result obtained by the above method. In an induction motor the zigzag leakage for a given voltage and frequency is independent of the air-gap, that is, if the main flux remains constant the zigzag flux will also remain constant. This may be shown experimentally. But the flank, slot, and peripheral leakages vary almost directly as the magnetising current when the slots are open.

Therefore if one can reduce the air-gap without altering any of the other dimensions of the machine, the leakage coefficient will be reduced and one can draw a curve between magnetising current and leakage coefficient. The magnetising current cannot be reduced to zero, even when the air-gap is nothing, owing to the current required to send the flux through the teeth and cores, but one can estimate the probable value of σ by producing the curve to the point where the magnetising current would be zero. The value of σ so obtained will be that due to the zigzag leakage only.

It is also possible in a finished machine to obtain a very good idea

of the value of the zigzag leakage by running it at two frequencies as different as possible, but keeping the same voltage and wave-form of applied potential difference. At the higher frequency the magnetising current and the main flux will be reduced. The main flux will vary inversely as the frequency, but owing to the increased permeance of the iron the magnetising current will be reduced more proportionately than the main flux, and therefore the flank, peripheral, and slot leakages will be reduced more than the main flux and the leakage factor will diminish. The zigzag leakage will be proportionate to the main flux.

Let σ_1 be the leakage factor at a frequency f_1.

Let σ_2 be the leakage factor at a frequency f_2.

Let K_1 be a factor which when multiplied by the magnetising current and by the frequency gives the leakage coefficient due to the flank, peripheral, and slot leakages. Let K_2 be the portion of the total leakage coefficient due to zigzag leakage.

Then—

$$\sigma_1 = K_1 C_1 f_1 + K_2$$
$$\sigma_2 = K_1 C_2 f_2 + K_2$$

where C_1 and C_2 are the magnetising currents at the frequencies f_1 and f_2. It should be pointed out that this test requires to be very carefully carried out, but since the voltage is constant the eddy currents *will be constant.*

Experimental Evidence.—When the machine described at the beginning of the paper was excited when coupled up in delta, it was found that the current in the branches varied considerably, and when connected in star the voltage across one of the arms was lower than across the other two. As the current through each winding when separately excited was practically the same, the out-of-balance currents must be due to the unsymmetrical arrangement of the end connections, to which attention has been already drawn. The voltages of the primary and secondary were nearly equal, and could be observed with great accuracy on an Ayrton and Mather electrostatic reflecting voltmeter reading normally to 8·5 volts, but by means of a potential divider it was arranged to read any voltage accurately up to 150 volts. This instrument was very convenient, as no correction was necessary in passing from one ratio to another, whilst with some of the very low voltages on the test coils used, the values could be read by coupling straight on to the voltmeter. As the leakage coefficient could not be determined by taking the ratio of the magnetising current to the short-circuit current, owing to the out-of-balance currents, it was determined by the voltmeter method. Starting with an air-gap of 0·14 of a cm. the leakage factor was found when connected in delta at a frequency of 60, and with a voltage of 120 between the mains; the mean leakage factor for the three phases was 0·0477.

Slot Leakage.—One of the easiest leakages in the present case to

separate out is that across the slot from side to side, as the coils composing the windings were wound in halves, the one near the air-gap and the other at the bottom of the slot. The voltage across each could be observed on the electrostatic voltmeter. To increase the accuracy of the readings (*i.e.*, the difference between the two for a given voltage across them) the air-gap was increased and a current considerably greater than the normal magnetising current sent through the rotor winding. When a current of 20·5 amperes was passed through phase winding No. 2 at a frequency of 60, the voltage, at the terminals of the half at the bottom of the slot was 61, and that at the terminals at the top of the slot was 60·1, the difference being 0·9. Now, the value theoretically of the difference between the voltages should have been as previously shown for one pole pitch.

$$E_{\text{Diff.}} = 2 \,.\, 6 \,.\, \tfrac{3}{8} \,.\, f n_s^2 \frac{D_s}{W_s} C_o \frac{N}{3} b\, 10^{-8}$$

$$\begin{aligned} E_{\text{Diff.}} &= 2 \,.\, 6 \,.\, \tfrac{3}{8} \,.\, 60\,(12)^2 \,.\, 3 \,.\, (20{\cdot}5) \frac{15}{3} \,.\, 16 \times 10^{-8} \times 2 \\ &= 0{\cdot}83 \text{ for the two pole pitches,} \end{aligned}$$

which is so near that determined experimentally that it may be taken as correct. The actual voltage generated in the coil by the slot leakage would, however, be $\frac{8}{3} \times 0{\cdot}9 = 2{\cdot}4$ at this current of 20·5 amperes and a frequency of 60, and is practically proportional to the magnetising current.

Analysis of Leakages at an Air-gap of 1·14 mm., Frequency 44, Three-phase Star Connected.

Experimental Determination of Zigzag Leakage.—A coil of fine wire was wound as described on page 197 of the same width as one of the coils of the rotor and having the same number of turns, viz., 60. It was arranged so that its end connections occupied the same relative position as those of one of the stator coils.

Phase 3 was chosen as its end connections came out straight in both stator and rotor. The rotor was excited three phases and the stator removed, the search coil being placed over Phase 3 of the rotor, with the coil resting just over the gaps in the slots and the end connections occupying the same position as would the end connections of the Phase 3 of the stator. Resistances were placed in each branch to vary the current and keep it within reasonable limits. To show the increase of leakage as the end connections were bent back further, experiments were carried out as follows :—

FREQUENCY 60.

Angle Coil is bent back.	Current 17 Amperes.		Current 11 Amperes.	
	P.D. of No. 3 Phase.	P.D. Test Coil.	P.D. No. 3.	P.D. Coil.
0°	25·3	19·90	15·9	12·30
30°	25·3	16·80	15·9	10·15
60°	25·3	14·60	15·9	8·35
90°	25·3	13·25	15·9	7·55

The difference in voltage between the phase winding No. 3 and the test coil when it comes out straight is 5·4 volts with 17 amperes, or 0·318 of a volt per ampere. The difference is 3·6 with 11 amperes, or 0·327 per ampere. The mean difference is 0·322 of a volt per ampere, and this is due to slot leakage, peripheral leakage, and leakage due to the end connections. Now, when excited star, with an air-gap of 1·14 mm. at a frequency 44, it was found that the leakage coefficient was 0·045 and the magnetising current was 4 amperes for phase winding No. 3 for rotor when pressure across the phase winding was 77·6 volts. At this frequency and current the voltage difference between the search coil and the phase winding would be $0·322 \times 4 \times \frac{44}{60} = 0·94$ of a volt. Now, half the leakage factor multiplied by the main voltage gives the total leakage voltage—

$$= 0·0225 \times 77·6 = 1·745.$$

Now the difference of voltage between search coil and winding is due to peripheral, flank, and slot leakages, therefore the difference between the total leakage voltage and that due to these leakages gives the zigzag voltage, *i.e.*—

$$1·745 - 0·94 = 0·805 \text{ of a volt.}$$

$$\text{Percentage zigzag to total} = \frac{0·805}{1·745} \times 100 = 46 \text{ per cent.}$$

The method referred to in the previous pages was now used to furnish another method of arriving at the zigzag flux. The air-gap was reduced progressively by filing away the end supporting pieces. The magnetising current and the leakage coefficient at constant voltage and frequency were measured for each air-gap, and the results are plotted on diagram Fig. 5. On diagram Fig. 6 a curve is plotted showing how the leakage coefficient varies with the magnetising current.

As the magnetising current diminishes owing to the smaller air-gap, the leakage coefficient diminishes at first fairly rapidly, but at the end more slowly. The diminution is due to two causes, first, of course, owing to the direct fall of the current, and secondly, owing to the magnetising current swinging more nearly into phase with the impressed volts as the power factor increases. The E.M.F. due to slot and flank

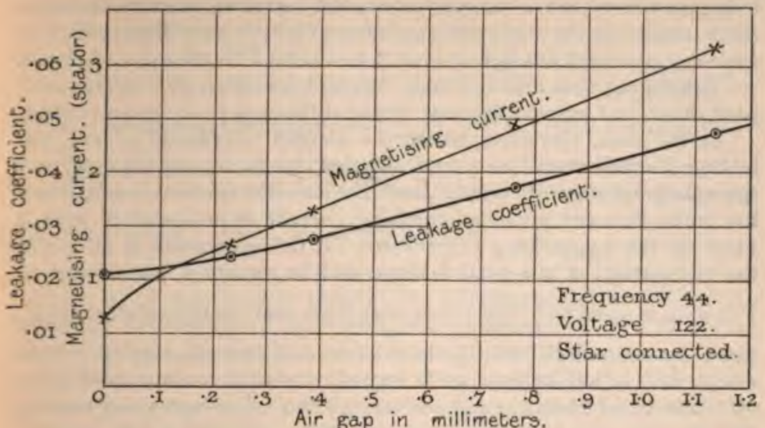

Magnetising current.
Leakage coefficient.
Frequency 44.
Voltage 122.
Star connected.
Leakage coefficient.
Magnetising current. (stator)
·06
·05
·04
·03
·02
·01
3
2
1
0
·1
·2
·3
·4
·5
·6
·7
·8
·9
1·0
1·1
1·2
Air gap in millimeters.

FIG. 5.

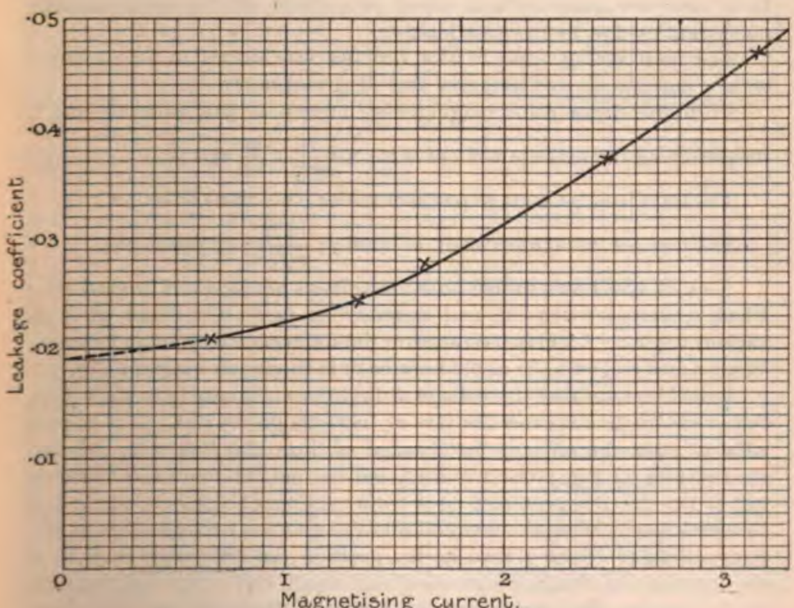

·05
·04
·03
·02
·01
Leakage coefficient
0
1
2
3
Magnetising current.

FIG. 6.

leakages, whose path is largely in air, will therefore be more nearly at right angles to the main voltage, and will have very little effect in reducing the main flux below what it would be if there was no leakage.

The zigzag flux and the main flux will, however, be in phase with each other, and practically at 90° phase difference to the main voltage.

In the case, therefore, where the air-gap is reduced to zero, the leakage coefficient will be practically that due to the zigzag only, and from diagram Fig. 6 it will be seen that its value is about 0·0195 when the curve between σ and magnetising current is produced to give a value for the magnetising current $=0$. If this is constant at all loads, the percentage of the total leakage due to zigzag at an air-gap of 1·14 mm. is equal to $\frac{0{\cdot}0195}{0{\cdot}047} \times 100 = 41{\cdot}5$ per cent., as against 46 per cent. given by the search coil. It is evident that the value given by the search coil is too high, since it exceeds the total leakage coefficient obtained experimentally; as, however, the two values agree very closely, they can be taken as confirming one another. The value 41·5 per cent. will be taken as the true value, however.

It is now proposed to find the remaining leakages at an air-gap of 1·14 mm. with a frequency of 44 per second, and with a pressure of 122 volts across the lines when connected 3-phase star. The mean magnetising current for the three legs of the stator winding was 3·18 amperes, and the mean value of the leakage coefficient was 0·047. It is now necessary to find what proportion of the total leakage coefficient is due to the slot leakage.

The total voltage due to slot leakage was shown to be 2·4 volts, with a current of 20·5 amperes and at a frequency 60.

With the mean rotor magnetising current of 3·65 amperes, and a frequency 44, the leakage voltage is $2{\cdot}4 \times \frac{3{\cdot}65}{20{\cdot}5} \times \frac{44}{60} = 0{\cdot}31$.

The total leakage voltage per arm under conditions stated $= 1{\cdot}75$.

Slot leakage voltage per cent. of total $= \frac{0{\cdot}31}{1{\cdot}75} = 17{\cdot}7$ per cent.

Now it has been shown how the voltage across a coil wound over Phase 3 varied when the end connections were bent back through various angles. A curve connecting the difference in voltage between Phase 3 and coil with the angle bent back is shown on diagram Fig. 7.

It will be seen that the voltage at first rises in a nearly straight line, but as the angle increases the rate of increase grows less. It is impossible to eliminate entirely the leakage of lines that link with the end connections of the phase winding, but do not link with the test coil, but if the curve is produced back for a very small distance, which it is assumed would bring the test coil into the position occupied by the end connections of Phase 3, then in that position the difference in P.D. at the terminals of the phase winding and search coil would be due only to slot leakage, the leakage across the tops of the teeth, and that across the slot above the winding. In that position the difference in P.D. at

the terminals of the two coils would be 4 volts—that is, with a current of 17 amperes at a frequency of 60. Now, the mean value of the magnetising current in the stator is 3·18, and in the rotor, owing to fewer turns, 3·65, at a frequency of 44. The difference of voltage due to this current and frequency $= \frac{4 \times 44 \times 3{\cdot}65}{60 \times 17}$ volts $= 0{\cdot}63$ of a volt.

The slot leakage voltage has already been found under the above conditions to be 0·31 of a volt, so that the top of the teeth leakage will be 0·33 of a volt : $\frac{0{\cdot}33}{1{\cdot}75} \times 100 = 18{\cdot}8$ per cent.

To find the percentage of the total leakage due to the end connections it is only necessary to measure the leakage coefficient on the three windings when they are each connected single phase. The difference in the leakage coefficient must be due to the end connections. When each phase was excited with 130 volts, the exciting current was 4·22 amperes at a frequency of 44 for the stator, and 4·8 amperes for the rotor. The following values of σ were obtained by the voltmeter method.

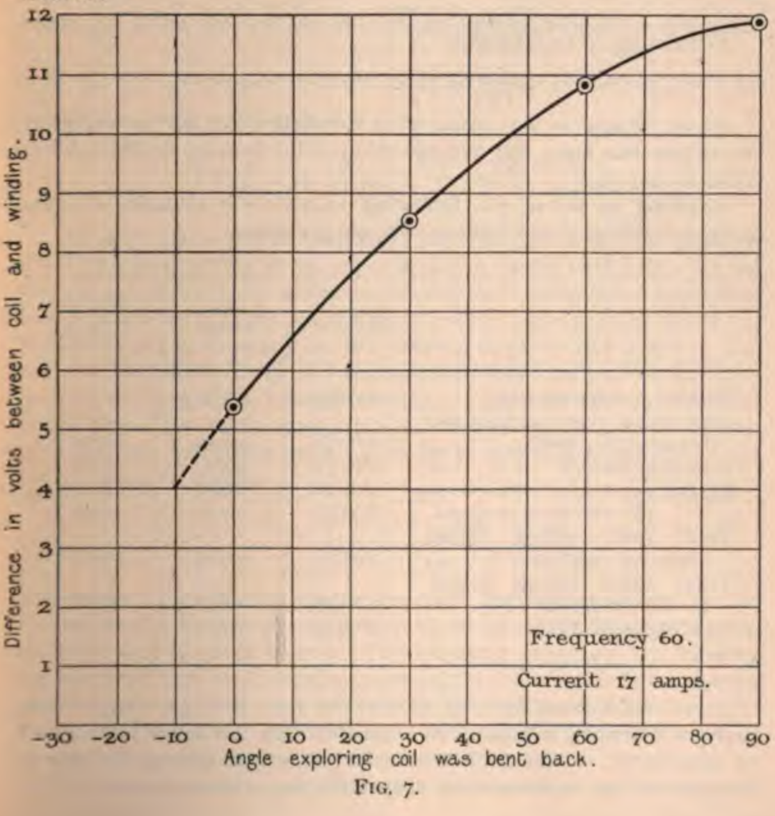


FIG. 7.

Phase 1.			Phase 2.			Phase 3.
0·045	...	...	0·0425	...	...	0·036

The mean difference in the value of σ in Phase 3 from that in Phase 1 and Phase 2 is $\frac{0·009 + 0·0065}{2} = 0·0077$.

The increase of leakage in Phases 2 and 3 will produce a voltage $= \frac{1}{2}(0·0077)(130) = 0·5$ of a volt; if the magnetising current was 3·65 this voltage would have been for the same frequency $0·5 \times \frac{3·65}{4·8} = 0·38$ of a volt. The value of the leakage voltage due to the end connections of Phase 3, when excited, was assumed to be 1·4 volts for a current of 17 amperes and a frequency of 60.

If the magnetising current were 3·65 and the frequency 44 the voltage would be 0·22.

Mean leakage of three phases

$$= \frac{0·38 \times 2 + 0·22 \times 3}{3} = \frac{1·40}{3}$$

$$= 0·47 \text{ of a volt.}$$

Percentage of total leakage

$$= \frac{0·47}{1·75} = 26·8.$$

At an air-gap of 1·14 mm., when connected star with a voltage of 122 across the lines, the average value of the leakage coefficient was found to be 0·047.

Analysed as above, the following values were obtained for the leakage coefficients and leakage voltages per arm.

	Leakage Coefficient.	Leakage Voltage.	Voltage per Cent. of Total Leakage.
Parallel portion of slot ...	0·0083	0·310	17·7
Across slot over winding and between the teeth	0·0088	0·330	18·8
End connections	0·0131	0·470	26·8
Zigzag (*a*) search coil ...	0·0216	0·805	46·0
(*b*) variation of gap...	0·0195	0·727	41·5
Total value taking zigzag given by method (*a*) ...	0·0518	1·915	109·3
Total value taking zigzag given by method (*b*) ...	0·0497	1·837	104·8
True value of total	0·0470	1·750	100·0

It will be noticed that the sum of the total leakage voltages thus analysed makes up a total which is greater than the actual total found by experiment. It should, however, be pointed out that all the leakage voltages are not in phase with each other, and therefore their vector

sum may be equal to that found. It should be pointed out here that the leakage voltage due to the end connections will be very much less in the present case than in an actual machine where the end connections of the rotor and stator are moving relatively to one another, and only for a very short period are they in the favourable position that exists in the present experiment. No attempt is therefore made to found any rule on this experiment for the leakage of the end connections.

Dr. Behn-Eschenburg gave as the equation for the zigzag or winding coefficient $\sigma = \frac{3}{N^2}$. In the present case, since—

$$N = 13{\cdot}5, \ \sigma_{zigzag} = 0{\cdot}0165.$$

Now in one set of experiments σ was found to be 0·0216, and in the other 0·0195, which are seen to be nearly equal to $\frac{4}{N^2}$, which gives $\sigma = 0{\cdot}022$. Theoretical considerations first seemed to point to a value $\frac{4}{N^2}$, so that this will be taken as giving more nearly the true value than $\frac{3}{N^2}$, and practically to confirm the equation previously given for the total leakage coefficient—

$$\sigma = \frac{4}{N^2} + \frac{\delta}{X.N.T.} + \frac{6\,\delta}{b} + \frac{7{\cdot}2\,.\,\delta\,.\,D_s}{T\,.\,N\,.\,W_s}.$$

An application of this formula to the thirty-three motors given by Dr. Behn-Eschenburg in his paper is shown on the next page. As the ratio of the depth to the width of the slot was not known it was taken as equal to three. This ratio would, however, generally be lower for high-voltage machines and for the rotors of squirrel-cage motors. The value of the constant for slot leakage was taken as 7·2, but this also varies according to the number and the arrangement of the conductors in the slot. The equivalent value of δ was also unknown. It will be seen that the calculated result given by Dr. Behn-Eschenburg has to be increased by a value—

$$7{\cdot}2\,\frac{D_s\,\delta}{W_s\,.\,N\,.\,T} + \frac{1}{N^2}.$$

The result is that the average disagreement from the observed value of σ is reduced from 23·4 per cent. to 12·3 per cent.; in other words, the disagreement is about halved. The algebraic mean got by dividing the algebraic sum of all the disagreements by the number of the motors gives 4 per cent., *i.e.*, the probability is that the modified formulæ would give a value 4 per cent. low, as against a probable value of 23 per cent. using the original formulæ.

The curves given by Mr. Hobart in the discussion on the original

paper showed for the same motors an average disagreement from the observed value of 9 per cent., and the probability is that the coefficient found would be 6 per cent. lower than the observed value.

The modified formula given on the previous page is therefore of about the same order of accuracy as Mr. Hobart's curves, and at the same time indicates how the value of σ varies with the dimensions.

List of Thirty-three Motors given in Dr. Behn-Eschenburg's Paper.

No.	$7{\cdot}2\,\dfrac{D_s\,\delta}{W_s\,.\,N\,.\,T.}$	$\dfrac{1}{N^2}$	Total to be added.	New Value.	Observed Value.	Difference.	Difference Per Cent. Observed.
1	0·0029	0·0023	0·0052	0·0524	0·060	0·0076	−12·7
2	0·0062	0·0051	0·0117	0·0691	0·090	0·0209	−23·2
3	0·0029	0·0023	0·0052	0·0403	0·045	0·0047	−10·4
4	0·0066	0·0051	0·0117	0·0570	0·075	0·0180	−24·0
5	0·0042	0·0039	0·0081	0·0470	0·050	0·0030	− 6·0
6	0·0075	0·0069	0·0144	0·0639	0·063	0·0010	+ 1·6
7	0·0042	0·0039	0·0081	0·0389	0·042	0·0031	− 7·4
8	0·0075	0·0069	0·0144	0·0558	0·056	—	—
9	0·0068	0·0056	0·0124	0·0672	0·067	—	—
10	0·0068	0·0056	0·0124	0·0472	0·046	0·0012	+ 2·6
11	0·0057	0·0045	0·0102	0·0499	0·054	0·0041	− 7·6
12	0·0075	0·0056	0·0131	0·0553	0·070	0·0147	−21·0
13	0·0093	0·0056	0·0149	0·0534	0·060	0·0066	−11·0
14	0·0057	0·0045	0·0102	0·0513	0·054	0·0027	− 5·0
15	0·0120	0·0045	0·0165	0·0582	0·062	0·0038	− 6·1
16	0·0045	0·0033	0·0078	0·0398	Not	known	
17	0·0063	0·0033	0·0096	0·0505	Not	known	
18	0·0047	0·0055	0·0102	0·0492	0·046	0·0032	+ 7·0
19	0·0066	0·0055	0·0121	0·0601	0·055	0·0051	+ 9·3
20	0·0033	0·0091	0·0124	0·0486	0·040	0·0086	+21·5
21	0·0084	0·0091	0·0175	0·0627	0·054	0·0087	+16·1
22	0·0038	0·0033	0·0071	0·0328	0·042	0·0092	−22·0
23	0·0010	0·0008	0·0018	0·0187	0·022	0·0033	−15·0
24	0·0068	0·0055	0·0123	0·0569	0·067	0·0101	−15·0
25	0·0017	0·0014	0·0031	0·0337	0·033	0·0009	+ 2·8
26	0·0065	0·0069	0·0134	0·0593	0·064	0·0047	− 7·3
27	0·0016	0·0017	0·0033	0·0314	0·034	0·0026	− 7·6
28	0·0068	0·0055	0·0123	0·0497	0·060	0·0103	−17·2
29	0·0030	0·0024	0·0054	0·0324	0·043	0·0106	−24·7
30	0·0082	0·0091	0·0173	0·0664	0·054	0·0124	+23·0
31	0·0041	0·0023	0·0064	0·0351	0·039	0·0039	−10·0
32	0·0104	0·0091	0·0195	0·0972	0·075	0·0222	+29·6
33	0·0085	0·0061	0·0146	0·0753	0·065	0·0103	+15·9

It has been assumed up to the present that the zigzag leakage is constant for a given machine, and independent of the air-gap and of the frequency and of the main voltage. To prove this the air-gap was varied between very wide limits, *i.e.*, from 1·395 mm. down to 0·38 of a millimeter, and the flux measured in three positions, as shown on the accompanying figures, 8 and 9. Coil I. was placed over the central tooth of Phase 2. Coil II. was placed on a tooth on rotor opposite

Coil I. Coil III. was placed in the gap between stator and rotor as shown. If the permeance on each side of the winding had been constant, then when Phase 2 only was excited the coils as shown in the figure would not measure any of the main flux, and there would be no

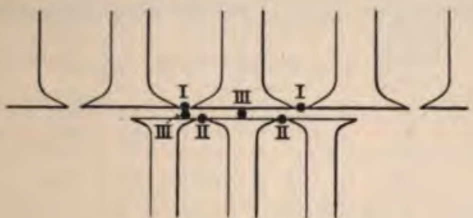


FIG. 8.

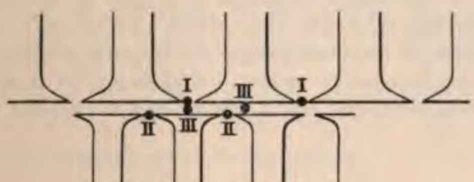


FIG. 9.

zigzag. In the figure the rotor is shown moved through half a tooth pitch, and the voltages generated in the coils are again measured. In each case 126 volts is put direct across Phase 2, and the magnetising current allowed to flow with the various air-gaps, *i.e.*, the main flux remained constant.

Air-gap in mm.	Primary Volts.	P.D. on Test Coils of Twenty Turns.					
		Position in Figure.			Position in Figure.		
		On Primary.	On Second.	Over Tooth.	On Primary.	On Second.	Over Tooth.
		I.	II.	III.	I.	II.	III.
1·395	126·3	0·845	0·520	0·475	0·89	0·73	0·475
0·762	126·5	0·890	0·525	0·475	0·87	0·79	0·475
0·380	125·6	0·830	0·580	0·470	0·85	0·76	0·475

The air-gap could not be reduced to a lower value on account of the exploring Coil III., but it will be seen that between the wide limits given the E.M.F. induced in the three coils is practically constant. If the distribution of the flux in the air-gap does not vary very much,

then it can be assumed that the zigzag is practically constant for all air-gaps. The windings were also excited with 3-phase currents, and the voltages registered on the coils are given as follows :—

Air-gap in mm.	Mean Voltage across Windings. Primary.	P.D. on Test Coils of Twenty Turns.					
		Position in Fig. 5A.			Position in Fig. 5B.		
		Coil I.	Coil II.	Coil III.	Coil I.	Coil II.	Coil III.
1·142	73	2·60	2·00	0·970	2·60	2·08	1·030
0·381	73	2·43	1·91	0·725	2·47	2·01	0·855

In this case the voltage measured is greater, as we are measuring the portion of the main flux that passes through the coils. In this case all the voltages for some reason are lower at a lower air-gap, but their ratio remains nearly constant. Now from the diagrams Figs. 5 and 10 it will be seen that when the air-gap is reduced to zero with

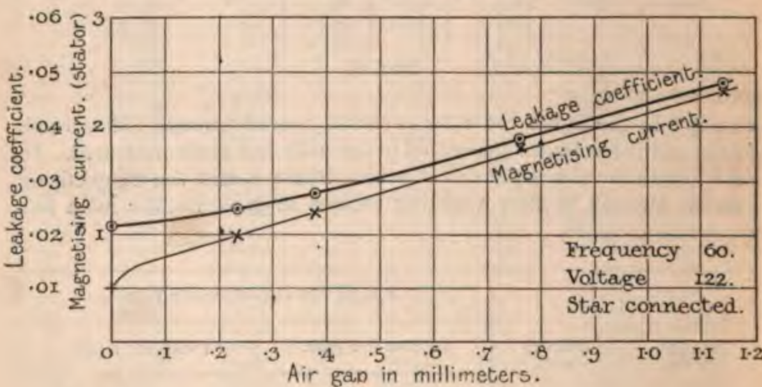


FIG. 10.

frequencies of 44 and 60, the leakage coefficient is practically the same in both cases, so that the zigzag is independent of air-gap and of frequency. It therefore must be independent of voltage in primary, since the variation of permeance of the iron parts within the limits used in ordinary motors will not affect the distribution of the zigzag leakage. While recognising the limits to an investigation by the above methods, the authors hope that results obtained will be of use in enabling a closer approximation to be made to the value of the leakage coefficient. They have to thank Professor Ayrton for the facilities and advice given them in connection with the experiments.

JOURNAL

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Proceedings of the Four Hundred and Forty-eighth Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, January 10, 1907—Dr. R. T. GLAZEBROOK, F.R.S., President, in the chair.

The minutes of the Ordinary General Meeting held on December 20, 1906, were taken as read, and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

TRANSFERS.

From the class of Associate Members to that of Members :—

Hermann Bohle.
Ignatius Bulfin.
James T. Cornish.
F. Mackenzie Lea.
George Ernest V. Thomas.
Henry Nicol Thomas.

From the class of Associates to that of Members :—

Joseph Jervis Atkinson.
Harold Wm. Firth.
John Joseph Francis O'Shaughnessy.

From the class of Associates to that of Associate Members :—

Ernest Holt Owtram.
Edward Vernon F. Shaw.

From the class of Students to that of Associate Members :—

Andrew H. Gordon.
Cyril Grimes.
Hugh Arnold Price-Hughes.
John Wm. Law.
Ernest J. Nichols.
Edwin Ross Rudge.
Herbert John Seale.
James H. Soames.

Donations to the *Library* were announced as having been received since the last meeting from Messrs. Gauthier-Villars, F. Gill, R. S. Hutton, Macmillan & Co., Ltd., T. C. Martin, F. J. A. Matthews, H. Wilde ; to the *Building Fund* from W. Duddell, S. E. Glendenning, W. Golledge, R. Hammond, Professor A. Hay, D. Henriques, Sir H. B. Jackson, E. Mascart, F. S. Miller, C. W. G. Nelson, F. Nicholson, R. O. Ritchie, M. Solomon, Sir J. W. Swan, A. J. Venables, T. C. T. Walrond ; and to the *Benevolent Fund* from J. R. Andrew, Ivon Braby, A. Denny, B. M. Drake, W. Duddell, R. Hammond, K. Hedges, S. H. Holden, J. R. P. Lunn, E. de M. Malan, C. H. Merz, F. H. Nicholson, C. C. Paterson, H. L. Riseley, S. G. C. Russell, Sir David Salomons, A. A. C. Swinton, F. J. Thompson, R. W. Wallace, K.C., T. C. T. Walrond, Captain R. F. Willis, J. Woodside, C. H. Wordingham, The Committee of The R. K. Gray Portrait Fund, to all of whom the thanks of the meeting were duly accorded.

Mr. A. Russell and Mr. M. J. E. Tilney were appointed scrutineers of the ballot for the election of new members, and, at the end of the meeting, the following were declared to have been duly elected :—

ELECTIONS.

As Members.

John Christie.
George Verity.
Arnaldo Paolo Zani.

As Associate Members.

William Tannahill Calderwood.
Henry P. Clausen.
Leonard Parry Mell.
Charles Mittelhausen.

NEW INCANDESCENT LAMPS.

By J. SWINBURNE, F.R.S., Past-President.

(*Paper read January* 10, 1907.)

A little more than a quarter of a century ago several men were working out the arc lamp, and inventors were bringing out what they called semi-incandescent lamps. People's ideas were very obscure in those days, and much confusion was due to the misstatement of the problem that was really waiting for solution. At this time they were trying to solve the problem of the "Subdivision of the Electric Light." The ordinary dynamo of commerce was series wound, and much difficulty was experienced in running more than one arc lamp on a given machine. Some of the makers succeeded to a certain extent. For instance, Jablochkoff had a dynamo with a number of separate circuits, and each circuit ran several lamps in series. Brush, again, ran lamps in series on series-wound arc machines, but inventors generally were trying to run several arc lamps from a single series-wound Gramme machine, giving 60 volts and, say, 20 amperes at rated output.

One of the first to understand the real nature of the problem and the proper means of solving it was Edison. He realised that high-resistance lamps could be worked in variable numbers in parallel on constant-pressure circuits. This seems so obvious now that we cannot understand where the difficulty came in; but a reference to the technical press just before 1880 will show that people's ideas were very chaotic. At this stage there were evidently two things necessary for distribution: a small lamp and high pressure. Edison seems to have begun with a high pressure especially in view, and therefore worked upon high-resistance lamps made of platinum. Swan, on the other hand, began by keeping small lamps especially in view, and worked on carbon, beginning with low resistance. Swan found low-resistance carbon lamps promising, then went on to high resistances, while Edison, having high resistances chiefly in view, took out a patent for using a filament of carbon to give the high resistance. The curious thing is that once platinum was put aside, it was always assumed that carbon was the only material possible. From 1880 until quite recently it seems to have been generally taken for granted that no departure from carbon was possible. Certainly, there were some experiments on

silicon. As silicon is very closely related to carbon, it seemed likely that it would make a good filament, but nothing came of it. Attempts were also made to coat carbon filaments with silicon and with boron.

It may seem strange that people did not experiment on some of the more refractory metals; but a little consideration will explain matters to some extent. In the first place, the interest was small. Now, when lamps are made by the million, an improvement means a fortune to the inventor. The result is that there are far more people, and especially far more very highly trained men, working on the subject now. Another point is, that in the early days very little was known about most of the refractory metals. During this time, for example, such substances as thoria, ceria, zirconia, and other "rare earths" have become quite familiar to the practical chemist. Again, we have become much more familiar with high temperatures and high melting points through working with the electric furnace. Many people, no doubt, thought of trying some of the refractory metals, but the information was very vague and very meagre, and no one had any clear ideas about high temperatures. Once we got into anything above 1,000° C. we had no means of comparing temperatures, and no sort of accurate ideas of temperatures at all. If one looked up chemical literature to find out whether a given metal had a high enough melting point to be of use, all that could be found was that some said it was infusible; others said it was fusible only at the "highest temperature of the forge." Suppose, for example, any one thought of trying chromium, he would find that the process recommended was mixing the oxide with lampblack and subjecting it in a carbon-lined crucible to the heat of a blast furnace. A lamp inventor seldom keeps a blast furnace; but the method suggests a rough way of working, and a very impure result, as even before the days of the vulgarisation of carbides, we all knew that certain metals took up carbon, like cast iron. Then he would find that the chromium so made is crystalline powder, less fusible than platinum, and that is all. The information would not lead the most sanguine inventor to try making chromium into wire, and making lamps of it. In fact, the information that it scratches glass, and is at least as hard as corundum, is in itself enough to discourage him. We can now get chromium by another method altogether, which does not involve carbon—namely, reduction by means of aluminium—and this process has given us several metals. Research in the direction of special steels has also made us much more familiar with many of the refractory metals.

But in order to make a successful lamp it is not enough merely to look up the melting point of a metal, or even to find it out by experiment. The people who have so far succeeded have had an exceedingly difficult task. They have generally had, first, to work out a chemical process which gives pure metal. This in itself is no small achievement. Next, they have to find out if the melting point of the metal is high enough, and then they have to face the exceedingly difficult problem of making fine enough wire of a metal which seems almost

unworkable. Even this is by no means all; the question of specific resistance and fineness and length of wire comes in.

This raises at once the most serious consideration as to the new metallic lamps.

The early Swan lamps took from 40 to 70 volts, but it was soon found that that was too low a pressure even for such work as hotel and theatre lighting. Edison used about 100 volts, and in this country the Swan Company turned out good 100-volt lamps about 1884. It was found very difficult in those days to make lamps for more than 100 to 110 volts, as the process then in use for making the filaments did not lend itself to very thin conductors of considerable length. About that time, however, the method of making filaments by squirting a solution of pyroxylin and reducing it, or squirting a solution of cellulose in chloride of zinc, came into use. All the time the demands of economical distribution had been pressing lamp makers to provide 200 or even 250-volt lamps. Carbon lamp makers have now been able to make 200-volt lamps, even of small powers, for some years. When it is remembered that the process of squirting a solution of cellulose into a liquid is now employed for making artificial silk, it will be realised that squirting enables the lamp maker to make his filaments as fine as he likes. The difficulty, then, is not in making a sufficiently fine thread in the first place, but in mounting it, and above all, in getting a durable lamp of such fine material. If the surface emission is the same, the pressure varies directly as the square root of the cube of the length of the filament. Other things being equal, a 200-volt filament is nearly 1·56 times the length of the 100-volt conductor, and about two-thirds of the diameter. It is thus very much weaker. Weakness is not by any means the only consideration, however. The slow wearing of the surface causes a greater percentage difference in the resistance of the lamp; and there is double the pressure available for causing discharge across from one leg of the filament to the other. The only way the lamp maker can reach high pressures, other things being equal, is by increasing the size of the lamp, that is to say, making 16 instead of 8 candle lamps, and so on. In old days the ordinary lamp was about 20 candles, then came 16 candles, and then the 8. These sizes seem to have been the result of chance, as the ordinary gas burner with which the electric lamp had to compete at that time gave from 10 to 12 candles.

We have thus the distribution engineer clamouring for high pressures, and the lamp maker trying to meet his demand, and making up to 250-volt lamps. The lamps are naturally worse than those made for lower pressures—in fact, until you get down to such low pressures that the filaments become sticks, the lower the pressure the better the lamp—but the combination of better distribution and worse lamp is on the whole much better for the public, as the gain through better distribution is greater than the loss through inferior lamps.

Before leaving carbon lamps, an American improvement has been

introduced by Mr. Howell. The carbon filaments are heated in an electrical furnace to convert them into a form of graphite. This is really perfecting an old process rather than inventing a new one. The effects of high temperature were known long ago. In 1884 and 1885 it was important not to treat the filaments with hydrocarbon, for patent reasons, and the filaments were simply electrically heated to harden them and reduce the resistance. To prevent deposition of carbon precaution had to be taken, as the vapour from the grease used for lubricating a glass tap or sealing a joint was enough to deposit visible traces of carbon. Filaments heated electrically without deposition were hard, flexible, and black, and in some cases they had about the same resistance hot as cold. They were more durable than carbon-coated filaments. The American lamps are probably a development of this idea. Every carbon lamp maker has known for many years that "flashing" not only deposits carbon, but entirely alters the nature of the filament itself. I believe flashed filaments do not act on hot strong sulphuric acid.

As soon as we deal with metal lamps the question of distribution comes up again. How are lamp makers to get the metal wire so fine that it will take, say, 200 volts? The metal lamp can only supplant the carbon on the ground of higher efficiency, and the efficiency claimed is much higher. Thus if a carbon lamp of 200 volts and 16 candles has an efficiency of 0·25 candles per watt, it must have a resistance of 625 ohms. To make a carbon of 625 ohms small enough to give 20 candles at 0·25 c.p.w. is a feat, but to make a metal wire to do so is a very difficult matter. But a metal lamp is made to work at, say, 0·75 c.p.w., and that means that for 16 candles there are only 21·4 watts. This means a resistance of 1,870, or nearly 2,000 ohms. To make a lamp filament of metal so as to have a resistance of 2,000 ohms, and only to give 16 candles at a temperature necessary to give such a high efficiency as 0·75 c.p.w., would be a wonderful feat. There are two influences which help the metal lamp, however; the resistance of the metal rises considerably with the temperature, as a metal is run not very far from its softening point, and the emissivity of bright metal is less than carbon, so that to give the same light at the same efficiency the wire may be larger. It seems probable, however, that bright metal surfaces increase in emissivity as they get hot, that is to say, they get blacker as they get hotter: a black body being brighter at a given high temperature than a white body.

We thus come upon the weak point of the metallic lamp; it is very difficult to make a small lamp of 200 volts. The questions are, therefore, whether the higher efficiency of the metal lamp will induce us to bring our pressure down to 100 volts or less, on the one hand, or whether the metal lamp can be made to take 200 volts or more either by further perfection by the use of some sort of transformer, or by using larger lamps, say, 50 candles each. Of course, we may take to running the lamps in pairs, or in threes in series, but that does not seem likely. We went through a good deal of trouble of that sort

in the early days of the carbon lamp, and it is improbable it will be repeated. Large buildings may be wired on the 3-wire system with a dead middle wire; but this does not appeal very strongly to modern engineers, and it is only applicable to new buildings that are not yet wired. If the metallic lamp had come into being twenty years ago it would have commanded its own conditions, but now it has to adapt itself to the conditions already existing. The average householder will certainly not put in a special transformer to give him 100 volts, and it will take a good deal of persuasion to induce a supply company to convert its 3-wire into a 5-wire system. At the same time it must be remembered that there are still plenty of 100-volt circuits, and the output necessary to supply these is quite great enough to give the metal lamp a footing which may enable it to prescribe its own conditions in the future. It seems probable, however, that people's ideas of the value of light will alter, and that incandescent gas or large metal lamps will soon lead them to use 50 candles as the normal light at each point. At the same time, the ingenuity which has made metal lamps possible seems quite capable of making 200-volt 16-candle lamps within reasonable time.

When we come to the question, What metals or compounds are available for lamp filaments? we have to consider the melting point, the specific resistance, and the possibility of making the filaments in practice. As to the melting point, it has been quite impossible to melt many of the metals until the electric furnace came into use; but the electric furnace generally deals with the metals in the presence of carbon, which probably alters their melting points. But apart from that, before the recent development of the study of radiation, there was no means of measuring such temperatures as the fusing point of tungsten, for example. Even yet we have no data as to the melting points of most of the refractory metals. The best guide available is the Periodic Law.

We may take what is generally known as the fourth group, and take the even series. This gives us carbon, titanium, zirconium, cerium, an element not yet discovered, and thorium. We may also take in, for consideration, boron, which is next to carbon, but in the third group, and silicon, which is also next to carbon, and in its own group, but in the third odd series. Then in the fifth group we may take the even series and get, after nitrogen, vanadium, niobium, didymium, tantalum, and an unknown element. In the next group we get, after oxygen, chromium, molybdenum, a ghost, and tungsten and uranium. In Series 6, running through Groups IV., V., VI., we get zirconium, niobium, molybdenum; and in Group VIII., rubidium, rhodium, and palladium. Series 10, which contains tantalum and tungsten in Groups V. and VI., has osmium, iridium, and platinum in Group VIII. By using elements whose melting points are known to be high as a sort of guide post, we can pick out the elements most likely to have high melting points.

Instead of using a table of elements we may work from curves

TABLE I.

Series	Groups. I.	II.	III.	IV.	V.	VI.	VII.	VIII.
1	H							
2	Li	Be	B	C	N	O	F	
3	Na	Mg	Al	Si	P	S	Cl	
4	K	Ca	Sc	Ti	V	Cr	Mn	Fe Co Ni Cu
5	Cu	Zn	Ga	Ge	As	Se	Br	
6	Rb	Sr	Y	Zr	Nb	No	?	Ru Pd Rh Ag
7	Ag	Cd	In	Sn	Sb	Te	I	
8	Cs	Ba	La	Ce	Di	?	Sm	? ? ? ?
9	?	?	?	?	Er	?	?	
10	?	?	Yb	?	Ta	W	?	Os Pt Ir Au
11	Au	Hg	Tl	Pb	Bi	?	?	
12	?	?	?	Th	?	U	?	

[redacted]

cellulose and carbonise it; but in early days it was proposed to make the filaments of a paste such as lampblack and tar worked up. No practical lamps were ever made in this manner; but straight rods about a millimetre diameter were made as arc-light carbons, I think, by Messrs. Carré, and these were made into incandescent lamps. They were probably ground retort carbon made into a putty with sugar, treacle, or tar, and carbonised.

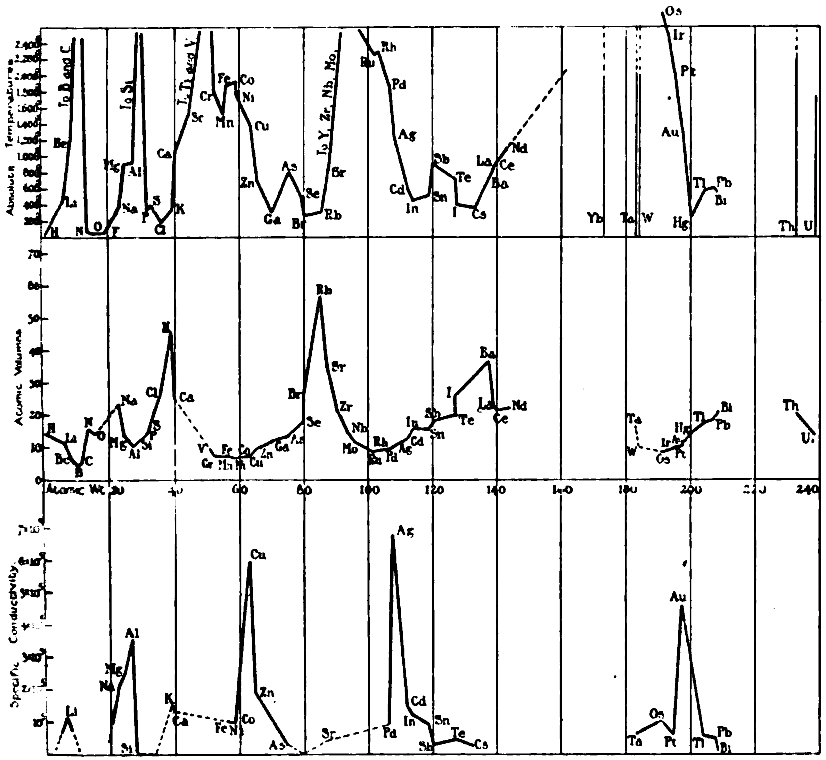


FIG. 1.

In making metal or carbide filaments there are several processes.

A carbon filament may be made first, and this may be coated by electrical heating with other substances, and the carbon can then be volatilised.

The same process can be carried out except that the carbon, instead of being volatilised, is combined, making a carbide. A wire may be deposited electrolytically from a solution.

The material may be ductile, in which case it is drawn into wire in the usual way.

The material may be made into a very fine, smooth powder, and mixed with some agglomerant, and squirted. The squirted filament is then dried, baked, and heated electrically to get rid of carbon if an organic binding material has been used.

An oxide may be mixed with carbon and made into a paste and squirted. The filaments, after baking, are heated electrically *in vacuo*; the carbon then reduces the oxide, making either metal or carbide according to the proportion of carbon employed. The carbon may be supplied by heating in hydrocarbon vapour.

The oxide may be made into a rod and run like a Nernst lamp, except that the process is carried out either *in vacuo* or in hydrocarbon gas, so that the oxygen is removed, leaving metal.

These are the most likely, or the least unlikely, ways of making filaments. There are others which figure in patent literature a good deal. The laws of nature, as revealed in patent specifications, are, however, so extraordinary that I do not feel justified in confusing your minds with them.

The accounts in the technical papers of the methods used by the inventors may not always be absolutely true in detail, and they frequently have quite a suspicious flavour of patent specification.

The plan of coating a carbon and then volatilising the carbon by heat has a heroic ring about it, which promises a lamp capable of very high efficiency; but I want to see it done. Most metals are likely to form carbides rather than let the carbon go off.

The Cruto lamp of about 1884 was supposed to be made by the converse process of depositing carbon on a fine platinum wire, and then heating the carbon till the platinum boiled off. The actual filaments were of such low specific resistance that platinum must have remained there.

The electrical deposition of metal filaments is generally troublesome because most of the metals that might do for lamps will not deposit uniformly enough, and electrolytic deposits of hard metals are not capable of being drawn down into wire. For such a process to be successful the core or mandrel must not be made of a metal that will form an alloy. It need not necessarily be metal at all; presumably such a material as nitrocellulose coated with blacklead, or made conductive by any of the ordinary methods, would serve. The resulting filaments would be tubes, of course.

Drawing down fine wire is generally impossible, as few of the metals are ductile. The drawing down of tantalum by Messrs. Siemens & Halske is a perfect triumph when not only the real but the reputed properties of that metal are considered. Whether they draw the metal down by itself or by the Wollaston method I do not know. Tantalum is not soluble in the ordinary acids.

The metal filament is very seriously handicapped by its softness when hot and its brittleness when cold. Many of the metal lamps will run in only one position, otherwise the wire sags. The wires are also often very fragile when the lamp is not in use.

It might be urged that alloys should be used for lamp-wires, on the ground that many brittle and intractable metals may make ductile alloys. There is another point; alloys have generally high specific resistances, and this is, of course, very important. Even a very small addition of another metal may increase the specific resistance very considerably. Unfortunately alloys generally have low fusing points. Where a very fusible and a refractory metal are alloyed, the addition of the refractory to the fusible metal generally first lowers the melting point. The melting point then rises in one way or another, according to whether definite combinations occur, until the melting point of the refractory metal is reached. If the metals have about the same melting point, their alloy may be taken as having a lower melting point. This is not a law, however. The alloys of antimony and aluminium have much higher melting points than the component metals. Starting from pure antimony, the melting point falls a little on the addition of a small percentage of aluminium and then rises quickly. An alloy of aluminium and gold has also a higher melting point than either of its components. It is possible, therefore, that alloys may be formed which have very high melting points. Perhaps metals of the same group and series may give refractory alloys. Platinum-iridium has, I believe, a high melting point. Tungsten and osmium are in the same series, but are in the sixth and eighth groups. Tantalum and tungsten are in the same series and neighbouring groups.

The addition of a very little of one metal to another may be worth while if it increases the specific resistance very considerably and lowers the melting point very little. In adding a trace of infusible to a fusible metal the first result is generally to reduce the melting point; but after a very small percentage the melting point rises very rapidly. If one begins with the infusible metal, the addition of the fusible component brings the melting point down very rapidly. In alloying two metals of about equal melting temperatures, the first addition of either brings the melting point down rapidly. The alloys with high melting points generally contain definite compounds, and compounds are often much more infusible than their components; ammonium nitrate, chloride, and chlorate are extreme but not apt examples. Arsenides and antimonides are not wholly unlike phosphides and even sulphides.

Alloying has a possible disadvantage in reducing the resistance-temperature coefficient. The rise of resistance is a most valuable property in a wire lamp, as it protects the lamp against overrunning, and allows it to be run bright on a more variable supply circuit.

One of the difficulties in squirting filaments of a paste of finely divided metals and an agglutinant is to get the metal fine enough to squirt smoothly. This is also a question of the agglomerant used. Some of these metals in the form of powder will not squirt properly; the paste comes out thin at first and then particles choke the nozzle and the squirting stops. Gum tragacanth is a very convenient material to use. It was used by Farneljehm for squirting thin threads

of magnesia, out of which he made beautiful little baskets for use as mantles on water-gas burners. These have been supplanted by the Welsbach mantles. After a metal filament is squirted it must hold together until it is heated to a temperature high enough to sinter the particles together and make the filament into a sort of wire. Obviously it must be difficult to make very fine filaments in this way; and very fine filaments are necessary for reasonably high pressures. If the agglutinant is carbon there is a chance of its being taken up by the metal and either making the carbide or reducing the melting point, just as carbon reduces the melting point of iron or manganese.

Squirting a mixture of oxide and carbon and then electrically heating to get metal does not look promising, because the resulting filament will probably contain carbon, or be a carbide, or else the metal will be only a sort of framework, as the volume of the metal must be less than the volume of the oxide plus enough carbon to reduce it. Such a filament will have a large diameter for a given weight of metal, and will most likely be very weak.

It may often be easier to squirt a very fine filament of pure oxide than of metal powder. The oxide can then be reduced, in many cases in hydrogen or even carbon monoxide, without forming carbide. In some cases I have found very fine "impalpable" powders can be made by calcining such salts as the oxalates. Prolonged kneading has a marked effect on the extrudibility of many pastes. A paste is much improved by being worked for hours in a small Pfleiderer, or by more powerful kneading. The corners of the particles seem to be ground off by attrition. If one steps on wet sand whose particles are sharp, the sand just round the foot looks dry, because the particles are moved out of their compact position, and constrain one another so that the interstitial spaces are increased, and the water is thus mopped up and drawn from the neighbouring sand. On raising the foot the sand sinks back into compact positions, and looks wetter than ever. The same thing is noticed on a small scale with (unboiled) starch paste. The action is very marked, and starch and water will not squirt well. Gum tragacanth may act partly as a smooth cushion which prevents crystalline or angular particles locking; but prolonged kneading appears to grind off the corners.

A filament made by reducing a paste of oxide with hydrogen is also more likely to be dense and strong than one which had the reducing material in its body in addition.

The method of making a squirted filament of oxide and reducing it by running it as a Nernst lamp *in vacuo* is troublesome, and does not give much promise of supplying long, thin filaments. If you consider the length and diameter of the Nernst rod for 100 volts, and compare it with a tantalum wire for the same pressure, it is evident that the method is almost hopeless. A wire the length of a tantalum lamp filament and approximately of the same diameter could not be made of oxide, and could not be run as a Nernst filament with any reasonable pressure if made.

Though metal filaments have been referred to so far, it must be remembered that there is no reason to limit the possible conductors to elements. To begin with, we have the first of the new lamps, the Nernst, made of oxides. The Nernst is now so well known, and in such extended commercial use, that there is no need to say very much about it. The stick of oxides, or glower, as it is generally called, has such a high specific resistance that it need not be long and thin to meet ordinary conditions. On the other hand, the rods cannot be made very thin, as the oxide is soft when hot, and is fragile when cold. The smallest rods made are for 0·25 ampere. On 100 volts these give about 16 candles. The makers do not make rods to take 0·125 ampere so as to give 16 candles on 200 volts. As the lamp requires a heater to start it, and an electromagnet to put the heater out of circuit, and a series resistance, or "ballast," it has always appeared to me the real field for the Nernst is not in competition with the 16-candle carbon incandescent, but in the region of 50 to 500 candles. The proportion of cost of the electromagnet, the resistance, and the heater is smaller in this case, and there is a more open field in competition with small arcs and Welsbach mantles.

I do not know what the Nernst glowers are made of now; they used to be a mixture of yttria, or "yttrite earth," with zirconia, and it is probable the same or a similar mixture is still used. It is not difficult to analyse the rod, but it is not worth while unless the Institution really wants to know. There may be a very large field for improvement here. The conditions are that the rod should stand a high temperature without getting too soft, and it should begin to conduct at a low temperature, so that starting should be easy. Nernst, no doubt, went over the ground pretty thoroughly in the first instance; but there are so many possible combinations that it looks as if there might be room for further good work. On the other hand, the lamp is of course patented, so there is not much encouragement for workers to make improvements. It may be thought, however, that it would be better to employ some black substance, such as some oxides or sulphides which conduct even cold, so that preliminary heating might be omitted. It is a very curious thing that there does not seem to be any such black or coloured compound in existence. There is a curious phenomenon in this connection. Some black or dark compounds, such as oxide of copper or oxide of vanadium, made in the ordinary way conduct well when cold, and can be heated electrically until fused, when they continue to conduct, of course; but if they are now allowed to cool they no longer conduct. Heating has changed their physical or crystalline state in some way. Both oxides of copper and vanadium are far too fusible for lamps. I do not think there are any promising sulphides. Most of the sulphides are easily fusible. As to the melting points of the tellurides and selenides, I am sorry to say I can give no information, as I am not at all well acquainted with those compounds.

It may be supposed that a Nernst glower should be run *in vacuo*, so

as to avoid the loss by convection. The glower appears to conduct electrolytically exactly in the manner of a fused salt; and it would further appear that with a direct current oxygen would be formed at one terminal and zirconium at the other, and that in a few minutes the glower would be entirely reduced to zirconium if the oxygen were pumped out as liberated. What really happens is that the lamp goes out. It does the same in an atmosphere of coal gas. This may be because the zirconium is liberated in a modification which does not conduct. I will return to this question presently.

The metals may now be discussed in the order given by the Periodic Law.

Titanium is a metal with a very high melting point. It is comparatively plentiful, especially in Scandinavia; at least, I presume it is, as some years ago the owner of a titanium property was trying to find an outlet. I could not suggest any at that time.

Zirconium is said to fuse fairly easily; on the other hand, people are said to make lamps of it. As a matter of fact, it is not easy to get such a metal as zirconium pure, and it is quite possible the melting point is very high, and that some particular alloy that was really being examined melted more readily. Nearly all the melting points of such metals are uncertain. It is not very difficult to separate zirconium from other metals, even from thorium, if trouble is taken; but the purification by precipitation is exceedingly tedious, as the hydrate does not filter well. In addition, it is difficult to remove the alkali metals completely. Zirconium lamps are made for up to 110 volts at 1 candle per watt. Whether they are made of zirconium or its carbide I do not know. Obviously, if zirconium is not very infusible, lamps cannot be made of it. Zirconium carbide I know from experience stands a high temperature.

Cerium is said to have a low melting point—below silver, in fact—and is therefore probably useless. I must repeat, however, that it is very difficult to get such metals pure. Cerium is very difficult to get approximately pure, as it is apt to contain other metals from the same group of earths. It is ductile. I do not know if a fine wire would be oxidised quickly on exposure to air. An alloy of cerium and iron makes sparks when filed or scratched; so it is probable cerium would be difficult to work into fine wires in the moist air.

Thorium.—This metal has probably a very high melting point. Thorium is in great demand for gas mantles, and the demand has created a good supply, so there would be plenty for making filaments. I have tried running a thorium "Nernst" glower *in vacuo*. It can be started either by coating with platinum, by dipping in chloride and heating, or by a Ruhmkorff coil. A direct-current electrolyses it too quickly, and an alternating too slowly, so I superposed one on the other without the companies making any objections.

After some time the rod conducts when cold, and apparently consists largely of thorium, but there is difficulty with the connections. This method is applicable to the reduction of a large number of oxides.

Boron.—Some work has been done on coating carbons with boron, but I have never heard that it was successful. Neither do I know if boron conducts at all. Of course, many bodies that are really conductors will not conduct when they are in the form of fine powder, unless the powder is strongly compressed.

Silicon.—In the early days of the carbon lamp, silicon was regarded as a possible rival, and some work was done on it. I do not know the result. The tetrachloride of silicon is a volatile liquid. I remember trying to deposit silicon on carbon filaments about twenty years ago by heating them electrically in an atmosphere of gas and silicon chloride, the idea being that the hydrogen of the gas would take the chlorine, and the carbon would get coated with silicon. I got no results. Much work has been done on these lines since, but I have not heard of any satisfactory result; and I have no reason to suppose they made better lamps than pure carbon. Of course, silicon deposited on carbon might form a coating of silicon carbide. Carbides were not in fashion in those days, and it never occurred to me to look for them. Some years later a great deal of work was done on deposition of silicon from the chloride by Langhans.

In old days silicon was not a familiar substance, and, even if you made it, there seemed to be no way of making it into filaments. Now it is a commercial article. But this does not help us much, as, according to modern authorities, it is fusible at too low a temperature to be any use in this connection.

Vanadium.—This seems a likely metal, but the fusing point is not at present known. It is not expensive.

Niobium.—Very little is known about the physical properties of this metal. It probably resembles tantalum pretty closely.

Didymium.—This has been separated into prazeodymium and neodymium by Welsbach; so the melting point of didymium, if determined, would only be that of an alloy. Didymium can only be separated into its elements by very long and tedious fractionation, and it is therefore practically impossible to get the metal pure enough to have a high melting point.

Tantalum is an exceedingly hard metal, and in its pure state is ductile. I do not know that any one knew it was ductile before Messrs. Siemens & Halske's chemists tackled the matter. It was generally known as a powder only. It has a specific resistance of 16·5 microhm centimetres cold, or about 85 at the temperature of a lamp. It has a high tensile strength, namely, 93 kg. per sq. mm., or 59 tons per sq. in.

It is drawn into wires of 0·05 down to 0·035 mm.; the large wire gives 25-c.p. lamps on 110 volts. Such a lamp has a filament 65 c.m., or 25½ ins. long, and a pound of tantalum will make 20,000 of these lamps (Böhm). The tantalum is melted in an electric arc or furnace, and the ingots are heated red, and hammered into sheet, and the sheet is drawn down into wire. Whether it is drawn plain, or by a special method, there is no information. The metal is so hard that a diamond

drill, run at 5,000 revolutions per minute for three days and three nights without stopping, only made a depression of a quarter of a millimetre or so. This experiment has been quoted a good deal, but one would like to know the state of the diamond drill. It may have lost its teeth in the first half-minute. Messrs. Siemens & Halske intend to make various uses of the marvellous properties of tantalum, and we will probably soon have tantalum pens, drills, cutting tools, unoxidisable springs, and other desirable objects.

Messrs. Siemens & Halske do not recommend their lamp for alternating currents, and it cannot therefore be as good as on direct-current circuits; but it runs on alternating currents very well, though perhaps not as long. On the other hand it is said that the alternating current alters the physical nature of the wire, rendering it brittle, so that it breaks, not by fatigue, but by any slight shock it happens to get. The Nernst lamp has shown peculiarities in this connection. Some glowers will run with direct and not with alternating currents, and some with alternating only. Direct current glowers also go wrong if the poles are changed. These mysterious properties of the Nernst do not seem to have anything to do with the behaviour of the tantalum lamp, however, but all the same there may be something in common. It is said that the makers have now overcome the aversion of tantalum lamps to alternating circuits. I have heard of tantalum lamps running on the County of London alternating circuits for over 600 hours with no failures. The breakages may be due to the trembling action due to the repulsion of the wire near the bends. This will most likely be overcome by making the zigs and the zags so that the wire does not come close to itself. At present they are very acute angles. The wires are often in movement through a sort of Trevelyan rocker effect at the points of contact of the wire with its numerous supports.

Chromium is largely used in making chrome steel, but the metal itself is not often seen. It can be reduced from its oxide by aluminium. Its melting point seems to be high. I know of no work done on chromium filaments

Molybdenum is very similar to tungsten, and unless the melting point is lower than that of tungsten, we shall probably soon have molybdenum lamps. The trioxide (the anhydride of molybdic acid) is volatile, and might therefore be used for deposition of molybdenum on a carbon filament, so as to replace it.

Tungsten is a very hard and brittle metal, which is sold in the form of a black powder, or as ferro-tungsten. It was for a long time considered infusible, but the electrical furnace showed, of course, that it could be melted. The powder is difficult to squirt, even mixed with a good deal of tragacanth. Kuzel has invented what seems to be an admirable way of getting over the difficulty. He gets the tungsten in the form of an exceedingly fine powder by employing a method that was used by Bredig for getting what is known as colloidal platinum. An arc is made to play under water between tungsten electrodes, and this is said to produce a very finely divided form of

metal. This is collected and worked up into a stiff enough paste and squirted. Tungsten is not an expensive metal, so the only cost is in making the filaments. Whether the filaments of this paste can be squirted so as to be fine enough for 200 volts will be a matter for the future to decide.

There have been processes proposed or worked in which carbon is heated in a vapour of a volatile tungsten compound, such as the trichloride, or oxychloride, in the presence of hydrogen. If the reduction in the case of a chloride is due to the hydrogen, thus involving no oxidation of the carbon, a filament must be obtained of carbon coated with tungsten, or of carbide of tungsten; but if there is any oxygen involved in the reduction, as in the case of the oxychloride, the carbon is burnt out, and the result is a filament of tungsten.

Uranium.—It is doubtful if the melting point is sufficiently high.

Ruthenium is like osmium, but is said to fuse at a slightly lower temperature.

Palladium is the most fusible of its group, and is therefore useless for lamp filaments.

Osmium is a crystalline metal which cannot be drawn into wire. It is very hard, scratching quartz. The Welsbach osmium lamp is said to be produced by making a paste of finely divided osmium and an organic binding material, and squirting it. The filaments are then baked, and heated electrically to a very high temperature to eliminate the carbon. The osmium lamp so far produced is for low pressures, as might be expected, but it has a very high efficiency.

To make lamps of osmium must be an exceedingly difficult matter, or, at any rate, the problem of how to make them must have been very troublesome to solve. Apart from the metal being very hard and infusible, it oxidises in the air if very fine, though this oxidation does not take place if the fine powder has been heated to a high temperature. The peroxide of osmium formed is very poisonous. It gives off enough vapour at ordinary temperatures to cause much trouble, and especially to injure the eyes very seriously. This oxide might be used for replacing a carbon filament with osmium. No doubt that has been tried. The osmium was, I think, the first of the new metal lamps, and was invented by Auer von Welsbach. It is made up to 75 volts with 40 candles by the Vienna firm, now the Westinghouse-Metallfaden-Glühlampfabrik, but 100–130-volt lamps of only 32 candles are supplied by the General Electric Company of this country. The rated efficiency is 0·8 candle per watt.

The osmium wire is said to be as small as 0·03 mm. diameter, which is rather less than the finer tantalum wire.

There is some doubt whether the lamps known as osmium are made of osmium or an alloy of osmium with tungsten. Tungsten is a curious metal, and it is not very easy to get into alloys, but it may alloy with osmium perfectly for all I know to the contrary.

Iridium is a very hard and brittle metal. Iridium lamps are being studied by Gülcher. He makes a paste with an organic binding

material, and squirts it. The filaments are then heated electrically until the carbon is burnt out and some particles of metal have sintered together. This lamp, like other metal lamps, labours under the disadvantage of being only for low pressures. It would seem that squirted metal filaments are less likely to be made fine than those made from drawn wire.

The carbides of the various elements remain for discussion. It is difficult to know which have high enough melting points. A great many are said to be transparent, and are therefore non-conductors. Among these are the carbides of aluminium, cerium, lanthanum, lithium, silicon, thorium, and yttrium. A great many decompose water, or are acted upon by the moisture of the air. This would make it very difficult to make lamps of carbide of aluminium, barium, calcium, cerium, glucinum, lanthanum, lithium, manganese, strontium, or uranium. Among the possible carbides are those of boron, chromium, molybdenum, silicon, titanium, and zirconium. I do not know anything about the carbides of palladium, tantalum, or tungsten.

It will be asked, What will be the effect of these new lamps on the industry? In the first place they will increase the output of stations, just as machinery increases labour. But there is more difficulty in foreseeing the result of high efficiency hampered with low pressure. A probable solution is that people will gradually take to using large lamps taking the same pressure, and about the same power as carbon lamps, but giving, say, four times the light.

As to the lamp-making industry, one might prophesy without much danger that the present makers will merely alter their manufacture and make metal lamps. This will pay inventors better, because the existing makers have their commercial organisations and their facilities [redacted]

Dr. Harker's work in preparing the table shown and has discussed it to some extent, I will call on Dr. Harker to explain the table a little more. After that, we have a number of gentlemen with us who have come from a distance, and, if time permits, I hope to ask them to speak. The President.

Dr. J. A. HARKER: It would be possible to say a good deal on this very suggestive paper of Mr. Swinburne's, but I will endeavour strictly to limit my remarks. Mr. Swinburne very justly complains of the lack of physical data regarding a great number of the bodies he is dealing with, but he shows us how to make the best possible use of those we have, by utilising Mendeléjeff's periodic system, given on page 216. A diagram is also shown giving the melting-points of many of the elements on the absolute scale. For some time I have been interested in the physics of high temperatures, and have gradually been accumulating material for a paper on the melting and boiling-points and properties at high temperatures of refractory materials generally. I thought it would be of interest for the purpose of this discussion for members of the Institution to have in the form of a fairly complete table what was known regarding the melting-points of the elements on the Centigrade scale. Only within the last six years has our scale of high temperature been at all definitely investigated, and in my opinion there is still great uncertainty attached to the higher temperature-measurements made by optical methods. In our work at the National Physical Laboratory we endeavoured to get a kind of "benchmark" for use in the preliminary mapping out of the really high-temperature region (above, say, 1,500° C.), by making a new determination of the melting-point of platinum. I cannot now go into detail, but it will suffice to say that the value found for the platinum point was 1,710° C. —70° lower than the old accepted value of Violle, depending on a determination made twenty years ago. This lower value has been confirmed in Germany, and even though subsequently it may be found to need alteration, if we know that a substance melts a hundred degrees higher or lower than the platinum point, it will be easy to apply the required correction. The data on the table are some of them taken direct from the papers of the various authors. To others corrections have been applied when it was possible to revise the standard points on which they depend, and others again are new determinations made at the Laboratory. Boron and carbon at ordinary pressures do not melt before volatilising. In Group 4 titanium melts in the region of 2,500°, but zirconium is quite low (1,300° C.), cerium still lower, while for thorium, whose melting point is undoubtedly very high, I can find no record of any determination. Didymium, which, as the author points out, is the next-door neighbour of tantalum, and therefore interesting, has been split up into neodymium and prazeodymium, which both melt quite low. The data in the eighth group are many of them very uncertain. Iridium, which melts above 2,000° C., gives off a great deal of vapour far below this temperature. It would have been possible, in view of the interest of lamp makers in refractory oxides and carbides, to have made a similar summary of some of their melting Dr. Harker.

Dr. Harker.

[redacted] F THE ELEMENTS—CENTIGRADE SCALE.*

[redacted]	V.	VI.	VII.	VIII.	IX.
[redacted]	N -215	O − 235	F − 223		He Ne
	P 44 V ,620	S 114 Cr 1,600	Cl − 102 Mn 1,230	Fe 1,503 Co 1,500 Ni 1,427 (Cu) 1,084	Ar
	As 500 Ni ,850	Se 250 Mo	Br − 7	Rh Ru 1,900 Pd 1,520 (Ag) 962	Kr
	Sb 632 Di 40Pr } 40Ne }	Te 450	I 116 Sm 1,350		Xe
	Ta ,150	W above 1,900		Os 2,200 Ir 2,250 Pt 1,710 (Au) 1,064	
	Bi 269	U			

[redacted] on which many of the figures were calculated has been revised (see p. 265).

points. There are quite a number of data of a sort, particularly as to relative refractoriness. There are two questions I should like to ask the author as an experimentalist of wide experience. First, Can he tell us anything, or refer us to any literature, regarding the methods adopted in difficult wire drawing, such as is necessary when a metal like tantalum is made into filaments? Second, Does he know of any really refractory material that can be said to be an insulator at very high temperatures? My difficulty has been to find a refractory material which by judicious treatment it is not possible to make into a sort of Nernst lamp. Almost all pure oxides conduct fairly well if only the temperature can be got high enough to start them. Some kinds of ordinary porcelain rod are fairly easy to make incandescent by their own current, and I have succeeded in making a bit of ordinary churchwarden pipe run as a lamp for half a minute or so on a 500-volt circuit. I will conclude with one further remark and an experiment. On page 226 Mr. Swinburne refers to carbide of silicon. The best known carbide of silicon is, of course, carborundum. This material squirted into filaments in the ordinary way conducts slightly, but, in the coherent crystalline form, quite well, and its use as a lamp filament was patented by Sir Joseph Swan some years ago.* Its resistance falls with rising temperature. But as a lamp of really high efficiency it suffers from a disadvantage which the experiment I propose to show will make evident. At about 1,900° C. the affinity between carbon and silicon, whether in air or in vacuo, becomes very small, and a hundred degrees higher the silicon distils out and burns, forming a cloud of silica vapour leaving in a few moments only carbon behind. Dr. Harker.

Professor GISBERT KAPP: I have tested some of these new lamps. I understand that carbon and Nernst lamps are not to be mentioned in the discussion, but perhaps I may be allowed to refer to them as starting points to compare the metallic filament lamps, namely, osmium and tantalum, with them. I found that the osmium and the tantalum lamps had about the same consumption per candle; it was of the order of 1·7 to 2; and the Nernst was also of this order. But when the lamps are compared with regard to their practical use, to which the author has drawn attention in his paper, namely, their use on a circuit which cannot be kept with mathematical precision at the same voltage, then there is a difference between these lamps. I have taken the Board of Trade latitude in pressure of 4 per cent. up and down. Luckily, electric light companies do not as a rule make full use of this latitude, but they may do it, and a ± 4 per cent. variation in the standard pressure is therefore a convenient basis for comparison. With the carbon lamp, a rise of 4 per cent. of the pressure which is written on the glass will raise the light on an average by 25 per cent.; if the pressure drops by 4 per cent. the light drops by 20 per cent. Professor Kapp.

* A speaker at a later stage in the discussion seemed to misinterpret this remark as to the use of carborundum as a lamp filament. What I stated was that it would not make a "high-efficiency" filament because if run at 1 or 1½ watts per candle, the temperature of the material would be too near its dissociation point. I was aware that it had been used for lamps of ordinary efficiency.

rofessor app.

The tantalum lamp is better in this respect, the 4 per cent. rise of pressure only raising the light 18 per cent., whilst a drop of 4 per cent. decreases the light by only 12 per cent. The osmium is between the carbon and the tantalum; the figures I found being + 22 per cent. with the higher, and — 18 per cent. with the lower pressure. I am sorry to say I cannot give you exact figures with regard to the Nernst lamp, but it is worse than the carbon. Although the Nernst lamp is very much more efficient than a carbon filament lamp, it has its disadvantages—the disadvantage of being more sensitive to variations in pressure. It has, however, one advantage, namely, great intrinsic brilliancy. For galvanometer and lantern work, and in fact in all cases where one wants to focus the light, the Nernst filament is a very convenient source of light, because it gives a very brilliant, concentrated light. The fact that the Nernst and the tantalum have about the same efficiency is surprising, because the temperature at which they work is not the same. I have not myself made tests on the temperatures, but taking the figures which were determined by Lummer about three years ago, the efficiency of the Nernst ought to be much higher than it actually is. Perhaps I might be allowed to mention the way in which Lummer obtained the temperature. He took the energy curve for the light, and determined the wave length for the highest ordinate. The product of this wave length and the absolute temperature is a constant. For the "black body" this constant is 2,940, and for molten platinum (the nearest approach to a perfectly reflecting body) it is 2,630. By these means it is possible to calculate the temperature of any source of light within the limits pertaining to the black and reflecting body. We do not know whether the incandescent filament has to be classified as a black or a shiny body, but we know

[redacted]

Professor Kapp.

lamps, modern metallic filament lamps, I found that the light given out was very nearly as the third power of the watts put into the lamp; for the carbon it was as the 2·7th power. There is an explanation for this discrepancy. If a carbon lamp is overrun, the lamp deteriorates while the experiment is in progress, and this may account for the reduction in the exponent from 3 to 2·7. I mention this fact to show that, notwithstanding this discrepancy, Guilleaume's law that the light varies as the twelfth power of the temperature may be accepted as approximately correct. If we put 1 per cent. more power into a lamp we get 3 per cent. more light. With carbon lamps that has been long known, but with metallic filaments and Nernst lamps it was not so obvious at first. If Guilleaume's law is true, then the Nernst lamp, having 16 per cent. more temperature than the carbon, ought to have 1·16 to the eighth power more light, that is to say, it ought to give 3·3 times as much light as a carbon lamp which takes the same watts. As a matter of fact, it only gives about 2 times as much light, and the question is, How can we explain this apparent falling off from a law which we saw from another experiment must be true? I believe the explanation lies in this, that part of the power goes in heating the support of the light-giving filament. In the case of the Nernst lamp you have a very short pencil of a refractory material which has to be held fairly close to a china plate, and has to be surrounded by a heating spiral. The support and the heating spiral take away a good deal of the heat which otherwise would be utilised in the Nernst pencil; and although the Nernst lamp may have the temperature determined by Lummer, namely, something like 2,300°, it will not give the light in proportion to the power it absorbs, because a good deal of the power goes to low temperature heating. That heating spiral is only used at first, and then is automatically switched out. But it remains in close proximity to the pencil and takes heat from it. Another reason of loss is the ballast resistance.

Perhaps I may be permitted, although I am going slightly beyond the limits of metallic filaments, to say a word about the question of efficiency of lamps. To define efficiency on a truly scientific basis we ought to know the mechanical equivalent of light; but this we do not know. We can, however, agree on a basis for computing efficiency if we find in nature a source of light which, having a low temperature, may be considered to have an efficiency of 100 per cent. I have gone to one of the very earliest works on that subject, namely, Langley's and Very's classic research on "The Cheapest Form of Light,"* by which they mean the light of the fire-fly. Langley and Very took energy curves of the fire-fly, the arc lamp, and other sources of light. The energy curve of the fire-fly lies wholly within the visible part of the spectrum; the ratio of visible to total surface, which I take to be the efficiency, is therefore 100 per cent. By planimeter I determined the ratio of visible to total surface of the energy curve of Langley's arc lamp and found this to be 3 per cent.

* *Phil. Mag.*, vol. 30, 1890, p. 260.

Professor Kapp.

Assuming that the arc lamps used in those days took about $1\frac{1}{2}$ watts per candle—I think that is a fair estimate for the lamps made sixteen years ago—I found the following efficiencies, translated on the fire-fly basis. The Welsbach incandescent mantle has a little under $\frac{1}{2}$ per cent. efficiency. The carbon filament lamp has either 1·3 or 1·8 per cent., accordingly as it is a long life or a short life lamp. The tantalum, according to the test of which I have previously spoken, has 2·5 per cent. The osmium lamp has the same efficiency. The ordinary open arc has 4·5 to 5 per cent. As regards flaming arcs, I have not made tests myself, but taking the figures published by Mr. Patchell in the discussion on Mr. Andrews's paper (*Journal*, vol. 37, 1906, p. 36), I find that their efficiency varies between 10 and 22 per cent.

The President.

The PRESIDENT: We are very much indebted to the makers of a number of these new lamps, who have organised for us a most interesting exhibition in the room; and I personally am more indebted to them because they have given to Mr. Paterson, of the National Physical Laboratory, permission to explain here a number of tests that he has been making of these lamps during the past six or nine months. We shall, however, in view of the time, have to postpone that until this day week. Professor Thompson has kindly arranged for us a small experiment, and I now call on him to show it to us.

Professor Thompson.

Professor SILVANUS P. THOMPSON: The experiment that I have arranged cannot be shown to the whole audience. It is simply an exhibition of filaments under a microscope, and therefore will have to be looked at individually after the meeting is closed. For some time in my house at Hampstead, where happily I enjoy what I wish everybody else enjoyed, 100 volt pressure, I have been trying a number of metallic lamps in order to gain the common experience of a householder with lamps on a circuit in everyday use. I have four osmium lamps, which have been in their place now for nearly eighteen months; not one of them has blackened, and they are going now. I have had six tantalum lamps since August last. Two of them are still alive; one of them on the table here is dead, and blackened a good deal before its death, or at its death, and three others have gone the same way. It occurred to me that it might be worth while to examine what had happened where the wire parted. I wanted to see the way it was broken off, or fused off, or what had happened. I therefore took to pieces one of these lamps, and put some of the bits of wire under the microscope. I have a few microscopic slides here prepared in that way of the filaments of the tantalum lamps and some carbon lamps for comparison. What astonished me very much was the entire difference between the appearance of the tantalum wire and the carbon filaments. The carbon filaments were all taken from old dead lamps, and they were practically all good cylindrical threads. I have not examined any old Edison lamps with filaments of bamboo. But the tantalum lamp is a wholly different thing to look at under the microscope. As the members will see for themselves, the filament looks more like a badly jointed bamboo rod.

Discussion at Meeting of January 17, 1907.

Mr. Paterson.

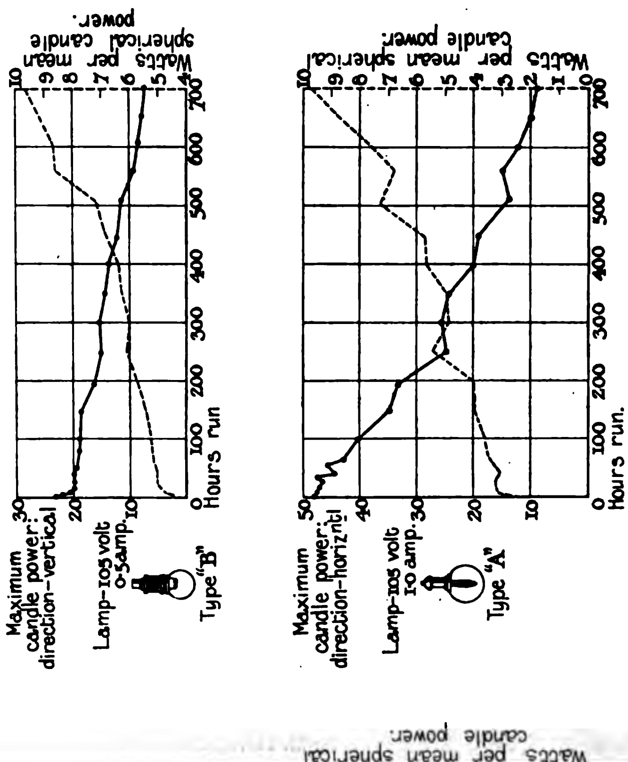
Mr. C. C. Paterson: We have long wanted a general survey such as this paper gives of those elements from which it is possible, or probable, that lamp filaments may be made. For reasons which he has given, Mr. Swinburne touches but lightly on the way the new lamps behave on life test both as regards candle-power drop and ability of the filament to withstand rupture. We have had occasion at the National Physical Laboratory from time to time to carry out life tests on some of these lamps, and I am able, by the courtesy of those for whom the measurements were made, to show some diagrams which embody the results of these tests (Figs. A, B, C, D, and E). It is probable that they represent average performance, and doubtless lamps would be found of each type which would give both better and worse results. I have not had occasion to make tests on the osmium lamps, but have some figures for "Osram" lamps up to about 200 hours. The figures given are approximately the average for each type of lamp. The lamps compared are

Test on Incandescent Lamps.

Lamp.	Volts.	Initial C.P.	Initial Efficiency.	Total Watts.	Drop in C.P. after 1,000 Hours, Per Cent.
Nernst	200	25 M. Hemisph. C.P.	2·2	56	70
Metallised Carbon (B.T.-H.)	100	16 M. Horiz. C.P.	2·8	45	20
Tantalum	110	23 M. Horiz. C.P.	1·9	44	35
Zircon	37	48 M. Horiz. C.P.	1·0	50	37
Early Lamp, April, 1906.					
Osram	100	25 M. Horiz. C.P.	1·4	35	—
Generally recognised figure for ordinary Carbon Lamps :--					
Carbon	100	16 M. Horiz. C.P.	3·6	57	20
"	200	16 M. Horiz. C.P.	4·3	69	20

in all cases those with the lowest candle power which can be commercially made for house lighting. Although the Nernst lamp shows the greatest drop in candle power, it must be remembered that it is the only one of these lamps which will run on a 200-volt circuit. The zircon lamp is of the earliest type, on which I understand there have recently been great improvements. The generally accepted figures for

Mr. Paterson.



[redacted] Volt Nernst Lamps—Life Curves.

Mr. Paterson.

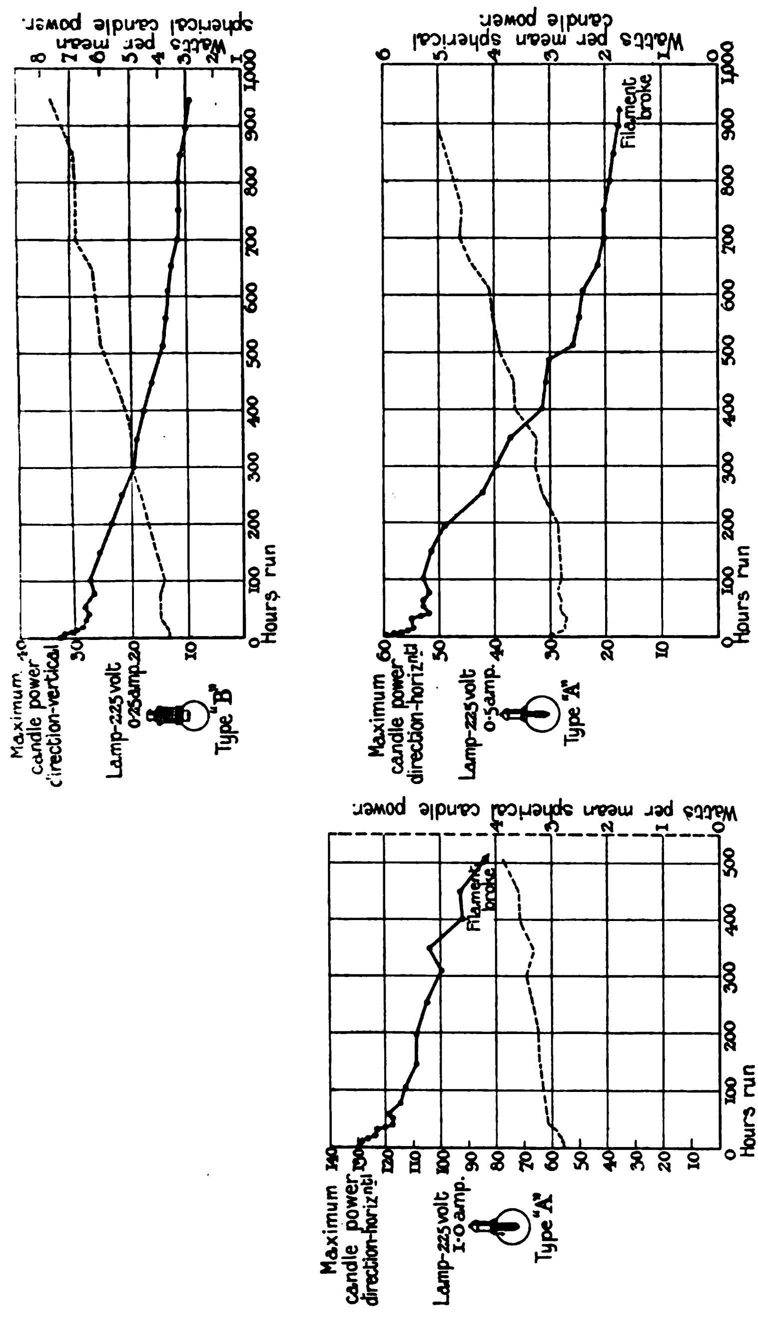
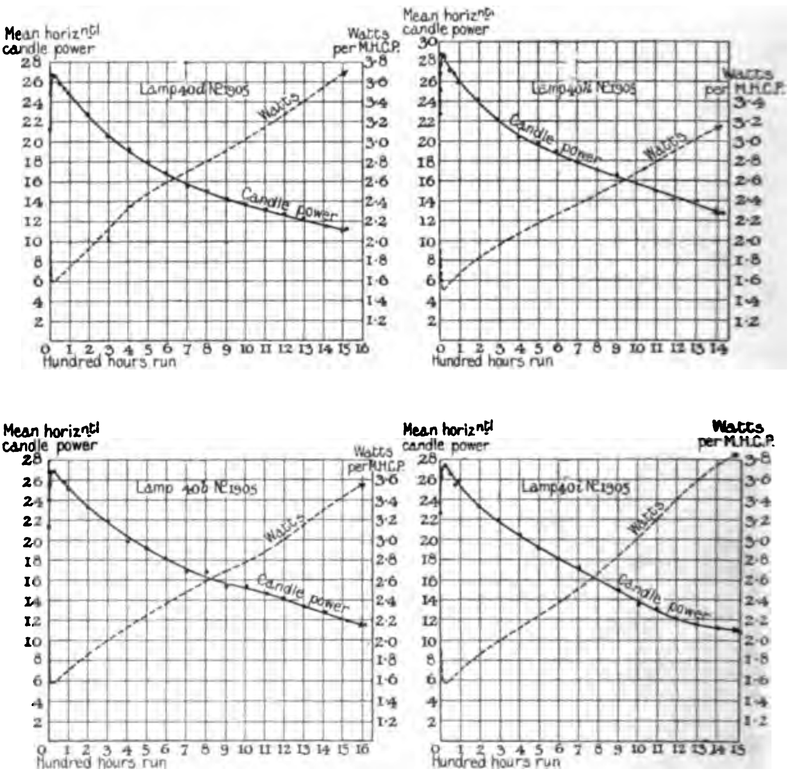


FIG. B.—225-Volt Nernst Lamps—Life Curves.

Mr. Paterson.

ordinary carbon filament lamps are shown at the foot of the table for the purpose of comparison.

The column of total watts is noteworthy as showing that although the efficiency is high the candle power is also high, which causes the total watts taken by all the lamps to be of very much the same order of magnitude. Another interesting point connected with the metal filament lamps is that the candle power only varies as about the 3·5

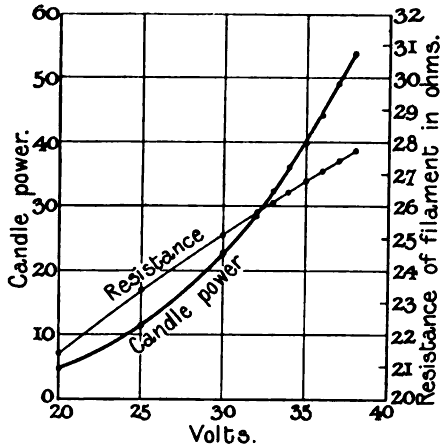


An arrow head on the curve shows point of failure of a lamp.

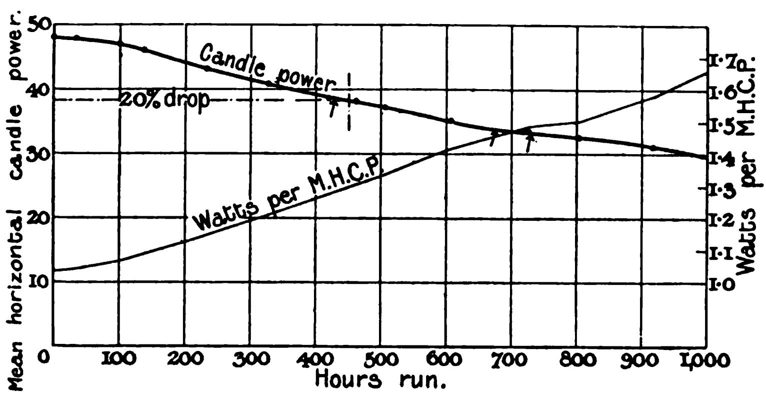
FIG. C.—Tantalum Lamps—Life Curves.

power of the voltage instead of the fifth or sixth power in the case of the carbon lamps. Although the author expresses the belief that within a reasonable time we may expect to have a 1 watt per candle 16-c.p. lamp, the difficulties in the way of making a really robust filament of this size seem very formidable, and one cannot help thinking that we shall for a long time at least have the 200-volt carbon filament lamp with us.

Mr. Paterson.



37-Volt Zircon Lamps—Curve showing Variation of Candle Power and Resistance with Voltage.



An arrow-head on the curve shows point of failure of a lamp.

37-Volt Zircon Lamps—Life Curves (Average) for Six Lamps.

FIG. D.

Mr. Paterson.

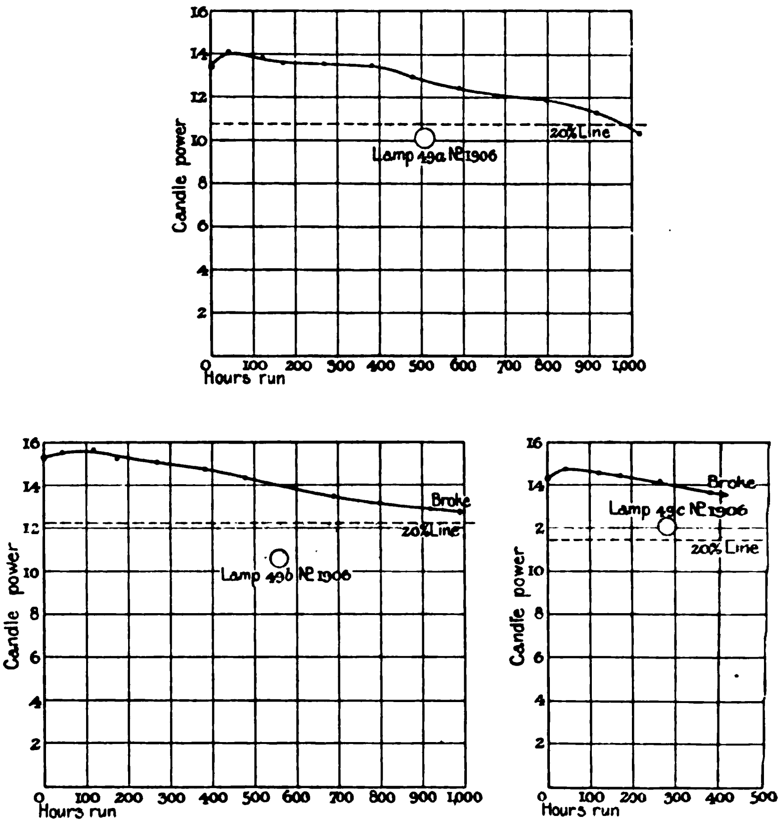


FIG. E.—Metallised Carbon Filament Lamps—Life Curves.

Dr. Thompson.

Dr. SILVANUS P. THOMPSON: The slides I have to show have been enlarged by microphotography from the specimens which I had under the microscope last week.

My attention has been drawn during the last week to the circumstance that in the *Electrician* for December 19th, Mr. J. T. Morris, in an article on new lamps, mentioned, in illustration of this fact which he had independently observed, that the tantalum filament shows numerous extraordinary junctions. The filament apparently breaks up and joins itself without ever parting, but the pieces do not join up accurately; the joints are badly healed.

A piece of zircon-wolfram filament from a 100-volt 60-watt lamp, one of those which was actually burning here in the room last week, but which gave out afterwards, shows no such kinks; it is quite smooth.

Dr. Fleming.

Dr. J. A. FLEMING: The Institution is to be congratulated on having secured from Mr. Swinburne a paper dealing so fully and freshly with

an interesting subject, and one which contains so much that is provocative of discussion. The general impression produced by the paper, I think, is the great importance of chemistry in relation to the subject of incandescent lamps. It is generally supposed that electricity affords a special field for the application of mathematics, but the paper before us is a conclusive proof that the substantial prizes are to be won by the combination of a knowledge of chemistry and electricity. The tantalum lamp is, in fact, a triumph of chemistry, and the Nernst lamp was the invention of an eminent chemist. It is not a little curious that, whilst Edison and the older inventors began by working at metals, platinum and iridium, and even tried oxides as glowers for incandescent lamps before they came to carbon, modern inventors have had to go back to metals and to oxides in order to improve upon carbon. The fact is that the older inventors did not bring to bear upon the subject the amount of chemical and electrochemical knowledge which the more modern chemist is able to do. Nothing is more astonishing in reading old Patent Specifications than to notice how few inventors or patentees realised the properties of carbon at a high temperature, and that it can reduce nearly all oxides. The President has restricted us to the discussion of new lamps. I presume, however, it is not out of order to institute comparisons between the new and the old, because it is only in that way that we can arrive at what is new, and say how far the new is useful. I should like, in the first place, to give a few figures showing the relation of light to power, current, and voltage in the case of these new and old lamps. These figures have been obtained in my laboratory by Mr. Upson and others, and are very instructive. In the case of the carbon lamp they are as follows :— Dr. Fleming.

Candle power $\propto$ (current)$^{5 \cdot 63}$.
" " $\propto$ (volts)$^{5 \cdot 95}$.
" " $\propto$ (watts)$^{2 \cdot 94}$ to $^{3 \cdot 14}$.

It is obvious from those figures that the fact can at once be deduced that within the limits over which the observations run we have—

Glower resistance $\propto$ (current)$^{-0 \cdot 054}$.

Hence, the last exponent being negative, the resistance decreases with rise of current.

In the case of the tantalum lamp the following are the figures :—

Candle power $\propto$ (current)$^{6 \cdot 22}$.
" " $\propto$ (volts)$^{4 \cdot 4}$.
" " $\propto$ (watts)$^{2 \cdot 57}$ to $^{2 \cdot 63}$.

Since the volt exponent is much less than the current exponent, it follows that the resistance variation is positive, or that—

Glower resistance $\propto$ (current)$^{+0 \cdot 29}$.

In other words, the resistance increases with the current.

Dr. Fleming.

For the osmium lamp the figures are :—

$$\text{Candle power} \propto (\text{current})^{6 \cdot 85}.$$
$$\text{,, ,,} \propto (\text{volts})^{4 \cdot 215}.$$
$$\text{,, ,,} \propto (\text{watts})^{2 \cdot 62}.$$

It follows from that that—

$$\text{Glower resistance} \propto (\text{current})^{+0 \cdot 38}.$$

It is obvious, therefore, that in all three lamps—carbon, tantalum, and osmium—the candle power varies roughly as the sixth power of the current, but by no means is it the same with the voltage. The voltage exponent is much lower for the metallic lamps than the carbon, varying very little more than the fourth power. This is a distinct advantage, because we have, or are supposed to have, constant voltage from our supply circuits. Therefore, what we are concerned with is the variation of candle power with the voltage. Hence it appears that a positive resistance temperature coefficient is an advantage in keeping the voltage exponent low. We ought, therefore, to try to obtain a large positive temperature coefficient for the material, so that it minimises the effect on the light of the variations in voltage to a larger extent than in the case of the carbon lamp. Next, as regards the electronic discharge across the legs of the carbon when the carbon lamp is worked on a continuous-current supply, one of the difficulties of making a high-voltage carbon lamp at first was this discharge across from the negative to the positive legs. Carbon is a material in which this electronic discharge is particularly large. It may amount to as much as 1 ampere per square centimetre of surface at the temperatures at which we work in carbon lamps. As I showed in 1890, it is very much less in the case of metals, and therefore in metal wire lamps the same difficulty does not present itself. On the other hand, against this we have the difficulty of obtaining small candle power. The 8-c.p. 200-volt carbon lamp and the 5-c.p. 200-volt carbon lamp are distinctly valuable possessions, and the running of two or more 8-c.p. lamps in series is by no means an equivalent. My memory goes back now in these matters as far as 1882 (and probably the author's as well), to the early days of electric lighting and the first Crystal Palace Exhibition. When Edison produced his 16-c.p. 100-volt lamp it was considered a great achievement. Then we asked for 8 c.p., and the only way he could do it was by cutting the 16-c.p. filament in two, but it was some time before the 100-volt 8-c.p. lamp was achieved. Another point that may be made against the new metallic lamps of very high temperature is this. In the case of the very high temperature glow lamps, as most of us saw on the last occasion, there is a paralysing effect upon the retina, due to the intense brilliance of the filament. The eye directs itself towards the lamps, and the moment one looks at the lamps and then looks away at something else, although the light may be brighter and more efficient one is not so well able to see surrounding objects. The common cure for this is to sand-blast the lamp bulb, or to put it

Dr. Fleming.

into a ground-glass globe. In so doing we lose the advantages gained by the greater efficiency. We manufacture light at the rate of $1\frac{1}{2}$ watts per candle, and then reduce the radiation to 3 or 4 watts per candle power by the shade. Again, the power of working lamps in a horizontal position is a very great advantage to the user. The common method of hanging head downwards is ridiculous. In the case of the ordinary 16 c.p., the light emission is not more than 9 c.p. in the axis direction, and in the ordinary pendant a notable diminution of light occurs in the downward position. It is perfectly easy to remedy that by a little

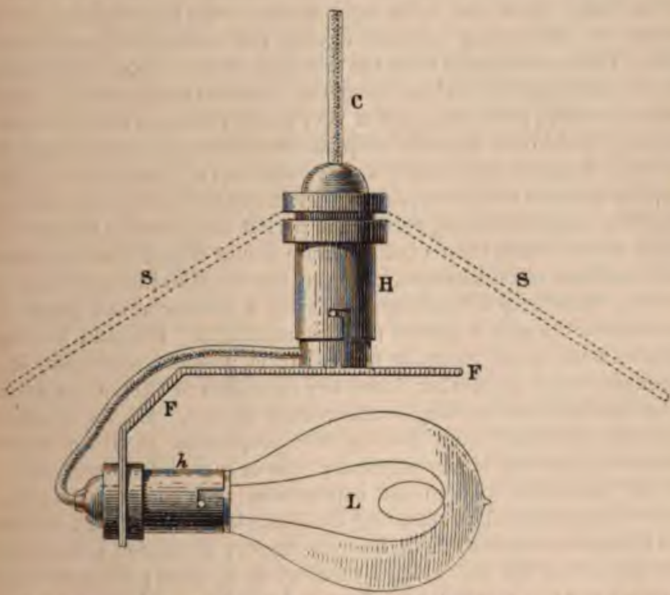


FIG. F.—Adapter for working Glow Lamps horizontally in ordinary Pendant Holders.

C. Pendant Cord. S. S. Shade.
H. Ordinary B.C. Socket.
F. F. *h*. is the Special Fitting which carries the Lamp L in a horizontal position, instead of hanging it head downwards, and so nearly doubles the illumination in a downward direction.

attachment. Instead of working with the lamp in the downward position I have devised a simple fitting to enable lamps to be placed with axis horizontal in any ordinary cord pendant (see Fig. F).

In forming an estimate of the commercial future of these new lamps, we are met by the difficulty that most of the tests yet published have been on single lamps, or on a few lamps, and it is therefore perfectly impossible to form any correct notion of the future of these lamps on such tests. I have not yet seen any target diagram of a considerable number of tantalum or osmium lamps (they may exist, but they have not come under my notice) showing, for instance, how closely a large

Dr. Fleming.

number of lamps can be made to specification, or actually to comply with their nominal or marked values. That is a very important item in estimating the commercial future of the lamps. With regard to the relative life on alternating and direct voltage, the author has given us an explanation, which may possibly be the correct one, of the shorter life of the tantalum lamp on alternating currents, namely, the mechanical stresses due to the attraction and repulsion of the filament. I should like to point out that, as regards luminous efficiency, it is possible that there may be an advantage in the metallic lamp over the carbon lamps when using alternating currents. We know that the carbon lamp gives the same light whether worked with continuous current or alternating current, having the same root mean square value. This arises partly from the fact that, at the temperature at which we are working the carbon lamp, the glower resistance has become nearly constant, or at least has a very small decrease with increase of current. But in the metallic lamp the resistance rises with increase of current. It varies very nearly as the cube root of the current. Therefore, if a metallic lamp be upon an alternating-current circuit where the voltage is periodic, during part of the period the metallic lamp will be taking more current than a carbon lamp would take at the same instant under the same circumstances, assuming the two take the same maximum current. The lamp is therefore receiving a greater mean power, and would probably give a greater mean illumination. I should like to see these measurements carefully made. It is possible there may be an advantage, apart altogether from any questions of life, in the working of the metallic filament lamp on alternating circuits. Mr. Swinburne has prudently refrained from making any prophecies as to the ultimate commercial success of any particular type of metallic glower lamp. In this he shows his wisdom and experience. We sometimes hear one invention announced as sounding the death-knell of another, but those who have some experience of the past history of applied science know that inventions do not always destroy each other; they sometimes stimulate one another to longer lives and greater usefulness. They find a particular field of usefulness which is peculiar to themselves. It may be so in this case, but it has been shown that a vast field for invention still lies open, and one full of promise, in connection with new incandescent lamps.

Mr. Solomon.

Mr. M. Solomon: The author's paper is extremely full of interesting passages, both from the general and also from the manufacturer's point of view. The question which interests us most is the probable effect these lamps will have on the industry generally. We may safely say that we all of us welcome the very great development which has undoubtedly taken place by the adaptation of metal filaments to electric lighting; but I think if we examine them fairly closely we shall see that the prospects are not quite so rosy as we may have been led to expect. On page 217 of the paper some very interesting curves are given. The upper curve shows the melting-points of various elements and the bottom curve shows the specific conductivity. I think if one

were to ask which of all these elements is the most suitable for making lamp filaments, one would be obliged to answer, from *a priori* reasons, carbon. The reason is that it not only has an extremely high melting-point—in fact, we have not melted it yet—but it shows up much better on the bottom curve than any of the other materials which are being used. It has an extremely high specific resistance, whilst all these other elements have, comparatively speaking, very low specific resistances. In searching for a high melting-point we have rather lost sight of the importance of high specific resistance, which enables us to get a lamp which is suitable for high voltage and for low candle power. The low voltage of the metal filament lamp is without doubt a very serious drawback. Remedies have been suggested, but I think the only two worth considering are, first, that central station engineers should change on to the low-voltage system in order to enable these highly efficient lamps to be used. I do not know what the general opinion of central station engineers is on this point, but with copper standing at about £110 I fear we are not quite so near the Millennium. The second suggestion is that the lamps should be run in series. Dr. Fleming has pointed out the disadvantages which were found when this was tried with carbon filaments, and it is quite clear that this system is very far from being a satisfactory one. It does not appeal to the ordinary householder, as it considerably adds to the complication, and it is not at all fair on the lamps. If two lamps are to be run in series, they should be paired as accurately as possible to run together. Although the makers may send out accurately paired lamps in the first place, when one of them fails the householder will take a new lamp from his stock and put it in without measuring the voltage, and under those circumstances it is very unlikely that the old and the new lamp will match, and the lamp which takes the lower voltage will very soon fail. The other way in which the low specific resistance causes trouble is with the candle power of the lamps. The author, in his paper, and more especially in his remarks at the last meeting, said that he thought we should change to higher light units. Although there is considerable evidence that the general public will make that change, from the fact that with incandescent gas mantles the general move was in that direction, I do not think that that is really an advance. As Dr. Fleming has said, we want the 8-c.p. and the 16-c.p. lamps. I think electrical engineers may pride themselves on having, to a great extent, educated the general public in the proper methods of artificial lighting; and good distribution of light, by using small units, is an extremely important factor in good lighting. If we are going to adopt 50 c.p. or even 25 c.p. as our smallest light unit, it seems to me it will be distinctly a move in the wrong direction from the general point of view of the benefits of electric lighting. There is one other consideration, namely, that if we are going to adopt these large light units and these low voltages, then it will be found that the metal filament lamp is not so very great an advance on what can be done with carbon lamps. We hear of an "Osram" lamp running at 1·4 or 1·2 watts per candle, and a Mr. Solomon.

Mr. Solomon.

tantalum lamp running at 1·8 watts per candle, and mentally I think we all compare them with a $3\frac{1}{2}$ or 4 watt carbon lamp. But we do not remember that the carbon lamp is a 200-volt 8-c.p. or 200-volt 16-c.p., and the metal filament lamp is only 100-volt, and is of 25 or 35 c.p. If we see what can be done with the 35-c.p. 100-volt carbon lamp burning in a comparatively large bulb, and base the comparison on current prices, then with a short-life high-efficiency carbon lamp one could almost, if not quite, equal the tantalum lamp.

There is one other point I should like to mention, and that is in reference to the burning of tantalum lamps on alternating current. It is very interesting that the tantalum lamp should not so far have given satisfaction on alternating current, but when one considers that the filament is a pure metal, it seems that it can only be mechanical difficulties, or difficulties in design, which prevent this result being attained. I remember very well when we were working in this country on the Nernst filament we succeeded in producing a very good filament for burning on direct current, but by no means could we get that filament to burn satisfactorily on alternating current. If I recollect rightly, the Westinghouse Company in America got exactly the opposite result. We found out, however, that our trouble was due to what might almost be called an accident. We had started to develop the Nernst lamp in Victoria Street on a 220-volt direct-current supply, and had confined our attention therefore to the circuit on which we could test the lamps most easily; but we ultimately discovered that, by an extremely simple alteration in the process of manufacture, the filaments which we were making could be made to burn equally well on alternating current as on direct current. I am inclined to think that when more attention is given to these new lamps for the purpose of making them suitable for all circuits, difficulties of this sort will be got over. It is merely a question of careful experiment, which I feel sure will yield to persistent attack. The last thing I should like to say is that Mr. Swinburne, not in the printed paper, but in the remarks he made at the last meeting, seemed to be under the impression that English lamp manufacturers were fast asleep. I have been connected with the Robertson Lamp Works for the past four years, and I can only say that if they are fast asleep they are having extremely expensive and laborious dreams. Perhaps English manufacturers are sufficiently conservative not to wish to come forward until they are confident that they have got the right thing for the English market, but I do not think it always follows that because they say nothing they therefore have nothing to say.

Mr. Hirst.

Mr. H. Hirst: Without my knowing that he was about to do so, Mr. Solomon mentioned one point on which I wished to dwell. Mr. Swinburne last week, in the paper which he read in this room, stated that lamp makers in this country were indifferent to the evolution of the incandescent lamp as going on at the present moment. He said he did not understand why we should sit quiet, waiting for other countries to bring us new inventions, and then acquire the patents. Mr.

Swinburne cannot possibly be informed of what the lamp makers are doing in this country, and Mr. Solomon's remarks indicate that something is doing. Mr. Swinburne also asked, Why do not lamp makers get hold of a number of superior students and engage them in research work? Has it occurred to Mr. Swinburne that the inventions which he has put before us this evening have not been the work of students in electric lighting factories? Professor Nernst, who made the first step in this direction, was anything but a student; he is a man of high scientific standing and of great physical and chemical experience. Dr. Auer von Welsbach arrived at his osmium lamp in connection with researches in quite a different direction. Kuzel and Just Hahnemann, and the other gentlemen that appear before us in connection with these tables, are anything but superior students; they are men who are ornaments to science in their respective countries. I believe we have in this country men of equal ability, but unfortunately they are generally so busy in the Houses of Parliament or in connection with expert work in the courts that the industry is deprived of their services. Mr. Hirst.

Mr. H. D. Munro: I am afraid I cannot add very much to the scientific knowledge on the subject of metallic filament lamps, but I have incidentally had a certain advantage in experience of their use which I think, at any rate as far as regards the "Osram" lamp, may be of some interest. I was able to procure some information about the "Osram" lamp early last year, and saw certain curves and tests in connection with it which led me to rely on it for street lighting purposes, especially on account of the tests which I had seen showing its reliability on certain voltages. Our supply at Exeter, from where I come, is alternating at 100 volts, and to that extent we have an advantage. I have been able to use these lamps now for about three months for street lighting purposes, and they have given a great deal of satisfaction. I am using 100-c.p. lamps fixed on tramway poles 40 yards apart, and they are giving a very good average illumination; in fact, it is better than any low candle power illumination that I personally have seen. In spite of the vibration which is natural on a tramway pole, and the ordinary variations of supply, these "Osram" lamps have an average life of more than 1,000 hours. A number of other lamps have now been fixed in side streets of the 50-c.p. size, and they are averaging even more, up to 1,200 hours apiece. When I say 1,000 or 1,200 hours, I mean the time up to which their effective candle power is within 10 or 15 per cent. of the candle power at which they started. There are something like 2,000 of these lamps in use at Exeter at present, and the result is practically what the author says in his paper, namely, that the consumer has decided to use these lamps more for the purpose of getting better light for the same money than the same light for less money. At any rate, consumers in my particular area are using all the "Osram" lamps they can get, and, as far as I have been able to ascertain, the results are practically the same as I have obtained on the street lighting. This is only a three months' test, but it is at any rate a test up to the practical working life of these lamps, and as far as it has gone Mr. Munro.

Mr. Munro. I think it is extremely satisfactory. As regards the manufacture of these lamps and the difficulties that may arise in connection with metallic filament lamps, I have brought up a few samples of a very peculiar nature which are now on the table, and I shall be glad to have the opinion of some one who knows more about the chemical peculiarities of these rare metals than I do. They are a very small percentage of the lamps, but they show a number of very curious multi-coloured deposits, blue, brown, and white, which show that some of the difficulties anticipated by Mr. Swinburne in his paper occasionally occur in the manufacture of these metallic filament lamps. As regards the tantalum lamp, I have the same experience that nearly every one has with alternating currents. Three out of four are quite a failure, and the fourth lamp has a long life, which is very remarkable when the failure of the other lamps is considered. I take it that the information which Dr. Thompson and others have given us as to the effect on the filaments of these lamps is quite a sufficient explanation of the reason why they will not last at the present time on alternating-current circuits.

Mr. Mackinney. Mr. VAL H. MACKINNEY: With the permission of the members I will put before them some results obtained from experiments on metallic filament lamps carried out last year in the Central Technical College at the suggestion of Professor Ayrton.

I first took a 100-volt, 25-c.p. tantalum lamp and a 50-volt, 25-c.p. "Osmi" lamp, ran them on a direct-current circuit, and determined their candle power for varying azimuths in the horizontal plane. The results are embodied in the curves shown in Fig. G. For the "Osmi" lamp the candle power was uniform in all horizontal directions within 3 per cent. of the maximum value, and in the case of the tantalum lamp within 5 per cent.

Next four lamps were taken, one new and one old tantalum and one new and one old "Osmi." These were tested under similar conditions for their distribution of light in the vertical plane. The ultimate standard employed, I should mention, was the 10-c.p. Harcourt pentane lamp. The results obtained are embodied in the two diagrams (Figs. H and I). In all these diagrams the numbers along the horizontal axis show the ratio of the candle power of the lamp tested to that of the standard employed. From the data which the curves represent I have calculated the mean spherical reduction factor for each of the four lamps. The results are as follows :—

TABLE I.

Lamp.		M.S.R.F.
Tantalum	New (run about 50 hours)	0·78
	Old (run about 1,000 hours)	1·00
"Osmi"	New (run about 50 hours)	0·84
	Old (run about 1,000 hours)	0·82

Mr. Mackinney.

We have therefore—

New tantalum lamp,	M.S.C.P.	=	M.H.C.P.*	× 0·78
Old tantalum lamp,	"	=	"	× 1·00
New "Osmi" lamp,	"	=	"	× 0·84
Old "Osmi" lamp,	"	=	"	× 0·82

Dr. Sharp† has given the following results for the average of ten tantalum lamps:—

Mean spherical reduction factor $\begin{cases} \text{Old lamp, } 0{\cdot}726 \\ \text{New lamp, } 0{\cdot}897 \end{cases}$

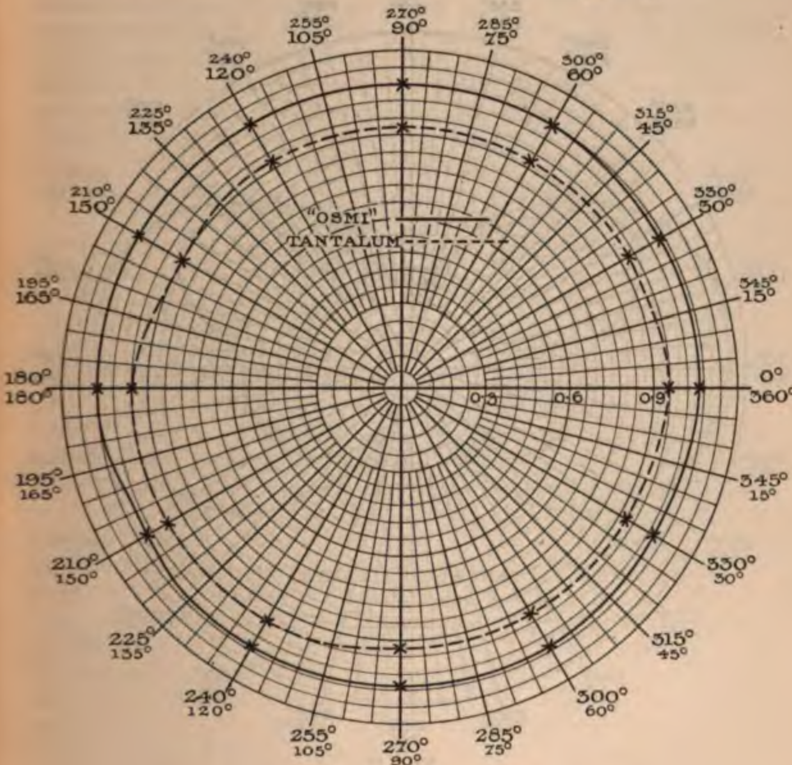


FIG. G.

A perfect comparison cannot be made between our results, as Dr. Sharp did not state the approximate "age" of his "old" lamps. However, my results fully bear out his statement that with the tantalum lamp the ratio of the M.S.C.P. to the M.H.C.P. increases with age, and therefore that it must be remembered that the M.S.C.P. falls off with age less than might be concluded from measurements of the M.H.C.P.

* M.H.C.P. refers to the mean horizontal candle power.

† *Electrical World*, vol. 47, 1906, p. 1249.

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At the Central Technical College we have also carried out some life tests on tantalum and "Osmi" lamps. The chief portion of the work was done by two of our third-year students (Messrs. H. G. Barkley and P. F. Harris). I have, however, some of my own observations made at the beginning and at the end of the lives of these lamps, and I will compare the results of the observations made with the tantalum lamps with Dr. Sharp's life-test curves on the same type of lamp.* His curves are the average of twenty

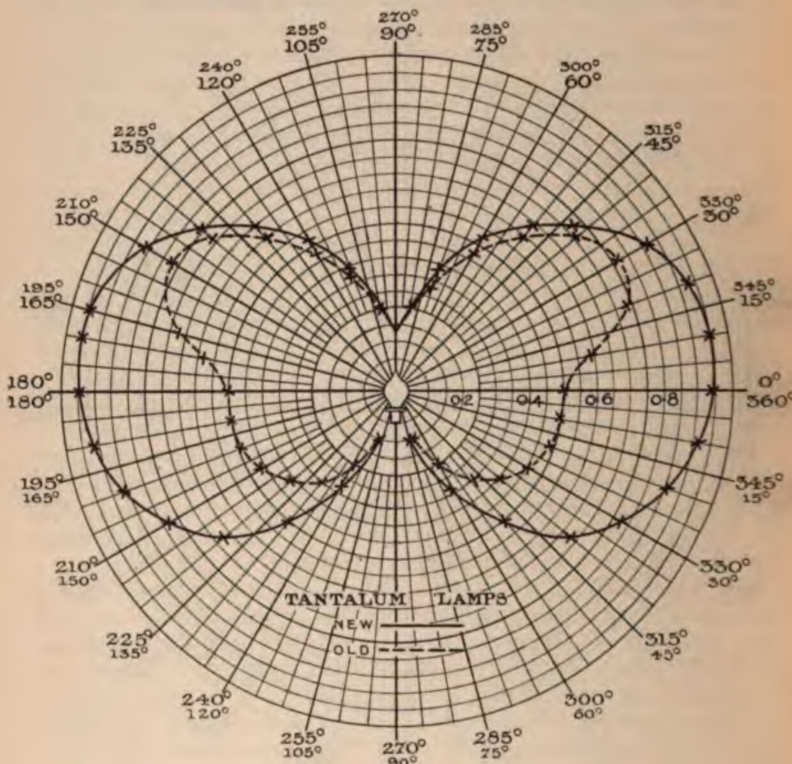


FIG. H.

lamps tested on direct current at rated volts, ours are the average of eight lamps tested under similar conditions. Taking his M.H.C.P. curve, we find that it starts at 23·5 c.p. (approximately), and reaches a point over 31 c.p. before 25 hours have elapsed. This great rise in the candle power at the beginning is probably due—as pointed out by Professor Ayrton and Mr. Medley as long ago as 1,894 in their

* Abstract of a paper read before the American Institute of Electrical Engineers, November, 1906; *Electrical Engineering*, vol. 1, 1907, p. 107.

paper entitled "Tests of Glow Lamps"—to a change in the surface Mr. Mackinney.
of the filament causing the emissivity for heat to decrease, which would raise the temperature, and therefore the light emitted, as well as the number of candles per watt. From 25 hours onwards Dr. Sharp's curve gradually drops, and his last reading on this curve indicates approximately that after 775 hours the M.H.C.P. of the average lamp drops to 16·75. Again, his inefficiency curve indicates at the start just over 2 watts per candle, then drops to about 1·3 during the first

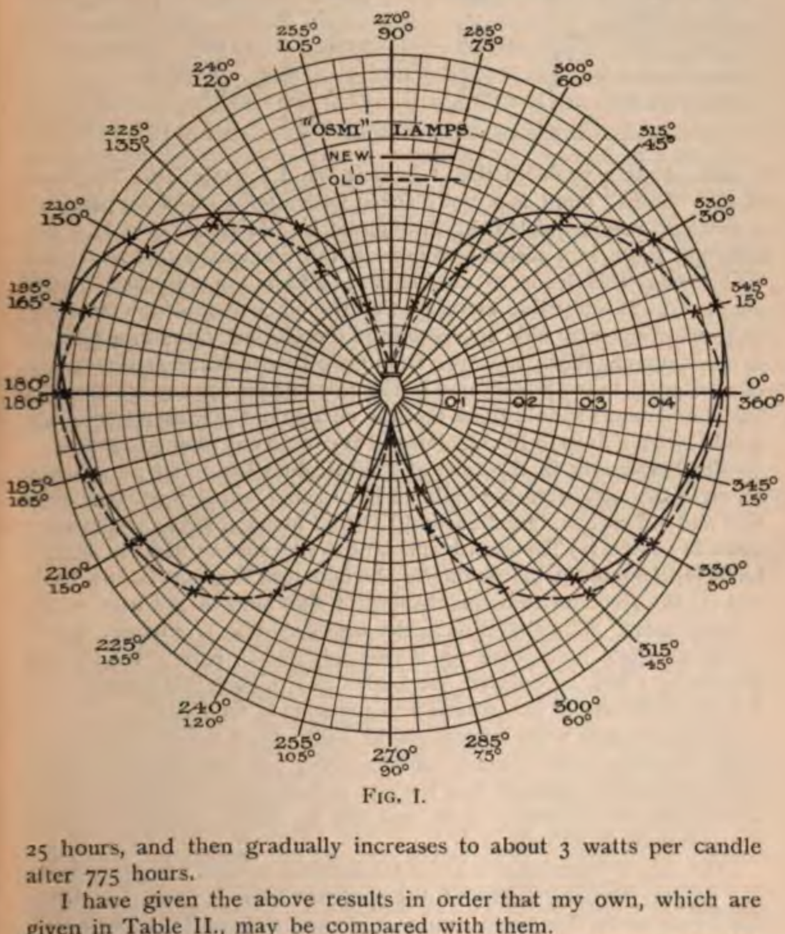


Fig. I.

25 hours, and then gradually increases to about 3 watts per candle after 775 hours.

I have given the above results in order that my own, which are given in Table II., may be compared with them.

My observations show that the average drop in efficiency for the tantalum lamp was about 17 per cent. after 1,000 hours, but whether this is due entirely to blackening of the bulb or only partially (and to some extent to a change in the filament) I am at the moment unable

Mr. Mackinney.

TABLE II.

Lamps.	Average Consumption in Watts per Lamp.	Average M.H.C.P. per Lamp.	Average Inefficiency. Watts per Candle.	Average Efficiency. Candles per Watt.
New tantalum (run 48 hours) ...	38·2	22·5	1·70	0·59
Old tantalum (run 1,012 hours) ...	38·2	18·7	2·04	0·49
New "Osmi" (run 48 hours) ...	34·9	21·0	1·66	0·60
Old "Osmi" (run 1,012 hours) ...	34·1	21·0	1·62	0·62

to say. It is a problem that I have in hand for solution. The bulb of the "Osmi" lamp does not blacken with age,* and has apparently a uniform average M.H.C.P. throughout its life (see Table II.), but although there is a distinct change in the filament with age (and a visible one) this leads to an increase, and not a decrease, in the efficiency. It is possible therefore that the blackening of the tantalum lamp with age may hide an *increase* in the efficiency due to a change in the filament. Since, however, from my own observations the average consumption in watts per lamp is the same either for a new or an old tantalum lamp, I should think that the change in the filament with age would cause a drop in the efficiency if it caused any change at all.

The blackening in the case of the tantalum lamp is by no means evenly distributed over the bulb. I have some actual photographs of new and old carbon, tantalum, and "Osmi" filament lamps which were taken to show : (1) The even blackening of the carbon filament lamp's bulb, (2) the uneven blackening of the tantalum filament lamp's bulb, and (3) the freedom from blackening of the bulb in the case of the "Osmi" lamp. The alteration in the mean spherical reduction factor of the tantalum lamp with age is certainly due in part to the uneven blackening ; but, as pointed out by Dr. Sharp, even this alteration in the M.S.R.F. may be due partially to a change in the filament.

In connection with my results in Table II., I would particularly draw attention to the average candle power per *new* (48 hours) lamp. It would seem to indicate that the 25 c.p. marked on the lamps themselves referred to Hefner units, and not to British candles, as some people seemed to think.

The author mentioned that for the lighting of such places as drawing offices the superiority of the mercury vapour lamp had been, apparently more than once, commented upon. He said that personally he saw no reason why a bluish light should not give as good results as a yellowish one ; if there was a difference he thought that the advantage

* One old "Osmi" lamp was run for ten hours at a voltage 5 per cent. above its nominal without causing the bulb to blacken in the slightest degree.

Mr. Mackinney.

must lie with the bluish illuminant, since oculists often prescribed blue glasses, for which procedure they doubtless had a reason. This statement is one of very grave importance. The subject of general illumination is receiving such a great amount of attention now that any statement indicating the superiority of a particular illuminant on account of the colour of the light it produces is likely to be quoted by one and all interested in the subject. I would point out, therefore, in the first instance, that from experiments made with one of Mr. Bastian's mercury vapour lamps the visual acuity would appear to be increased when employing such an illuminant in place of an ordinary carbon filament glow lamp, only providing the object under observation is within a distance of 1 metre (approximately) of the eye. For reading and drawing purposes, therefore, the peculiar colour of the mercury vapour lamp has its advantage. However, this advantage is not perceptible when the object is at a distance of 2 to 3 metres; in fact, at a distance of 6 to 7 metres the visual acuity is decreased in comparison with that obtained when a yellow illuminant is employed.

I cannot see that the case of oculists prescribing blue glasses permits one to assume the probability of a light of bluish tint being preferable to a yellowish one. Blue glasses are prescribed when there is an excess of yellow light, or the physiological conditions present indicate that light of certain wave-lengths in the region of the yellow will be harmful; in fact, to tone down the yellow to the natural proportion. If the light is simply too bright or dazzling, neutral tinted glasses, not blue, are ordered.

During December last I had occasion, in connection with a paper read before the Optical Society by Dr. Drysdale, to determine to what accuracy I could reproduce my readings when comparing a red illuminant against a green one with—

(*a*) A simple wedge photometer, and
(*b*) A Lummer-Brodhun photometer.

The results obtained are striking, and present a case analogous to that of the mercury vapour lamp. I have drawn my results up in the form of a table (see Table III.). First, the results are striking because of the accuracy of those obtained with the "wedge" photometer; secondly, because of the marked difference between these and those obtained with the Lummer-Brodhun photometer; and thirdly, because of the gradual rise in the candle power of the red lamp—in comparison with the green one—with the increase in voltage. When employing the "wedge" photometer the eye was focussed on an object at the distance of about 25 c.m., whereas with the Lummer-Brodhun photometer it would be focussed for an infinitely distant object, the rays entering the eye being parallel.

The results would seem to point out, therefore, that with increased accommodative power the eye becomes less sensitive to red light! This is in agreement with the observed increase in the visual acuity for near work mentioned above when the greenish-blue light of the mer-

Mr. Mackinney.

Table III.

RED — GREEN

$\frac{100}{8}$ × 0 — PHOTOMETER — 280 × $\frac{100}{8}$

Voltage.	Results with the Lummer-Brodhun.			Results with the Simple Wedge.		
	Equalisation Points.	Mean.	Candle Power (Red in terms of 8 c.-p. Green).	Equalisation Points.	Mean.	Candle Power (Red in terms of 8 c.-p. Green).
92	133·9 134·2 134·7 135·2 134·3 134·7	134·5	6·8	127·7 128·1 127·6 128·2 127·6 128·2	127·9	5·6
93	135·0 135·7 135·3 134·8 135·4 135·2	135·2	7·0	129·3 129·2 128·6 129·1 129·5 130·1	129·3	5·9
[redacted]	137·2 138·6 137·2 [redacted]	[redacted]	[redacted]	131·3 131·9 130·8 [redacted]	[redacted]	[redacted]

[redacted]

considerable extent upon the size of the retinal image it produces. Before the problem is finally solved we must know— Mr. Mackinney.

(*a*) The effect of a given alteration in the size of the retinal image.

(*b*) Whether an alteration in the power of the crystalline lens of the eye during accommodation, and the consequent increase (percentage) in the chromatic difference at the focal points, enters into the problem.

(*c*) Whether the natural limit placed upon our resolving power (owing to the finite size of the wave-length of light) enters into the problem. This natural limit depends of course upon the wave-length of the light under consideration. I find from calculation that in certain cases it might easily exceed the physiological limit, and therefore enter into the problem.

(*d*) The amount of light (in candle-metres, say) falling upon a screen at varying distances from different coloured lights whose illuminating powers have been determined equal at unit distance. In other words, we must know the penetrating powers of different coloured lights—monochromatic and otherwise.

We must also bear in mind the singular property of the eye discovered by Purkinje, which, if the illumination is carried below 1 candle metre, will certainly enter into the problem, that the intensity of sensation increases and decreases more rapidly for the red light than for the blue, for the same variation of objective luminous intensity.

The increase in the candle power with the voltage of the red lamp in comparison with the green one is peculiar. Personally, from theoretical considerations, I should have expected a result quite the reverse.

Mr. J. T. Morris: I have one or two facts to put before you as to the operation of some metallic lamps when new. Through the kindness of certain manufacturers I have been able to carry out a series of tests on a number of these recent lamps in my laboratories at the East London College. The results are given in the following table. Mr. Morris

In the first column the ratio of the resistance of the lamp hot to cold is given. In the carbon lamp it is 0·55, but in the tantalum lamp, instead of being 0·55, it is 6·3. For the osmium the figure is still higher, and also for the zircon, and so on—the tungsten lamp giving the extraordinary change in resistance between hot and cold of 12·8. The result of this is that, when these metallic filament lamps are switched on to a circuit, the current at the moment of starting is very much higher than its normal value. I have brought with me four oscillograms which have been taken of the starting currents of various lamps (see Figs. J, K, L and M):—No. I., 220-volt 5-c.p. carbon; No. II., 110 volt 22-c.p. tantalum; No. III., 110-volt 28-c.p. "Osram"; No. IV., 110-volt 35-c.p. Just-Wolfram.

Measurements taken from these give the values shown in the table, column 2, as the ratio of initial current to final current.

I have also one or two points which seem to confirm the results of other experimenters. In the last column but one (see table) the increase of candle power for 1 per cent. rise in voltage is given. For

Mr. Gaster. then, "I believe that we may in time to come improve the efficiency of the lamps by varying either B^2 (Leuchtungs-Vermoegen—lighting power) or the volatilisation temperature of the filaments, or both together, by a good combination and wise selection of the materials, and by due care in their manufacture."* That sentence elicited the remark that it is easier to prophesy than to show the lamp. I am particularly favoured to-night in being able to show a 200-volt metal filament lamp which consumes a little over 1 watt per c.p. Messrs. Dowsing Company have been good enough to lend me a few Zircon-Wolfram lamps to show in actual working. The Zircon-Wolfram lamp was shown for the first time in this country at a lecture I recently gave on the "Progress in Electric Lighting."† The lamp was then only a 37 volt, but had developed to 110 volts at the time it was shown again last August at the British Association Meeting at York, and to-night I am able to show, I think for the first time in this country, a 200-volt metal filament lamp burning direct on the circuit of this building, which is a very gratifying result. My reference to the remarks I made many years ago is solely for the purpose of demonstrating that the results obtained to-day were foreseen by those who made a scientific study of the methods of converting electric energy into light. In view of the progress made with these new lamps, it must be borne in mind that most of the present drawbacks of the lamps, such as risk of breakage in transport, or that they may have to burn in a vertical position, or that they are of very high c.p. units, etc., may be looked upon as only temporary, because the improvements which are constantly made are sufficient to encourage hope for the future. I need only refer to the similar development of gas mantles, and call to mind their brittleness originally, and the high price at which they were sold, as compared with the improved mantles of to-day and their present reasonable price. I believe that to obtain similar results with the new metal filament lamps is simply a matter of time.

In my opinion these new lamps form the beginning of a new era in illumination, and will considerably help in improving electric lighting generally. Patience is, however, still required for the gradual development of the necessary improvements, and it is useless to condemn an invention because it is not immediately commercially successful. I may say that the Zircon-Wolfram 200-volt lamp shown here to-night is a development of only one year's experimenting, as it is less than a year since the first 37-volt lamp was exhibited in this country. It may be interesting to notice that the Zircon-Wolfram lamp as well as the "Osram" lamp, for which an efficiency of 1 watt per c.p. is claimed, can run equally well on both alternating and direct current circuits. The tests carried out at the National Physical Laboratory with the early Zircon-Wolfram lamps, which have been referred to by Mr. C. C. Paterson, were made by using the same

* *Journal of the Institution of Electrical Engineers*, vol. 27, 1898, p. 170.
† *Journal of the Society of Arts*, vol. 54, 1906, p. 322.

Mr. Gaster.

lamps first for about 100 hours on a direct-current circuit and for the rest of the test on alternating-current circuit available at the National Physical Laboratory. I understand that recent improvements in the manufacture of these lamps have given considerably better results than those obtained with the earlier types.

Coming to the question of the use of the tantalum lamp on alternating-current circuits, I am at a loss to understand why people endeavour to buy these lamps and use them on alternating-current circuits against the recommendation of the manufacturers. In view of this recommendation, I wrote asking why the life of these lamps is so considerably shortened when used on alternating-current circuits, and was given to understand that the frequency has a direct bearing on the life of the lamps. A lamp burning on an alternating-current circuit using up to 25 ∿ per second will have a life almost as long as the lamp used on direct-current circuit, but with the increase of the frequency the life is shortened considerably, so that on a circuit with about 50 ∿ the life of the lamp was reduced to about one-third, and if the frequencies are increased, say up to 2,000, as was done by the makers for the sake of experiment, the life of the lamp is reduced to about 10 to 15 hours. The explanation given, which is not shared by all investigators, is that there is an oscillatory motion set up assisting crystallisation, the breakages taking place between such formed crystals. The makers, being aware of this drawback, do not recommend the use of the lamp on high-frequency alternating-current circuits for the present until further improvements are made, and it is only reasonable to pay regard to them when they advise us not to use their goods for certain purposes.

Regarding the "Osmin" lamp, very little practical information is available as yet, but reference may be made to a very interesting paper recently read by A. Libesny,* from which it appears that such lamps can be used at 1 watt per candle, and that the lamp will stand reasonable voltage fluctuations without being so much affected as the present carbon lamps are.

Although Messrs. Siemens & Halske, the makers of the tantalum lamp, have tried, and are still trying, to make lamps of higher voltage with smaller c.p. than they can be made at present, using metal filaments, they have taken out a British patent—No. 9,109 of 1906—for the manufacture of lamps by using a combination of carbon filament in series with metal filaments, in order to obtain the necessary resistance for making high-voltage lamps, which should be more efficient than the high-voltage carbon lamps of to-day, but nothing has, as far as I know, transpired regarding the working of this patent.

Apart from the metal filament lamps, of which there is a large number claiming high efficiencies, I should like to mention another line of research for improving the methods of electric lighting which is receiving considerable attention now in the United States. I mean

* *Elektrotechnik u. Maschinenbau*, vol. 24, 1906, pp. 437, 456; *Science Abstracts*, vol. 9, B, 1906, p. 364.

Mr. Gaster. the use of "vacuum lamps" with intermittent ionisation, like the Macfarlan-Moore type. In this type of lamp a permanent gas under reduced pressure is transiently ionised by the high potential stress, thereby becoming a luminous conductor, and allowing a discharge which relieves the stress, whereupon the ionisation ceases and is re-established with such rapidity that the eye does not observe the interval. By this method of illumination an efficiency is obtained of 1 watt per candle. Tests have been made by the Electrical Testing Laboratory of New York for comparing this particular method of illumination with incandescent and Nernst lamps. The tube in question was 179 ft. long and 1¾ in. in diameter, following the contour of a picture gallery in which it had been installed, and very satisfactory results regarding proper diffusion of light are claimed to have been obtained. The automatic valve invented by Mr. Moore for maintaining the desired vacuum seems interesting. This type of illumination is only used on alternating-current circuits.

With regard to the question whether the new metal filament lamps which, for the present, are of high c.p. units will be largely used, I am not quite prepared, like others, to admit that the higher candle power of the new lamps is a great drawback, because that peculiarity might be helpful in other directions. It has been said, with a great deal of truth in it, that the effect of electric light upon sight is not in any way worse than that of other illuminants; but it is no use denying that there is a dazzling effect produced by looking often at the filament of incandescent lamps. The remedy suggested is very simple, and in many places is taken advantage of, namely, to conceal the lamp; but very often this cannot be done without sacrificing a great deal of light which is wanted on the object to be illuminated. With the advent of the new lamps which give three or four times more light at the same expense as that of the carbon filament lamps used at present, the possibility of concealing the lamp is greatly enhanced, and a much better diffused illumination can thereby be obtained without undue expenditure.

Regarding the remarks of the author, that the lamp makers in this country do not give sufficient support to scientific research, I must say that since I expressed a similar opinion about eighteen months ago I am pleased to notice that a gradual change is taking place, and there is a growing tendency to recognise the true merits of scientific research. I have been personally approached on more than one occasion by a leading lamp maker to keep him in touch, not only with those who are scientifically interested in improving the lamps, but also with those who actually know how to make them, the lamp maker being prepared to pay reasonable remuneration for such service. I should like to add that I do not think that any distinction ought to be made between a foreigner or native born in appreciating his work. Science is international, and if any use can be made of improvements, no matter where they come from, let us take advantage of them to the benefit of the industry.

Mr. Gaster.

Communicated: Since the meeting I have noticed a new departure for making lamps of higher efficiency—I mean the invention of Professor A. C. Parker and Walter G. Clark, of New York, called the "Helion" filament incandescent lamp. It is said that the filament is composed largely of silicon, which is reduced and deposited together with the other materials, the base being a specially prepared carbon filament. It is claimed that this filament, though not metallic, can be operated at a specific consumption of 1 watt per candle, at a temperature much below the temperature of metallic filaments when operated at this consumption. This is an interesting departure from previous experience, and as the information available is scanty, I only refer to it for the present.*

Mr. Robertson.

Mr. C. J. Robertson: There is one small point that I should like to mention, and that is the question of the frequency of the current, which has already been touched on by Mr. Gaster. It would be of very great interest if some definite tests could be published on that subject, showing the particular frequency at which this breaking down effect is at a maximum, and also the point at which it disappears, if there is such a point—I mean the difference in behaviour of the same lamp between alternating-current and continuous-current values, for in that there seems to lie a hidden secret.

There is a good summary in the author's paper of that which is already available by a lengthy search in the technical press and in Patent Specifications. There is no doubt that metallic filament lamps have a most promising outlook for the future, and my company, as well as others, are keenly aware of its possibilities, and are working towards the perfecting of the processes necessary for actual commercial manufacture. It must be remembered that the Nernst lamp was hailed with the same acclamation as is now being given to metallic filaments, but the Nernst has not fulfilled what was expected of it. Consequently we must not be too precipitate in our opinion of these later forms of lamps, which in most cases are still in what may be called an experimental state.

The author gives Edison the credit of being the first to "begin with a high pressure especially in view," but I think this credit is due to one of our own countrymen, *i.e.*, St. George Lane-Fox, who in his patent No. 3,988 of 1878 fully describes the advantages of high pressure and metallic filament lamps in parallel on circuits of 100 volts. This patent is for a platinum-iridium filament lamp, and in the same year he patented regulators and meters, all for use on such 100-volt lamp circuits. Our better knowledge of high temperatures and of electric furnaces for the reduction of the rarer metals is due in large part to the pioneer work of Professor Moissan. The earliest practical treatment of carbon filaments by means of the electric furnace, that I can recollect, was by Messrs. Woodhouse and Rawson, who treated their furfural filaments in this way at their Cadby Hall Works.

Before the carbon filament lamp can be displaced by these

* *Electrical World*, vol. 49, 1907, p. 10.

Mr. Robertson. newer lamps there must be further improvements made in the direction of—

1. Higher voltage.
2. Decrease of fragility when hot or cold.
3. Better behaviour on alternating currents, which nowadays are of great importance.

It is possible that these points may be considerably improved upon, but much work remains to be done.

As regards the behaviour of metallic filaments with alternating currents, I have seen a reference to experiments where it was found that the greater the periodicity the weaker was the molecular cohesion of these metallic filaments, as if something similar to rapid magnetic reversals were taking place in the filament, causing it to become extremely brittle, and in the case of very rapid alternations the filament would not last many minutes.

Reference is made by Mr. Swinburne to the probable use of larger lamps of about 50 c.p. on the present high-voltage circuits, but if a lamp of, say, 250 volts 50 c.p. is to be used, it is difficult to see how the increased length of such a wire is to be disposed of in the bulb. The greater the candle power the thicker will be the diameter of the filament, and consequently the longer it will have to be for any given voltage. From the view of increased efficiency, the metallic filaments are a considerable advance, but their quality is very dependent upon the processes used for their preparation. After all they only represent a mechanical efficiency (reckoned from the energy in the coal) of, say, 2 to 2½ per cent., and they would appear in this respect to be the very best we can get from glowing bodies; so that little further advance can be expected on these lines, and we must look to some fundamentally new principle to improve on this comparative inefficiency.

Mr. Sayers. Mr. H. M. SAYERS: I have a few photo-micrographs of tantalum filaments to show.

Fig. N is a new tantalum filament taken by reflected light. The surface has a metallic lustre, but it is rough, and under the microscope it presents a porous appearance.

Fig. O is a tantalum filament which has been running on continuous current; the photograph was taken by reflected light. It will be seen that the surface is broken up and crinkled in a somewhat irregular fashion. There are a few marks towards the left of a cuneiform shape. The general appearance of the thing is that it is wrinkled more or less in a concentric fashion.

Fig. P is a tantalum filament, viewed by reflected light, which has been run on an alternating circuit of 83 $\sim$. It shows the filament broken up into short sections, but the surface is much smoother and brighter than even a new filament, and has not the crinkled appearance produced by the continuous current.

I will not go into theories accounting for these appearances; but I should like to call attention to an article by Dr. C. Sharp,* in which he

* *Electrical World; Electrical Engineering*, vol. 1, 1907, p. 107.

FIG. N.

FIG. O.

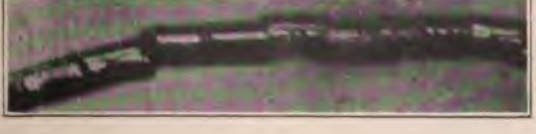


FIG. P.

gives a number of sketches from tantalum filaments run at different frequencies, and it would appear from that article that the effect of the increase of frequency is to shorten the sections into which the filament breaks up. At 25 ∾ there is very little breaking up, but at 130 ∾ the lengths of the sections are rather less than the diameter of the filament. Mr. Sayers.

Mr. HAYDN HARRISON: I think we are to be congratulated on having a paper on this subject at a time when we are all wondering what will be the immediate effect of the advent of the high-efficiency lamp. Of the ultimate effect there is little doubt that it will benefit all of us, in the same way as the gas mantle benefited the gas lighting industry; but the result in the near future is of primary importance at present. The first to benefit will be those supplying low tension at the consumers' premises; and, as an advocate of alternating-ourrent distribution, I cannot help thinking that I shall have the opportunity of congratulating those who have stood by it. The only thing in my opinion which led the majority to lay down direct-current systems in this country was the question of motors; but results such as have been obtained at Leeds, Coventry, and elsewhere, show that the motor load can be dealt with equally well on alternating systems. In fact, one might say that the problem of designing efficient alternating motors is much easier than producing efficient incandescent lamps of low candle power for voltages of 200 or over. There are, of course, other factors—such as the high price of copper, regulation, etc.—which will help to turn the balance in favour of alternating systems; but the lamp question will, to my mind, eventually settle it. The arc lamp is also, by the use of impregnated carbons, being brought to a higher efficiency for alternating work; and for public lighting, where a great economy is effected by using lamps in series, the possibility of using static transformers to obtain the required pressure for varying lengths of streets is an advantage which is not sufficiently appreciated at present. Mr. Swinburne's paper mostly touches on possibilities; but the remarkable results which are being obtained at present by the "Osram" and other tungsten lamps are worthy of note. I have now had several months' experience with the "Osram" lamps made by the Auer Company, and find that not only are the elaborate tests made by the Reichsanstalt—to the effect that these lamps burn to an average efficiency of over 1·15 watts per Hefner candle-power for over 1,000 hours—confirmed in practice, but even better results are sometimes obtained. This has meant a saving of nearly 100 per cent. both at my office and private house, despite the fact that I have had to continue the use of carbon filament lamps in certain positions. It is true that the use of these lamps often means an unnecessary increase of light, owing to the smallest unit being 30 c.p.; but I regret to have to say that this increase of illumination is appreciated by the majority. I regret it because I am convinced that nature intended the eye to be rested during certain intervals; but the advent of the larger units of light, more especially the gas mantle, is causing people to accustom Mr. Harrison.

Mr. Harrison. themselves to high illumination at all times; thus the eyes are only rested during the hours of sleep, and the opticians and ophthalmic specialists are reaping the benefit. But I would, nevertheless, point out to Mr. Swinburne that one can obtain small units of light of much higher efficiency than carbon lamps, provided the metallic filaments are worked at a slightly lower efficiency than that for which they are sold. For instance, an "Osram" lamp giving 30 c.p. at 1·15 watts per candle, will give 15 c.p. at 1·8 watts; thus it is still twice as efficient as a 16-c.p. high-voltage carbon lamp, and, as stated before, this candle-power and efficiency will remain constant in the case of a good lamp. From Mr. Swinburne's remarks concerning the composition of the glowers of Nernst lamps, I gather that he considers the gap they had to fill no longer exists. In any case, I trust that the metallic filament lamp will not be controlled by any one company—otherwise we may see as little progress made in the perfection of them as has occurred in the case of the Nernst lamp. It is, of course, probable that lamps of which the filaments are run at the high temperature necessary to obtain high efficiency will have to be placed in a pendent position, as the filaments will be soft at the temperature. This can be altered, as in the tantalum lamp, by having a multitude of supports for the filament; but I should like to ask Mr. Swinburne if he does not consider that the contact of a wire at that temperature with several supports at a very much lower temperature not only reduces the possible efficiency, but also weakens the filaments by stresses set up at those points. The effect that metallic filament lamps are likely to have on the industry is much emphasised when it is borne in mind that their mean spherical efficiency is higher than that of an ordinary arc lamp, and is only exceeded by that of a flame lamp, or a mercury vapour lamp of the larger size.

Mr. Snell. Mr. ALBION T. SNELL (*communicated*): Having no laboratory at my disposal I was not able to make researches with regard to the physical properties of metallic filaments, but I soon recognised that the wire lamp was bound to effect a change in the practice of electric lighting and to extend its sphere of usefulness. For the past eighteen months I have had different makes of these lamps under close observation on a number of public supply circuits as well as on private plants, and at the present time I am responsible for the erection of many hundreds of lamps. The results have fully justified the expectations of more light for a given power or less power for the same light, as compared with the ordinary carbon filament; and the life of the "wire" lamp has been proved to be at least equal to that of the carbon.

The limiting of pressure to about 120 volts prevents the free use of wire lamps on many public supply circuits; but I have found little difficulty in securing economic and convenient arrangements for the larger rooms by running two lamps in series. With new private plants, where the pressure can be fixed at about 100 volts, the 13 and 21 c.p. lamps are sufficient for all purposes except in the few passages and

lavatories, where 5 or 8 c.p. carbon lamps are preferable. If new work be carefully planned, a considerable saving can be made in outlay for plant. In a recent case I was able to save about 30 per cent. in the size of engine, dynamo and battery, and 40 per cent. in the larger copper conductors by the use of wire lamps. I have found no special difficulty in grading two lamps of the same c.p. to run two in series. In my private office there are two 21-c.p. lamps thus connected; one has been in use eighteen months, the other six months. The office lamps got mixed inadvertently during cleaning, and the difference in "colour" was not noticed until I made one of my periodic examinations of the lamps. I have found more difficulty in this respect with the 50-c.p. lamps marked for 120 volts. The difference in c.p. is of course most marked in the first few hours, before the newer lamp has passed the point of maximum illumination. Mr. Snell.

Mr. C. TURNBULL (*communicated*): It is frequently stated that Nernst lamps must not be reversed. We have some in use here in the generating station, where the voltage is reversed two or three times a night, when the station load is put from one side of the system to the other to assist the balancer. They actually run better under these conditions than the Nernst lamps in the town which are not reversed. I have a burner which has run about 1,000 hours, and it is in excellent condition and shows no signs of decay. The burner has run under its voltage part of the time, because it is of high enough volts to take the high pressure which is necessary in the station on the peak of the load. After the load has gone off the burner would be under-run about 20 volts, and this partly accounts for the long life. At the same time it must be admitted that it seems to be all the better for its frequent reversing, and it would be interesting to know if others have had the same experience. Possibly it might be worth while to wire Nernst lamps on to reversing switches so that the current was reversed every time the lamp was put on. Mr. Turnbull.

Mr. LIONEL CALISCH (*communicated*): May I point out that the melting-point of the rare metals is of little importance to materials available for metallic filament lamps? Carbon, as is well known, is the most refractory metal we know, melting, it is believed, at about 3,600° C., and yet it is an unsatisfactory material for lamp filaments. Mr. Calisch.

The reason for this is that the limit of temperature at which we can work our filament is not the melting-point, but the temperature at which the evaporation of the filament becomes so rapid as to limit its life. This happens when we overrun a carbon lamp: we increase its efficiency by sacrificing its life.

Now the metals zirconium, osmium, tantalum, tungsten, etc., have a far lower melting-point than carbon, but they have at the same high temperature a lower vapour tension, which is possibly due to their greater atomic weight. Therefore we can work the filament at a higher temperature than a carbon filament and get a higher efficiency with a long life. I believe C. P. Steinmetz pointed this out

Mr. Calisch. a few years ago, and he again draws attention to this fact in a recent article.*

Mr. Davies. Mr. H. S. DAVIES (*communicated*): There are a few points upon which I should like to touch in regard to incandescent lamps; first, as there is a perceptible difference in the length of life of a lamp caused by a variable pressure of from 5 to 15 per cent., I think the lamp manufacturers should endeavour to make their lamps capable of standing a greater variation of pressure than at present. For there are very few central stations where the voltage does not vary more than 3 per cent. An examination of the self-recording voltmeter charts, will bear out what I state, for, as a rule, they show anything but a very straight line. To some lamps a varying pressure is fatal; the Nernst in particular is thrown out of adjustment with a 5 per cent. rise in pressure on A.C. circuits, which is no unusual rise on a supply system.

We have tried many hundreds of incandescent lamps from different makers, and have found that they were unable to withstand for any length of time an alternating current of a pressure varying from 5 to 15 per cent.

I have not yet discovered a 4-watt per candle lamp that will give not more than 0·2 of a watt increase per candle after a 500 hours' life, or an increase of only 5 per cent. The ordinary carbon filament lamp not only decreases in c.p. with age, but increases the watts per candle. So perhaps I am asking too much in the above.

Mr. Swinburne. Mr. SWINBURNE (*in reply*): I have very little to say in reply, because my paper was largely written for the purpose of eliciting information, and it has been very successful in that direction. As regards the tantalum lamp, Messrs. Siemens have written to say that the difficulties connected with alternating current are due to crystallisation, and that they depend on the frequency and shape of the curve. It seems to me very likely that it is simply due to the waggling of two neighbouring wires, and of course the quicker the waggling the more it will affect them. Many metals will not stand vibration. If a submarine cable is suspended across a depression on the bottom, the ocean current may vibrate it, and break the copper inside the steel protection. In the early days of arc-light dynamo making we came across the same phenomenon. I am also told that pure tantalum is not hard—the hard material is really an alloy. Leaving the question of the tantalum lamp, Dr. Harker showed us some silicon carbide, and pointed out that it went in what he thought was a peculiar way. I would suggest that we must not be discouraged by that experiment from working with carbides like silicon carbide, because I think there must have been something wrong with it. Silicon carbide is an endothermic compound, and is therefore probably not dissociated by heat. It is made at a very high temperature, at a much higher temperature than he showed us. Dr. Harker got it so hot that the silicon burned very quickly. Silicon has a very great greed for oxygen, as the

* *Electrical World*, vol. 48, 1906, p. 1041.

Mr. Swinburne.

people who run converters with silicon iron know. The heat of combination of silicon and oxygen is very high; and what probably occurred in Dr. Harker's experiment was that he got the carbide hot and practically burned it in air. It does not follow, however, that anything of the sort would happen in vacuum. Mr. Story made a silicon carbide filament here last week. The new 200-volt lamp shown to us on the last occasion, and also again to-night, seems to me to be an important improvement. It is, I think, the first 200-volt wire lamp, but it is also important in another way, if, as I am told, it is made of zirconium and tungsten. I would suggest it is probably not an alloy at all, because an alloy would probably have a low melting point, and that this may be the first of a series of new compounds. Mr. Cooper and I were discussing this, and we were rather suggesting that probably it is a zirconide. Zirconium is rather akin to silicon and carbon, and it is possible this is the first of a new series of compounds which may play a very important part in electric lighting. I have never heard of zirconides before, but it looks as if this was one. I will only refer to one other point, and that is Mr. Solomon's and Mr. Hirst's able defence of their own people in employing experts. I am very glad, but I am afraid it is not always so, because my remarks were based on trustworthy information which came from another quarter.

The President.

The PRESIDENT: Before asking you to accord a hearty vote of thanks to Mr. Swinburne for his paper, I want to make one remark. Dr. Harker put before us last time a table giving the melting-points of a number of elements, so far as they were known at the date of his observations. It is the product of a very considerable amount of work, and it is based on the results of several workers, some of them at the Reichsanstalt, and others of our own. The very next morning after the paper was read we received from the Reichsanstalt a paper in which, I am sorry to say, they revise the basis upon which a large number of the figures in the table have been calculated. So that I am afraid Dr. Harker would have put those figures before you, had he been speaking to-night, perhaps with somewhat less confidence than he did a week ago. I do not think the table can be corrected in accordance with the new data, as they are too insufficient at present, but they throw some doubt on the melting-point of platinum, on which many of the other points are based. It is a very complicated matter, and I understand from the communication that it is by no means certain that the authorities of the Reichsanstalt entirely accept the new basis. At any rate, it is stated definitely at the end of the paper that they do not intend to alter their own standards.

I now ask you to accord your hearty thanks to Mr. Swinburne for his paper, and not only to Mr. Swinburne, but to the several members who have so kindly come forward and made this, I think I may say, a most interesting and important occasion. We have had exhibits of lamps from Messrs. Siemens, from the General Electric Company, from the Dowsing Company, from the British Thomson-Houston Company, and from the British Westinghouse Company. Professor

The President. S. P. Thompson was good enough at the last meeting to bring a microscope and show us some interesting slides, and to-night he has also shown us some slides of very great interest. I think I shall be only expressing your wishes when I ask you to accord a vote of thanks to Mr. Swinburne and to those other gentlemen who have helped us in this matter.

The resolution was then put, and carried with acclamation.

Proceedings of the Four Hundred and Forty-ninth Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, January 17, 1907—Dr. R. T. GLAZEBROOK, F.R.S., President, in the chair.

The minutes of the Ordinary General Meeting held on Thursday, January 10, 1907, were taken as read, and confirmed.

Messrs. C. K. Falkenstein and V. A. Fynn were appointed scrutineers of the ballot for the election of new members, and, at the end of the meeting, the following were declared to have been duly elected :—

As Members.

Thomas Henry Unite Aldridge.
Oscar Howard Baldwin.
Augustine Robert Everest.
Robert E. T. Hartmann-Kempf.

As Associate Members.

Ernest Atkinson.
John Pery Bradshaw.
Wilfred James Burford.
Edward Stanley Fardon.
Harold Hill.
Douglas Edgar Edwin Giles.
Alfred Johnson Harrison.
Arthur Hart.
Frederick Henry Hartmann.
Samuel Hodgson.
Graham Montague.
Bernard Louis Myer.

As Associates.

Archibald Macdonald.
Robert Newby Hartley Reid.

As Students.

Francis Dixon Abbott.
Robert Philip Abel.
Arthur John Aldridge.
Arthur Liddiard Annison.
John Aylmer.
Arthur Chantry Baker.
Rabindra Nath Banerji.
Norman H. Barker.
Habib Basta.
Walter Jervis Batchelor.

Jal. R. Batliboi.
Harry Latour Bazalgette.
James Robert Beard.
Robert Lancaster Bell.
George William Blankley.
Henry Percy Bramwell.
Raymond Brooke-Little.
James Illingworth Law Brooks.
Richard Arthur Broster.
Ivan Crisp Brown.
Wilfrid Ormerod Burgoyne.
Samuel Bury.
Frank Camden.
A. H. M. Campion.
Christopher Charles Casperd.
Percy West Charlton.
Ashford Vincent Clarke.
John James Climas.
John Connor Connor.
Bernard Alfred Martin Cooper.
Robert Crawford.
William Campbell Crockatt, B.Sc.
Walter Leslie Davis.
John McLeod Donald.
Frederick P. Dumjahn.
Frank Gordon Dunn.
Francis Thomas Emberton.
Charles George Gordon Faine.
Sidney Woods Farnsworth.
Edward Victor Beauchamp Fisher.
William Nelson Rich Garne.
Horace Gray Gilliland.
Oscar McIntosh Goddard.
Tom Golding.
Ronald Grierson.
James Park Hacking.
John Hargrove.
Arthur Forrest Henderson.
Sydney Kirkness Heppell.
Percy Garabaldi Hugh.
Aubrey Illingworth.
Cecil Turner Inman.
Daniel Jenkins.
Lawrence Walter Johnson.
Ernest G. Kennard.
Thomas Moore Kirkby.
Robert Henry Lee.
Frank Mignifie Lines.
Edward Mallett.
Renold Marx.
James Meredith.
William Leavis Merrick.
James Miller.
Allan Monkhouse.
William Harold Morgan.
Robert Torrens Mulholland.
Frank Henry Haswell Oakley.
George Wood Pearce Page.
Cecil Frederic Pallott.
Abraham Richard Palmer.
Henry Edward Parry.
Sydney Charles Potts.
Henry Walter H. Richards.
Harold Willoughby Richardson.
James Hepburn Rickie.
Richard Clive Rigby.
Cornelius Rissik.
Henry Rivers Rivers-Moore.
James John Roberts.
Joseph A. Rugeroni.
Norman C. J. Saunders.
Cecil Albert Schurr.
Malcolm Innes Lewis Smith.
Leslie Bevis Sparks.
Edward Dugdale Spencer.
Robert Whitfield Stanners.
Joseph Pearce Stockbridge.
Alexander Sutherland.
Archibald Stephen Talbot.
William Sennett Thorn.
Hubert Royds Tidswell.
Frank Edgar Tilley.
Hugh Carleton C. Tufnell.
Walter Lyman Upson.
Chotalal H. Vora.
Thomas Walmsley.
Stan Sylvester A. Watkins.
Andrew Weiss.
Charles G. Wilcox.
Christopher Kenneth Wise.
Reginald Dashwood Wolfgang.
William Henry Wood.
Robert Yorke.

Donations to the *Building Fund* were announced as having been received since the last meeting from R. A. Dawbarn, W. B. Marr, J. Shaw, H. D. Symons; and to the *Benevolent Fund* from F. Gill, H. A. Irvine, R. Robertson, and J. G. Wilson, to all of whom the thanks of the meeting were duly accorded.

The PRESIDENT: Before we come to the formal business of the meeting, I have the very great privilege of announcing that our friends the members of the German Elektrotechnischer Verein and of the Verband Deutscher Elektrotechniker have most kindly and generously sent to us the very beautiful address which is lying on the table as a token of thanks for the entertainment we were able to give them last July, and a recognition of the pleasure they received from their visit to our country. The address will lie on the table for members to examine afterwards if they wish.

The discussion on Mr. Swinburne's paper was concluded (see page 233), and the meeting adjourned at 9.35 p.m.

Proceedings of the Four Hundred and Fiftieth Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, January 24, 1907—Dr. R. T. GLAZEBROOK, F.R.S., President, in the chair.

The minutes of the Ordinary General Meeting held on January 17, 1907, were taken as read, and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council:—

TRANSFERS.

From the class of Associate Members to that of Members:—

[redacted]

INVESTIGATIONS ON LIGHT STANDARDS AND THE PRESENT CONDITION OF THE HIGH-VOLTAGE GLOW LAMP.

BEING AN ACCOUNT OF TESTS MADE AT THE NATIONAL PHYSICAL LABORATORY.

By CLIFFORD C. PATERSON, Associate Member.

(*Paper read January* 24, 1907.)

Four years have now elapsed since Dr. Fleming raised the general question of photometry before this Institution in a comprehensive paper on electric lamps.*

The paper dealt in a very thorough way with the general position of light standards in 1902, and Dr. Fleming pointed out the uncertainty which existed in photometric comparisons at the time, due in no small degree to the difficulty of reproducing light standards accurately, and also to uncertainties which existed as to their relative values.

The issue by the Engineering Standards Committee of their Specification for Carbon Filament Glow Lamps gives the subject a fresh significance, and it is the object of the author in this paper to lay before the Institution, first of all the results of some investigations on flame standards made at the National Physical Laboratory, and afterwards to describe other tests, some of which were carried out on carbon filament lamps for the Engineering Standards Committee, to whose courtesy the author is indebted for permission to publish the results.

The author hopes that the data given here will be of value to the electrical and gas industries, and that in the discussion on the paper members of the Institution will enhance its usefulness by freely expressing their opinion upon any of the points which are touched upon in it.

COMPARISON OF FLAME STANDARDS.

The investigations referred to were undertaken two years ago at the instigation of the Institution of Gas Engineers with the object of determining the ratio between the candle powers of the three principal standards as used by the gas industries at the present time in France, Germany, and Great Britain. The author desires to acknowledge the kindness of the Institution of Gas Engineers in allowing the publication, in this paper, of the main features of the report which was submitted

* *Journal Institution of Electrical Engineers*, vol. 32, 1903, p. 119.

to them eighteen months ago, and which they have only refrained from publishing earlier at the request of the International Photometric Commission.

This Commission, on which the Institution of Gas Engineers is represented, arranged for similar tests to be made in France and Germany in order to obtain a set of independent comparisons which shall enable the ratios between these standards to be defined with more certainty and greater accuracy than at present.

The results of tests in France and Germany, although completed more recently, have already been published,* and a comparison of the figures is exceedingly interesting as showing the order of agreement which has been obtainable in the photometric measurements made by separate observers working under different conditions with different apparatus.

A detailed comparison of the methods adopted in carrying out the tests cannot usefully be made until the full report of the Commission has been published, but the results of the tests in the various laboratories are tabulated and compared at the close of this section of the paper.

The standards to which reference has been made are respectively the Carcel, the Hefner, and the 10-candle-power Harcourt pentane lamps. In all measurements which are described here special care has been taken in the manipulation of the lamps so as to conform to the same regulations and practice which are observed in the countries in which they are the recognised standards.

Influence of Atmospheric Conditions.

It is now generally appreciated that atmospheric conditions have an important influence on the candle power of flame standards. Dr. Liebenthal,† at the Reichsanstalt, was the first to make quantitative measurements in order to determine the amount of their influence on the candle power of lamps. He investigated two standards, the Hefner lamp and the Woodhouse and Rawson 1-candle pentane lamp, and the formulæ (referred to later in this paper) which he found for correcting the Hefner lamp are now generally accepted.

In a photometer room the principal variations to which air is liable are the following :—

1. Variation in the amount of carbon dioxide.
2. Variation in the amount of water vapour.
3. Variation in the proportion of oxygen and nitrogen.
4. Changes in barometric pressure.

* *Société Internationale des Electriciens, Bulletin*, vol. 6, ser. 2, 1906, p. 375. "Étude sur le Rapport des trois lampes Carcel, Hefner, et Vernon Harcourt," by MM. Laporte and Jouaust. Also "Extraits du Rapport presenté à la Commission Internationale de Photometrie," by M. Perot and M. P. Janet.

Journal für Gasbeleuchtung, vol. 49, 1906, p. 559. "Photometrische Versuche," etc., by Dr. Emil Liebenthal.

† *Zeitschrift für Instrumentenkunde*, vol. 15, 1895, p. 157.

Before any measurements with flame standards can be considered reliable, it must be ascertained that the air is under standard atmospheric conditions, at least as regards 2, 3, and 4. If the conditions be abnormal, the amount of water vapour in the air as well as the barometric pressure must be known.

In making measurements to determine the amount of candle-power variation due to any one of the above atmospheric changes, it is very difficult artificially to increase or decrease one without at the same time varying the others; and although attempts were made by the author to vary artificially the amounts of carbon dioxide and water vapour in the air, the results were difficult to interpret. The method was, therefore, discontinued in favour of the plan of waiting till the weather changed, and the desired conditions came in the ordinary course of events.

As these experiments must, in the nature of things, cover a considerable time, more extended investigations have been made by the author on the Harcourt 10-candle pentane lamp. This is the standard of the Metropolitan Gas Referees, and has been adopted at the National Physical Laboratory as being the most convenient unit of light, and the Standard whose value is now most generally recognised as representing that of ten British Parliamentary candles.

Carbon Dioxide.—In order to determine the effect, if any, of variations in the amount of CO_2 present in the air, samples of air were taken from the neighbourhood of the lamps whilst observations were being made. The air was slowly drawn into previously exhausted 10-litre vessels which, when full, were tightly stoppered and examined at the end of the observations.

TABLE I.

COLUMN 1. Hours after Lighting up.	COLUMN 2. Candle Power of Pentane Lamp.	COLUMN 3. CO_2 in Litres per Cubic Metre.	COLUMN 4. Water Vapour in Litres per Cubic Metre of Pure Air.
0	10·06	0·55	9·0
¾	9·75	0·9	10·6
1¾	9·60	1·4	11·0
3½	9·45	1·8	11·8
4½	9·33	1·9	12·2

The amount of CO_2 was determined by shaking up with barium hydrate and subsequent titration with oxalic acid. It was found that the normal variation of carbon dioxide in the air at Bushy House only covered a range of from 0·3 to 0·7 parts per thousand, and no candle-power difference was observable due to this amount of change.

In order to increase the effect and to determine whether the introduction of CO_2 into the atmosphere of a closed room had an appreciable effect on the candle power of the lamp, the latter was burnt in a closed room for several hours under normal atmospheric conditions. The test was then repeated with the introduction of additional CO_2 into the atmosphere. The results of the latter test are given in Table I. After correcting for the increase of humidity, the decrease in candle power after 4¼ hours amounts to some 5 per cent., which is very little in excess of the observed decrease when the lamp is merely left burning in an ordinary closed room. These tests are not quantitative, but serve to show that an increase of CO_2 considerably above the maximum amount met with in an ordinary ventilated photometer room has no appreciable effect on the candle power of the lamp.

Dr. Liebenthal found, from experiments in which he artificially increased the amount of CO_2 to 14 litres per cubic metre, that the candle power of the Hefner lamp varied 0·7 per cent. for an increase of 1 litre per cubic metre. As the amount of CO_2 in a closed room, $5 \times 6\frac{1}{2} \times 4$ metres, in which the lamp has been burning for upwards of one hour, only varies from about 0·5 to 0·8 litre per thousand, the influence of carbon dioxide on the candle power of flame standards may be considered, for practical purposes, as negligible.

Water Vapour.—The amount of water vapour in the air is the most disturbing factor to be considered in dealing with flame standards, as it cannot be remedied by ventilation of the photometer room. There are large natural variations in humidity throughout the year, and rapid changes from day to day.

The total variation, for instance, in the candle power of the pentane lamp between a warm, damp day in summer and a frosty one in winter, may amount to 10 per cent., and between two consecutive days it is frequently 2 per cent. or 3 per cent.—the changes in the Hefner lamp from this cause being very little less than in the pentane.

A discussion of the various methods of measuring humidity is a subject which hardly comes within the scope of this paper. The chemical method, Dynes dew-point hygrometer, the ordinary wet and dry bulbs, and the Assmann ventilated wet and dry bulb thermometers have all been used. The last-mentioned instrument has been found to give the most reliable and consistent readings, as well as being the easiest to manipulate.

The author reproduces in Fig. 1 a curve which he has already published * showing the effect of variations in the humidity of the atmosphere on the candle power of the pentane lamp; candle power is plotted vertically, and humidity horizontally. The latter, for obvious reasons, is stated volumetrically, and is expressed as the number of litres of water vapour per cubic metre of dry air. If, therefore, e stands for the aqueous pressure, then the water vapour per cubic metre of pure air

$$= \frac{e}{b - e} \times 1{,}000 \text{ litres}$$

where b is the reading of the barometer.

* *Report of the British Association*, 1904.

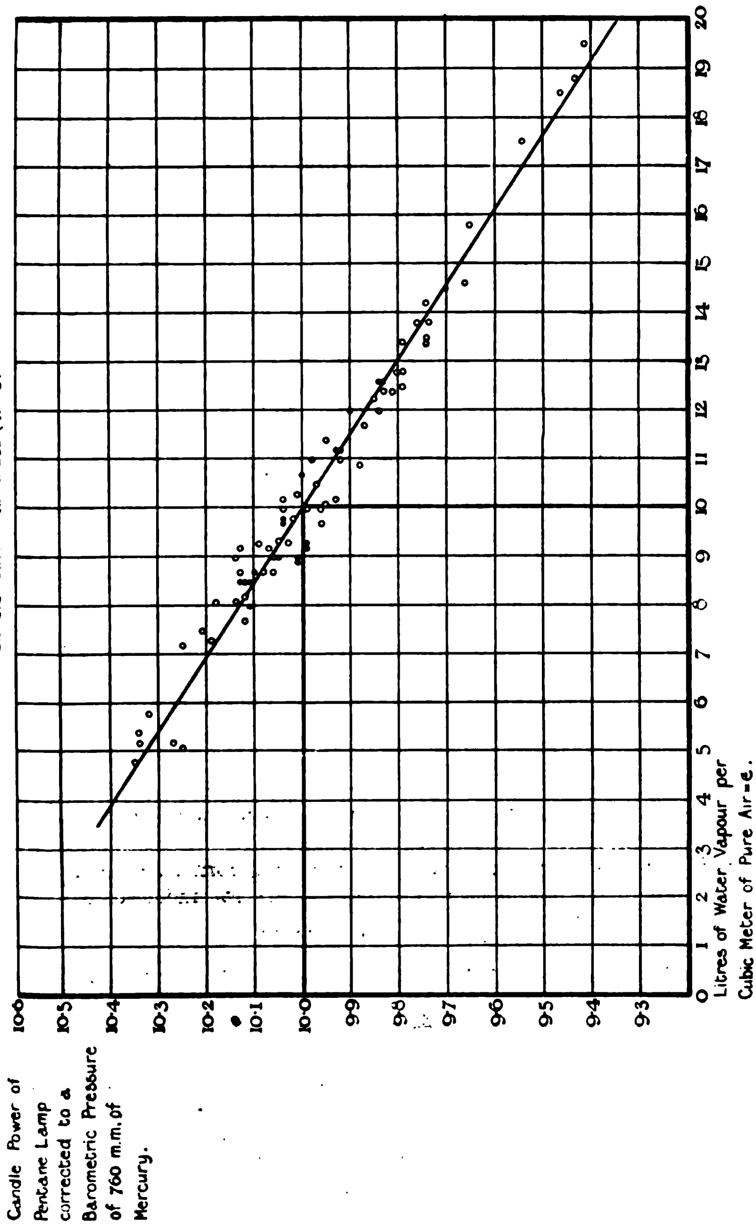


FIG. 1.—10-Candle Harcourt Pentane Lamp—Variation of Candle Power due to Humidity.

Each point on the curve is the mean of a set of observations similar to that described on page 283. None of the atmospheric conditions were artificially produced, but the results were obtained by observations taken under normal conditions throughout the year. These show, therefore, the kind of candle-power variations which may be expected when using the lamp under favourable conditions—amounting to a possible error, due to humidity alone, of plus or minus 5 per cent.

The curve has been obtained from the barometer and humidity readings by the method of least squares. On the assumption that the

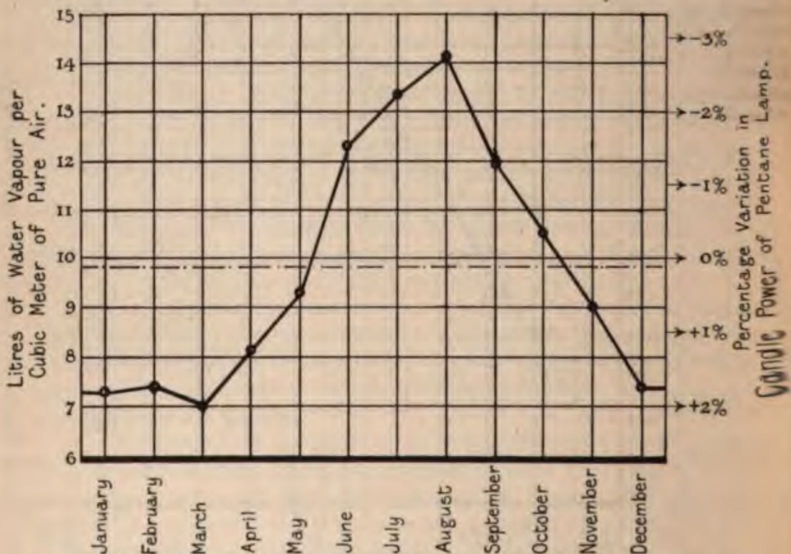


FIG. 2.—Diagram showing Mean Humidity for each Month of the Year.

variations follow straight-line laws, the following formula is obtained connecting candle power and humidity for the 10-candle lamp :—

$$\text{Candle power} = 10 + 0{\cdot}066\,(10 - \epsilon)$$

where ϵ is the number of litres of water vapour per cubic metre of dry air.

It will be observed from this formula that the candle power of the lamp has its standard value when the volume of water vapour is 10 litres. This figure has been taken as representing very nearly indeed the mean humidity over the three years 1897–8 and 9, both at the Meteorological Office in Victoria Street and at the Observatory Department of the National Physical Laboratory. The former gives a mean of 10·04 litres per cubic metre, and the latter 9·85. The curve in Fig. 2 shows the mean humidity over three years for each month of the year as found at the Observatory. Humidities are plotted as

ordinates on the left-hand side and percentage variation in candle power on the right. It will be seen from this that the average monthly humidities range from 7 to 14, corresponding to errors of about plus or minus 2 per cent. in candle power. As already observed, however, individual observations are liable to twice this error.

Fig. 3 gives a curve similar to that shown in Fig. 1, but obtained for the Hefner lamp. Its candle power is expressed, for the sake of com-

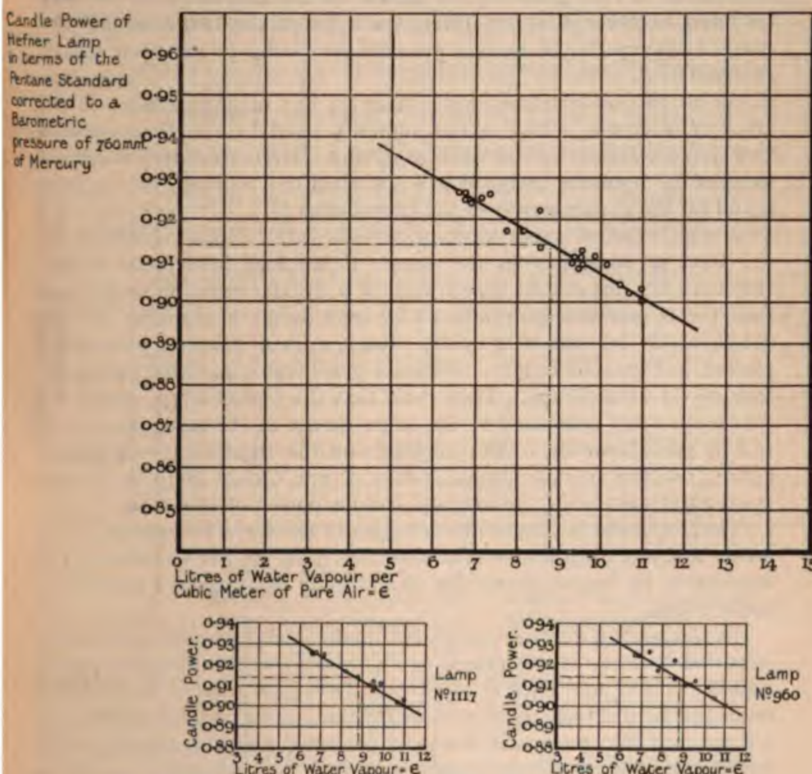


FIG. 3.—Hefner Lamp—Variation of Candle Power due to Humidity.

parison, in terms of the pentane lamp burning in an atmosphere containing 10 litres of water vapour per cubic metre of pure air. Two lamps previously certified by the Reichsanstalt (the German Standards Laboratory) were used in the experiments, the observations on the individual lamps being shown separately by the side of the main diagram, in which all points have been plotted. The formula obtained from these observations is—

$$\text{Candle power (pentane units)} = 0{\cdot}914 + 0{\cdot}006\,(8{\cdot}8 - \varepsilon).$$

Dr. Liebenthal,* from the average of a much larger number of observations made over a greater range of humidity, found a formula which, using the ratio of Hefner to pentane as 0·914, gives the following relation:—

$$\text{Candle power (pentane units)} = 0{\cdot}914 + 0{\cdot}005\,(8{\cdot}8 - \epsilon).$$

As this is based on more numerous data, it should be accepted in preference to that given by the author for the Hefner lamp. It may be noted, however, that the difference between the two formulæ produces a discrepancy of under 1 per cent. in candle power even for an extreme case.

As all the observations were made in the neighbourhood of 8·8 litres of moisture (which is the humidity fixed by the Reichsanstalt at which the lamp shall be taken as giving 1 Hefner candle), the exact value of the humidity constant will not affect the accuracy of the ratio found for the candle power of the two standards.

A similar set of experiments was made with two Carcel lamps, but the want of constancy in the lamps themselves introduced inconsistencies into the results which were of a greater order of magnitude than the largest change produced by such range of humidity as was obtainable at the time of the year. The results of the tests have been plotted in Fig. 4 in order to obtain a graphical comparison with the tests on the other lamps. They show that the Carcel lamp cannot be relied upon for constancy to the same extent as the other lamps. It will be seen, however, by the diagrams on the right-hand side of the figure, that the average candle power of one Carcel lamp is in very close agreement with the average candle power of the other.

The Proportion of Oxygen and Nitrogen in the Air.—The author is not aware that any quantitative measurements have yet been made on the diminution of candle power due to a scarcity of oxygen in the photometer room.

Dr. Liebenthal discusses the matter on theoretical grounds, and calculates that a decrease or increase of 1 per cent. in atmospheric oxygen should make a difference of 4 per cent. in candle power. It has been remarked by most experimenters with flame standards, especially with a large unit like the 10-candle pentane lamp, that the candle power rapidly falls off in a closed room, and that this reduction cannot be accounted for by the increase of water vapour or carbon dioxide in the air. The author has made measurements in a closed room of 130 cubic metres capacity, and finds that the candle power of the pentane lamp falls about 1 to 1½ per cent. in 1 hour.

Mr. Dow † has recently published some curves showing results of tests made at Kensington in a room of about 400 cubic metres capacity, in which, with two persons in the room and two gas burners alight, the candle power dropped some 2·5 per cent. in 1 hour.

* *Journal für Gasbeleuchtung*, vol. 49, 1906, p. 559.

† "Sources of Error in the Harcourt 10-C.P. Pentane Standard," by J. S. Dow, B.Sc., *Electrical Review*, vol. 59, 1906, p. 491.

In a small, closed photometer room, in which no effort is made to ventilate, a variation of from 7 to 10 per cent. may quite possibly be obtained.

With a small unit such as the Hefner lamp, the trouble due to this cause is not so great, but it may safely be said that under no circumstances is it safe to use a flame standard in a room which has not very efficient ventilation.

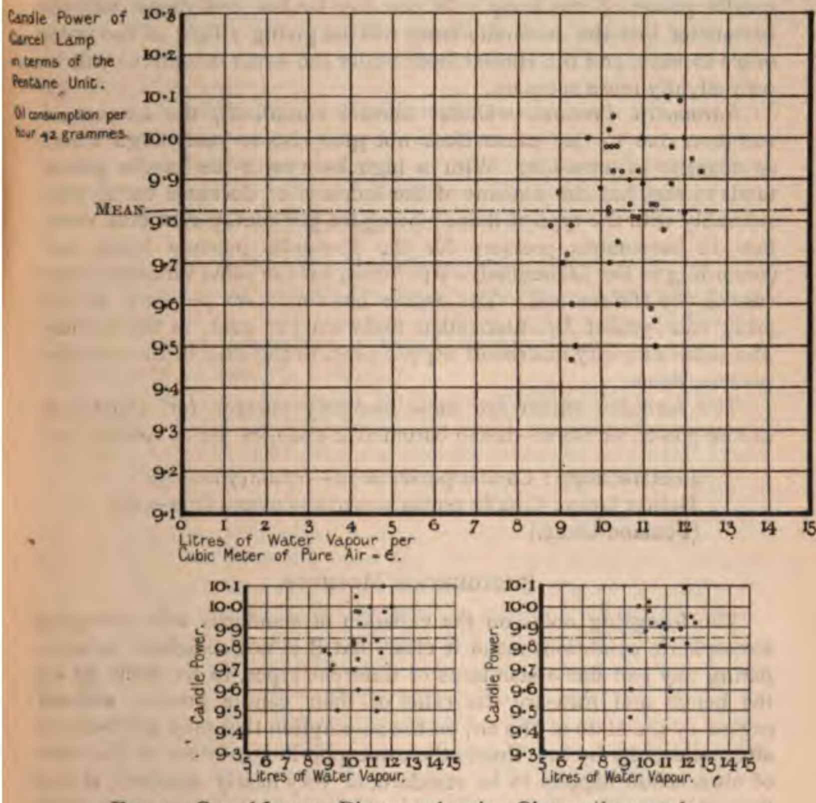


FIG. 4.—Carcel Lamp—Diagram showing Observations made.

In the case of a 10-candle-power pentane lamp, no measurements should be relied upon which have been made after the lamp has been burning in a closed room for more than 15 or 20 minutes, unless, of course, the source of light which is being measured is affected in the same way by atmospheric changes.

The author proposes to make some tests on flame standards in the near future, measuring at the same time the amount of oxygen in the air, with the object of determining if the diminution observed

in their candle power, as explained above, may be attributed to this cause. The experiments would have to be made in a small, closed room into which oxygen can be introduced in large quantities.

These facts illustrate how misleading may be the results obtained from comparisons made against a flame standard unless proper precautions are taken and the necessary corrections applied. Suppose, for instance, that the lamp is being used in a room in which the ventilation is bad, and causes, say, a drop of 1·5 per cent. in the candle power of the lamp. If the day be hot and damp and the barometer low the 10-candle lamp will be giving a light of the order of 9·2 candles, and the Hefner lamp under the same conditions will be only slightly more accurate.

Barometric Pressure.—Under normal conditions, the amount of variation due to this cause does not give rise to such large errors as changes in humidity. With a high barometer the candle power tends to rise, but the amount of the increase or decrease varies considerably with the type of flame—being 0·8 per cent. per 10 mm. variation in barometric pressure for the 10-candle pentane lamp, and (according to Dr. Liebenthal) 0·1 per cent. for the same variation in the case of the Hefner unit. The author has found 0·2 per cent. in the latter case, whilst Dr. Liebenthal finds 0·6 per cent. in the former. The same authority has found 0·4 per cent. in the case of the 1-candle pentane flame.

The formulæ which are most probably correct for expressing candle-power variations due to barometric changes are as follows:—

Pentane lamp: Candle power $= 10 - 0{\cdot}008\,(760 - b)$.
Hefner lamp: Candle power $= 0{\cdot}914 - 0{\cdot}0001\,(760 - b)$.
(Pentane Units.)

Photometric Methods.

The foregoing notes on the variation of standards with changing atmospheric conditions make it clear that it is not sufficient, in comparing any two flame standards of different types, to fix them up on the bench and measure the ratio of their candle powers, without regard to the state of the air, on the assumption that they will both be affected equally by it. Unless the atmospheric conditions at the time of observation happen to be standard, or very nearly standard, it will be necessary to make a series of candle power comparisons with hygrometric and barometric readings. A formula has then to be deduced from which the candle power, under standard conditions, may be found.

The method of comparing two flame standards by fixing them both on a bench together, and obtaining points of balance between them, is not to be recommended, especially when the ratio between them is of the order of 1 to 10, as it is in the case of the Hefner and pentane lamps. The method entails a change over of the lamps to opposite ends of the bench, a maximum difference of colour in the photo-

meter, and the simultaneous regulation of the height of the two flames—which is a troublesome operation.

The double comparison, or substitution method, described by Dr. Fleming in the paper already mentioned, is much to be preferred. By this method a constant source of light, such as an electric lamp, is fixed at one end of the bench and the two flame standards to be compared are alternately measured against it.

In the tests described in this paper, each flame lamp was compared through the medium of an electric comparison lamp of suitable colour against the large bulb electric standards which now form the principal working standards of the Laboratory. The method has the following advantages :—

a. Only one flame lamp has to be regulated at a time.

b. Personal and bench errors are eliminated.

c. A set of observations is obtained from which the relations between candle power and atmospheric conditions can be deduced for each lamp.

DESCRIPTION OF THE LAMPS.

The 10-Candle Harcourt Pentane Lamp.—The details of this lamp are too well known in this country to require any but a brief description. A photograph of the lamp is shown in Fig. 5, and a reprint of the Gas Referees' official description is given in Dr. Fleming's paper on "Photometry of Electric Lamps." *

Liquid pentane is contained in the rectangular saturator at the top of the lamp. Air passes in at one of the cocks, and being drawn round baffle plates over the surface of the pentane, passes down a rubber tube to an argand burner. The air supplied to the outside of this flame is drawn through the cylindrical box enclosing the steatite burner, whilst that feeding the inside of the flame is heated by its passage up the annular space between the outer and inner metal chimneys. It goes from this through the rectangular box seen at the top of the chimneys and down the centre of the supporting pillar to the middle of the burner. As far as the author is aware, the extent to which a variation in the dimension of any part of the lamp affects its candle power has not yet been determined. Some points in the lamp are, of course, much more sensitive than others, and the author has on hand at the present time a set of experiments on a special lamp, by which it is hoped to ascertain the most important points in its construction, and the amount to which a given increase or decrease of the dimension of any part will affect the candle power.

The Metropolitan Gas Referees state in their notification that in regulating the height of the flame to about midway between the bottom of the window and the bar which crosses it, a variation of a quarter of an inch either way does not materially affect the candle power of the standard. This statement may be accepted, provided the accuracy required is not greater than about 1 per cent. If, however, measurements are to be within this limit of accuracy, it is essential that the flame shall be kept

* *Journal Institution of Electrical Engineers*, vol. 32, 1903, p. 126.

at the right height. Dr. Liebenthal* finds that the flame may rise above the mark but not fall below it. The author's experiments, however, confirm those of other observers† in showing that Mr. Harcourt has so proportioned the dimensions of the lamp that when the flame is at its correct height its candle power is a maximum—any variation above or below the normal height causing a decrease in candle power. This is a point which is greatly in favour of the lamp, as it means that a certain variation of height is possible, within which it is easy to regulate the flame, and within which the candle power does not perceptibly vary. By affixing a piece of rubber tube to the air inlet cock on the saturator, and regulating the flow of air through it by an ordinary screw clip, a most sensitive means of flame adjustment is obtained. The observer also is able to stand at such a distance from the lamp that he will not disturb the steadiness of the flame through air currents set up by breathing near it. From this point of view the lamp compares favourably with the Hefner standard, the flame adjustment of which, in spite of the ingenious optical device employed, is a matter requiring great patience and entailing some uncertainty.

The procedure which the author has found it desirable to adopt when using the standard is as follows: The lamp is set up plumb at the end of the bench by means of a plumb-line passed through the chimney, and made to coincide with its central axis by means of a centring plug at the top. The lamp is then levelled till the plumb-bob hangs exactly over the middle of the burner. A gauge of the same pattern as that employed by the Gas Referees is then used to measure the distance of the lamp from the photometer. It has a pin at one end which exactly fits the centre of the burner, and a shoe at the other end, the tip of which must graze some known point on the photometer (either the screen or some portion of the box which is a definite distance from it).

The lamp is lighted up and all doors and windows of the room are thrown open. The lamp is allowed to burn in this way with a free circulation of air for half an hour. The electric comparison lamp is then brought up to its correct voltage by the potentiometer, and all windows and doors closed before taking readings.

The author has not as yet found any photometer which, for the same coloured light, is equal in sensitiveness and accuracy to the Lummer-Brodhun contrast apparatus, which has been used in all the experiments described in this paper. Some twelve or fourteen readings are made during the first ten minutes after the room has been closed, together with observations on both ventilated and unventilated hygrometers placed at different points in the neighbourhood of the lamp. It is desirable for one observer to remain at the photometer and on no account to see the position of his own settings, whilst another adjusts the flame and takes down readings.

* *Journal für Gasbeleuchtung*, vol. 49, 1906, p. 559.

† "Sources of Error in the Pentane Lamp," J. S. Dow, *Electrical Review*, vol. 59, 1906, p. 491.

FIG. 5.—10-Candle-Power Harcourt Pentane Lamp.

FIG. 6.—Hefner Lamp.

As regards the individual accuracy of single photometric observations made under these conditions, much depends upon the condition of sensitiveness of the observer's eye on any particular occasion. This is certainly a variable quantity. The author has found on some occasions that ten readings will agree amongst themselves to within plus or minus a quarter of 1 per cent., whilst on others the extremes are occasionally as much as plus or minus 1 per cent. A set of ten observations taken at random from a test book shows the following values for the pentane lamp in terms of the National Physical Laboratory Standard Electric Lamps, and illustrates the kind of individual variations to be looked for in such measurements :—

$$9{\cdot}81_5,\ 9{\cdot}81_5,\ 9{\cdot}87_6,\ 9{\cdot}90,\ 9{\cdot}90,\ 9{\cdot}85,\ 9{\cdot}87_8,\ 9{\cdot}87_8,\ 9{\cdot}85,\ 9{\cdot}83_8,\ 9{\cdot}86_7.$$

The humidity on this occasion was 12·4, and the barometric pressure 762 mm.

The Hefner Lamp.—A photograph of the Hefner lamp is reproduced in Fig. 6. It is the official standard of light in Germany, and detailed instructions for its use are published in the *Zeitschrift für Instrumentenkunde* for 1893. The construction of the lamp is simple. Amyl acetate, passing specified tests for purity, is contained in the cylindrical reservoir which forms the base of the lamp. A wick dips into this and passes up the thin-walled German silver tube projecting from the centre of the base.

The wick, however, does not rise above the top surface of the tube, but, keeping about level with it, serves to conduct the liquid to the point of ignition. The exact thickness, diameter, and height of this tube are some of the most vital points which determine the candle power of the lamp. The flame is a lambent one, and resembles in general appearance that of an ordinary candle, except that it is circular in cross section. The exact height at which it gives one Hefner candle (about 0·9 English candle) is 40 mm. In order to adjust the flame to the correct height, the lamp is fitted with a sighting arrangement, in which an image of the top of the flame is cast on a ground glass disc and adjusted to a cross line.

The general precautions to be observed as regards ventilation when using this lamp are the same as for the pentane standard, except that the room may be closed somewhat longer while readings are in progress before the candle power of the lamp shows signs of diminution.

Considerable care and skill must be employed in judging whether the flame is at its correct height. The author has observed a tendency for the flame to vary its shape from one which is high and pointed, to one which is somewhat depressed and flattened at the top, and an error in candle power may result from taking either of these as correct, and adjusting either the pointed or the flattened flame to the level of the cross line. The same effect has been remarked by Laporte and Jouaust.*

The Carcel Lamp.—The Carcel lamp, of which a photograph is

* *Société Internationale des Electriciens, Bulletin*, vol. 6, ser. 2, 1906, p. 375.

shown in Fig. 7, is the working standard of the French gas industry. It has a glass chimney and a wick of annular cross section, to which a continual supply of pure colza oil is maintained by means of a clock-work pump. According to the official instructions, the wick should stand 10 mm. above the wick holder, but in practice this is found to give too great a consumption of oil, and it is necessary to lower it to 7 or 8 mm. in order to approximate to standard conditions. The chimney is made of thick glass, and reduced in diameter 7 mm. above the wick.

The lamp should give its standard candle power when consuming 42 grammes of oil per hour. As the exact height of the glass chimney above the wick, and the depth to which the wick is charred, affect the rate of consumption, it is very difficult to adjust to the 42 grammes per hour. The actual consumption is therefore measured, and provided it falls within 39 and 46 grammes per hour the candle power is corrected accordingly. There is some doubt, however, whether this is strictly accurate, Laporte having found that for a given variation in consumption the candle power change is proportionally too great.*

Various precautions must be observed when using the lamp. For each experiment the oil and the wick must be new, and the latter should have previously been stored in a desiccator in order to ensure absence of moisture. As soon as a full stream of oil is circulating over the wick, the latter should be charred to an even depth of about 2 mm. all round by means of a flat flame burner. The lamp may then be lighted, turned very low, and the chimney fixed so that the neck presses close down on to the wick.

Under these conditions there is only a very shallow ring of flame, which tends to equalise the intensity all round the wick. After about fifteen minutes' burning in this condition the chimney is raised, the wick turned up, and after twenty minutes' burning the lamp is counterpoised on a balance on the photometer bench. A weight of 10 grammes is then added to the scale on which the lamp is fixed, and the time observed before the balance again swings over. The correct time for a consumption of 10 grammes, at the rate of 42 grammes per hour, is 14 mins. 17 secs.

The author has found that, after taking a large number of observations, and using the utmost care in all the adjustments, he has been unable to make readings from the lamp agree with certainty to within an accuracy of + or — 3 per cent. (See Fig. 4.)

In any two directions at right angles the light from the flame may vary by as much as 2 per cent., and to ensure even the 3 per cent. limit of accuracy it is generally necessary to take a mean of four readings round the flame in directions at right angles to each other in a horizontal plane.

SUMMARY OF RESULTS.

In summarising the results of the tests described here, and comparing them with those made in other laboratories on the Continent,

* *Société Internationale des Electriciens, Bulletin*, vol. 15, 1898, p. 166.

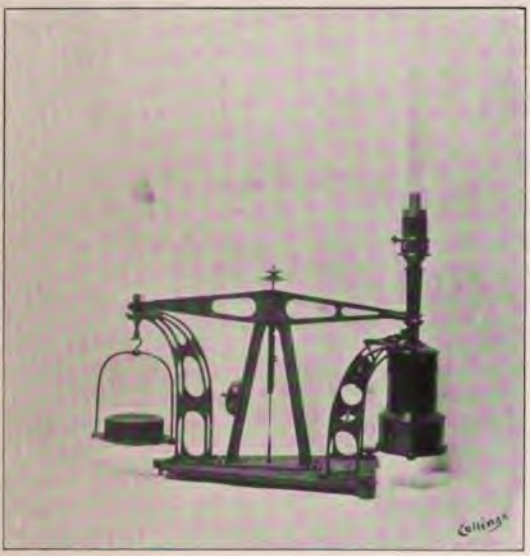


FIG. 7.—Carcel Lamp.

it is essential to take into consideration the fact that the Hefner lamp in Germany has its standard value when the volume of water vapour is 8·8 per 1,000 litres of air, whereas both the pentane and Carcel lamps are taken as having their nominal value when the humidity is 10 litres per 1,000.

The following values are obtained for the Hefner and Carcel lamps from the curves given in Figs. 1, 3, and 4. These values are in the terms of the 10-candle Harcourt pentane lamp. In the case of the Carcel standard the mean of all the readings shown in Fig. 4 has been taken. The variations of the Carcel lamp with changing humidity will hardly concern these figures, since all measurements were made in the neighbourhood of 10 litres per 1,000.

Hefner lamp = 0·914 pentane units.
Carcel lamp = 0·982 pentane units.

The following tables give the values for the same ratios recently obtained and published * by the Reichsanstalt in Berlin, and by the Laboratoire Centrale d'Electricité and Laboratoire d'Essais et Conservatoire in Paris.†

Expressing the values of all the units in terms of that given by the pentane lamp, we have :—

	Pentane.	Hefner.	Carcel.
National Physical Laboratory	1	0·914	0·982
Reichsanstalt	1	0·917	0·991
Laboratoire Centrale	1	0·929	1·000
Laboratoire d'Essais	1	0·928	0·996

Expressed in terms of the unit given by the Hefner lamp they are as follows :—

	Pentane.	Hefner.	Carcel.
National Physical Laboratory	1·094	1	1·074
Reichsanstalt	1·09	1	1·080
Laboratoire Centrale	1·076	1	1·076
Laboratoire d'Essais	1·077	1	1·067

* *Journal für Gasbeleuchtung*, vol. 49, 1906.

† The values for the three ratios given by the Laboratoire d'Essais differ among themselves by about 1 per cent. The mean values have therefore been taken.

Expressed in terms of the unit given by the Carcel lamp :—

	Pentane.	Hefner.	Carcel.
National Physical Laboratory	1·018	0·931	1
Reichsanstalt	1·009	0·926	1
Laboratoire Centrale	1·000	0·929	1
Laboratoire d'Essais	1·004	0·937	1

By taking the average of the ratios found in the two Paris laboratories as representing the French values for the lamps, then the mean of the three sets of results found in Paris, Berlin, and London gives a value for the Hefner lamp of 0·92 English units as represented by the 10-candle pentane standard burning in an atmosphere containing 10 litres of water vapour per cubic metre of pure air. As the generally accepted value for the Hefner lamp is 0·88, it shows a difference of 4 per cent. between the old and the new values. It is interesting to note that no value in these tables differs from the mean by as much as 1 per cent., and the majority are within the half of 1 per cent.

Comparison and Criticism of the Three Lamps as Standards of Light.

The measurements made at the National Physical Laboratory on the three principal flame standards now in use have given opportunities for comparing them under working conditions and judging of their respective suitability as standards of light.

As regards the Carcel lamp, it has not been found that its constancy from day to day is comparable with that of either the Hefner or the pentane lamp, its variations from the mean being as much as + or − 3 per cent. The reason for this can only be attributed to the difficulty in reproducing the same conditions of capillarity in the wicks used. Although all possible care was taken to produce the most favourable conditions of constancy, the results were not satisfactory, and in the notes which follow, the pentane and Hefner lamps only are considered with reference to their suitability as light standards.

General Construction.—The Hefner lamp, which is only one-eleventh the candle power of the pentane lamp, is much simpler in general construction, small and more easily set up, and should be simpler to manufacture and adjust to standard dimensions.

Ease of Regulation and Working.—It has been found in using the lamp that the flame of the 10-candle pentane standard is a great deal easier to adjust, and remains more constant while observations are being made, than that of the amyl acetate lamp.

The fact that the Hefner unit has a lambent flame, burning in free air, whereas the pentane standard is well shielded and, owing

to its chimney, has a more stable flame, makes the latter practically independent of draughts which would render measurements with the Hefner lamp quite impossible. Although the flame of the Hefner lamp may be shielded, slight movements of the air cannot be avoided; these disturb the flame so that it only remains at its correct height for a few seconds together, rendering adjustment difficult and the taking of candle-power readings rather a tiring process.

On the other hand, it is not safe to assume that, owing to the fact that the top of the flame of the pentane lamp is cut off, the latter may be allowed to vary appreciably in height. Variations of 3 mm. up or down do not materially affect the candle power, but for accurate work a second observer is required to see that the top of the flame is flat, and to regulate it to the correct height.

Effect of Atmospheric Changes.—As regards changing humidity, the two standards are affected to nearly the same extent, the Hefner lamp being slightly less influenced than the pentane lamp. The latter standard, however, is very much more sensitive to barometric variations than the Hefner unit, an inch change in pressure being equivalent to 2 per cent. in candle power.

The Nature of the Light.—The pentane lamp has a whiter light than the Hefner unit, being much more nearly the same tint as the Carcel standard.

The fact that its candle power is eleven times that of the amyl acetate lamp makes it of the same order of magnitude as the ordinary lights which are tested against it.

These two factors, coupled with the greater ease of adjustment when making observations with the pentane lamp, greatly outweigh, in the author's opinion, the disadvantage of the more complicated construction and the larger correction that has to be applied for changes in barometric pressure.

Secondary Standards of Light.*

Under this section the author proposes to give the results of tests made at the National Physical Laboratory on a number of high-voltage standard electric lamps.

The tests were made in order to ascertain what order of constancy may be expected from the best high-voltage glow lamps, with a view to using them as photometric standards.

Dr. Fleming's work on large bulb low-voltage incandescent lamps for use as standards is now well known, and the author, from several years' experience with large bulb Fleming Ediswan lamps as laboratory standards, can testify to their reliability when ordinary precautions are taken in using them. Dr. Fleming † has published descriptions of these lamps, but as yet no curves have been published showing

* See notes on the use of Glow Lamp Standards, J. S. Dow, *Electrician*, vol. 57, 1906, p. 855.

† *Journal Institution of Electrical Engineers*, vol. 32, 1903, p. 119. *Report of British Association*, 1904—"Large Bulb Incandescent Electric Lamps as Secondary Standards of Light."

their performance when run for a considerable length of time. Curves, therefore, showing the behaviour of these lamps during the first part of their life will be of interest, as they illustrate the peculiar properties of the lamps and the difference between low- and high-voltage glow lamps from the point of view of their suitability as standards.

Ten of these lamps were obtained two years ago from the Ediswan Company, for use as laboratory standards. They were mounted on fixed sockets with soldered leads as shown in Fig. 8. They were run for periods varying from 400 to 600 hours on a circuit the pressure of which was carefully regulated. Candle power readings were taken at frequent intervals during the run. The curves of six typical lamps are shown in Fig. 9. The efficiency of these lamps was initially about 4·5 watts per candle * and 4·3 after 500 hours. When compared with the life curves of ordinary lamps (Figs. 15 and 18) it will be noticed over what a long period the initial rise in candle power lasts. The best curve is that of lamp 15, which remained constant for 200 hours. Lamps 8 and 10 have just attained the end of their candle power rise, while in the case of lamp 11 there is no sign after 600 hours of any diminution in the rapid rate at which the candle power is rising.

The first point which is evident from an inspection of these curves is, that the performance of filaments specially prepared for constancy cannot be predicted with certainty. If a lamp is to be used as a photometric standard of the first grade, its history for 100 to 200 hours should be known, and its life curve should be closely watched in order to ascertain the point at which it is beginning to flatten out.

If the only photometric standards required were of the low-voltage type, the problem might be considered as solved. A properly aged specially prepared low-voltage filament if burnt for, say, ten minutes a day for five days in the week should last two or three years without changing the half of 1 per cent. in candle power, provided that no excess voltage is applied.

Where, however, the only source of supply is a high-voltage variable circuit, it is essential that the standard and the test lamp shall be run in parallel, so that the effect of voltage variations may be reduced to a minimum. This entails either the use of a low-voltage lamp with a fixed resistance in series, or a high-voltage lamp of reasonable constancy. The method of putting a resistance in series with a low-voltage lamp and running on a high-voltage circuit is not so satisfactory as it might appear at first sight. The resistance must dissipate some 60 watts and must keep constant to 0·1 per cent. if the candle power is to be correct to 0·5 per cent. If, further, the voltage variations on the circuit are great, it is essential that the temperature coefficient of the resistance shall be the same as that of the lamp.

In the tests under consideration six lamp makers kindly undertook to co-operate with the Laboratory in the investigations, by supplying high

* To avoid confusion the author has, in accordance with the usual engineering practice, given the efficiencies in "watts per candle" instead of "candles per watt."

voltage 16-candle-power lamps, with filaments which they considered would be most suitable for standard purposes. The filaments were in one plane and the bulbs of ample dimensions. The lamps numbered in all thirty-seven, and were run on a pressure-regulated circuit with frequent observations of candle power and current.

Sample curves from each batch are shown in Figs. 10, 11, and 12. In examining these curves, however, it should be borne in mind that as the efficiency of the various sets was different, the curves can only be considered as roughly comparable.

The average rate of fall in candle power for each make of lamp is given in Table II., as well as its average efficiency. The figures in column 4 are taken from the portion of the curve at which the rate of fall is most even.

TABLE II.

COLUMN 1. Make of Lamp.	COLUMN 2. Number Tested.	COLUMN 3. Average Initial Watts per Candle.	COLUMN 4. Percentage Fall in Candle Power per 100 Hours.
A	5	3·4	4·6
B	4	4·2	0·9
C	3	4·1	1·7
D	8	4·0	3·7
E	11	4·5	2·9
F	6	3·7	3·5

Make A has an initial efficiency which is unnecessarily high for standard work, but except for lamp No. 6 the life curves do not differ from those of ordinary filaments of high grade quality.

Make B shows an interesting set of freak curves. The candle power of lamp No. 7, with an efficiency of 4 watts per candle, has remained constant for over 300 hours to within the half of 1 per cent.

Other lamps of this type show the same characteristics as Nos. 9 and 10, which, after falling and again rising in candle power, finally reach an approximately constant value after 500 hours.

Lamp No. 7 shows, of course, the best performance, and is the type to be aimed at—but it is, unfortunately, the only one of its kind out of some five or six which have been tested.

Make C has some interesting features. Only three lamps were tested of this kind, but of these Nos. 12 and 13 show a high degree of constancy over a portion of their life—especially as they have the

comparatively high efficiency for 200-volt standards of 4 w
candle.

Makes D and *F* show no special features of interest, as they even fall in candle power throughout the whole of their life.

Make E. The foregoing remarks apply to Make E also, exc lamp No. 34, burning at 4½ watts per candle, which shows but gradual fall in candle power.

As a result of these investigations there is no doubt that high- lamps are to be obtained which remain constant for a consi time. They cannot, however, as yet be produced with certain trying a number of special lamps, some may be found which every way as good as low-voltage standards—but at present th alternative to this is to select a lamp with as small and as regular as possible, and rely on restandardisation after about twenty running. It should not be necessary to run a standard under o conditions for longer than forty-five minutes per week in all. case restandardisation would only be required every six months.

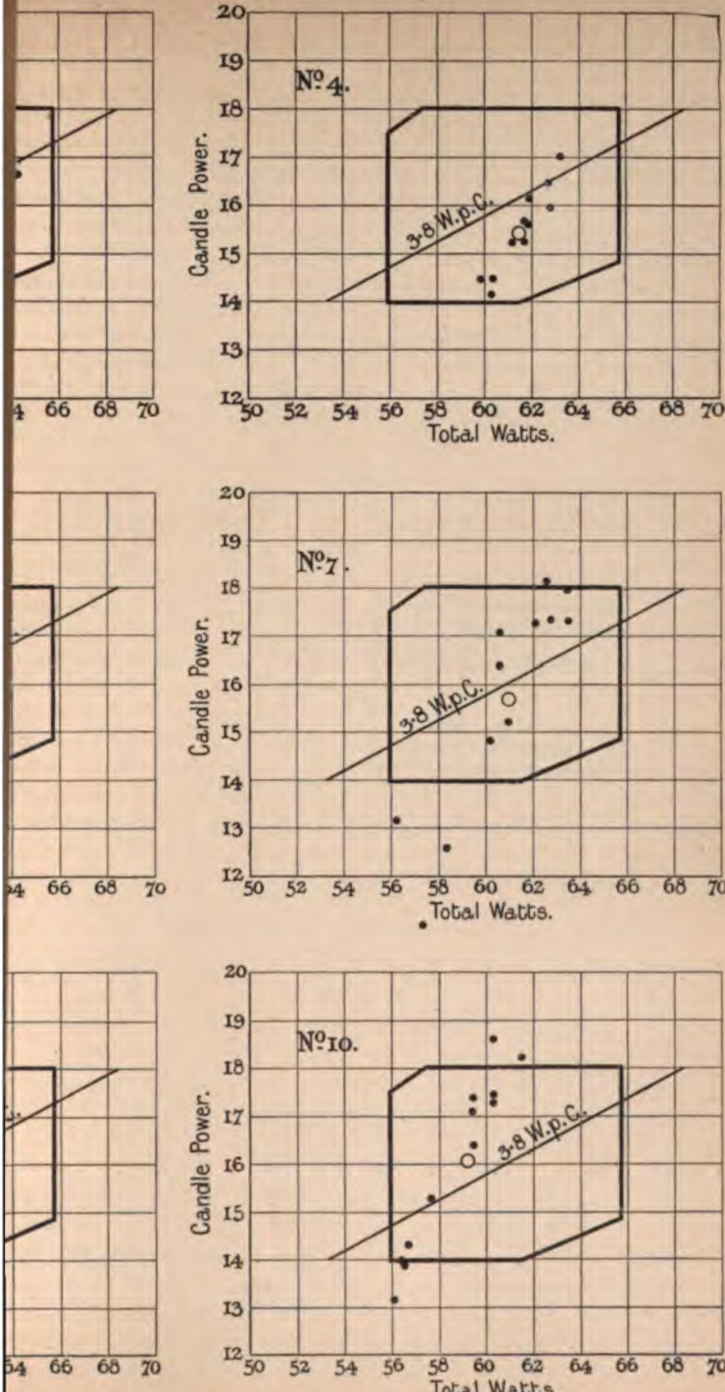
Commercial Glow Lamp Testing.

It should be unnecessary, after all that has been said rece emphasise the need for a closer supervision of the glow lamps are supplied in the ordinary course of business to purchasers country. The matter has been enlarged on by Mr. G. Wilk Mr. L. Gaster,† Mr. J. W. Howell,‡ and more recently by Sir W Preece.§ These writers have given data showing how in this c unevenly rated and low-efficiency lamps are supplied by m They miss the incentive to improve the quality and uniform their goods which arises from satisfactory inspection, and a tec knowledge of the subject on the part of the buyer.

Lamps which are otherwise good are classed for use on vo for which they are unsuited, and either give out early or have a and inefficient life. On the other hand, many lamps are sold w although of average efficiency, drop within a comparatively short to 80 per cent. of their original candle power. It is common k ledge that some of these lamps, though labelled with British name of foreign manufacture. The fact that some of the largest lamp m are so strongly in favour of inspection, and that they assisted Engineering Standards Committee in drafting the specificat speaks eloquently in favour of the desirability of supervising lamp supplies.

It is noteworthy that although the greatest effort is often mad obtain the last ½ per cent. efficiency in the machinery generating lighting current, often under severe penalties to the maker, an efficiency of 20 per cent. or more in the glow lamp, the last lin

* *Journal Inst. Electrical Engineers*, vol. 37, 1906, p. 52.
† *Journal Society of Arts*, vol. 54, 1906, p. 322.
‡ *Proc. Amer. Inst. Electrical Engineers*, vol. 24, 1905, p. 617.
§ *Report of Brit. Assoc.*, 1905. ‖ See Preface to Glow Lamp Specification



t 16-Candle-Power Lamps by ten British Makers.

transformation, is allowed to pass altogether unheeded and unchecked. It is surely in the best interests of the supply companies that the consumer should get the best value for his money.

In the specification of the Sub-Committee on Physical Standards of the Engineering Standards Committee, published this winter, rules are laid down for the inspection and testing of samples from consignments of glow lamps with the object of determining whether the delivery from which they were taken comes up to specification.

The tests specified may be divided into two classes :—

1. Tests to determine the initial rating of the lamps and their uniformity in candle power and watts.

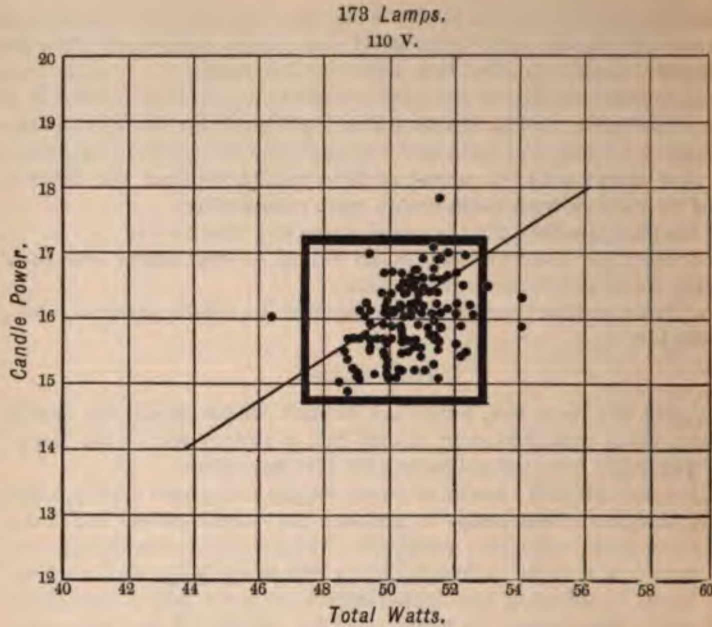
2. Tests to determine the maintenance of the candle power of lamps during life.

Initial Rating.

Under the first test, limits are defined within which the candle power, watts, and efficiency should fall, a percentage of the lamps being selected from a consignment for this inspection.

It is the present custom of many supply companies who provide their consumers with lamps to measure the candle power and watts of all the glow lamps they purchase. The practice is admittedly very laborious, and it is the author's opinion that in carrying out these tests the buyer is relieving the manufacturer of work which should be thoroughly done before the lamps leave the factory. If they have been carefully and properly tested first, it should only be necessary to check over a limited number of lamps from a consignment, and, if they are not satisfactory, to return them to the works for re-inspection, in order that wrongly rated lamps may be weeded out.

The Engineering Standards Committee has kindly given the author permission to publish the results of some tests which have been carried out for them at the National Physical Laboratory on 120 high-voltage glow lamps. These were made by ten British makers and were bought over the counter in the ordinary way. The Sub-Committee on Physical Standards, which has drawn up the specification for glow lamps, wished to know the grade of the filament, and the kind of uniformity, to be expected at the present time in lamps supplied to chance purchasers. The type of lamp chosen was for 16 c.p. at 200 volts. The lamps were tested for mean horizontal candle power and watts at the marked voltage, and the results plotted in the usual way on target diagrams. These are shown in Fig. 13, and plainly illustrate the fact that there is room for great advance by the makers in the uniform rating of their lamps. In Fig. 14 are shown sets said to be typical of American lamps. That on the left-hand side shows a set of American Edison 200-volt lamps bought on the open market, and tested by Mr. F. C. Bailey. That on the right-hand side shows a set of 110-volt lamps tested by the New York Electrical Testing Laboratories, who give this diagram as a fair sample of good American practice. The limits shown on these diagrams are those within which lamps must fall in



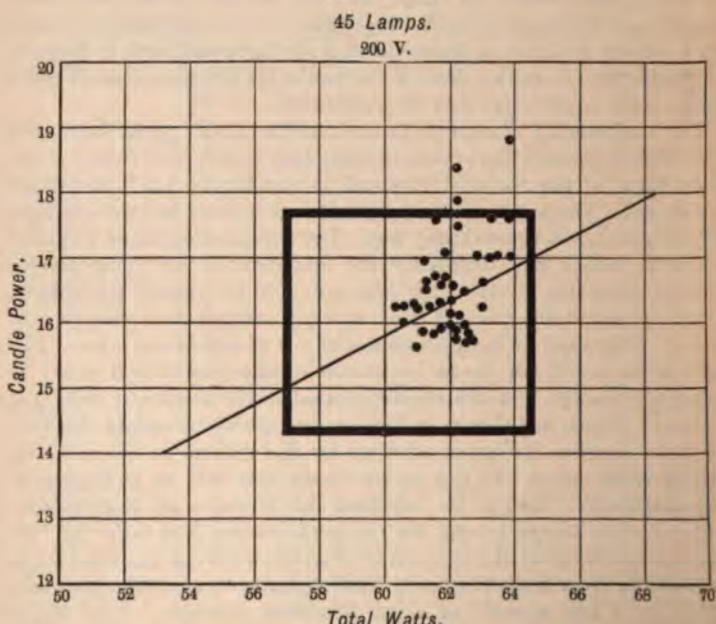


FIG. 14.—Target Diagram of American Lamps. From *Proceedings, American Institute Electrical Engineers*, vol. 24, 1905, p. 603.

order to meet the requirements of the American markets, whilst those shown by the six-sided figure in the centre of each diagram for the English lamps are the individual limits which would be allowed by the Standards Committee's specification for 3·8 watt lamps. The large circle in the centre of each diagram represents the mean of all the lamps.

A cursory inspection and comparison of the diagrams in Figs. 13 and 14 bring out many points of interest regarding the uniform rating of lamps. In batch 7 the candle power, nominally 16, varies, not unevenly, over a range from 11 to 18, the corresponding efficiencies being from 5·2 to 3·4 watts per candle, and the "useful life" * at the marked voltage varying, probably, from 2,500 hours in the case of the inefficient lamp, to 230 in the case of the efficient one. In group 2, on the contrary, although the candle power is uniformly low, the limits are within 13 and 15 candles, and the useful life at the marked voltage would lie, approximately, between the narrow limits of 340 and 650 hours.

The extreme values of candle power, watts per candle, and life for each batch are given in Table III. It must be admitted that these wide variations leave much to be desired.

TABLE III.

COLUMN 1. Make of Lamp (see Fig. 13).	COLUMN 2. Mean Horizontal Candle Power (Extreme Values).	COLUMN 3. Watts per Candle Power (Extreme Values).	COLUMN 4. Probable Life.†
Batch.			Hours.
1	16·1 to 21·5	3·04 to 3·75	43 to 130
2	13·1 to 14·8	3·83 to 4·19	340 to 650
3	15·2 to 17·8	3·56 to 3·90	330 to 660
4	14·1 to 17·0	3·72 to 4·26	470 to 1,040
5	12·9 to 20·4	3·15 to 5·02	76 to 1,170
6	12·1 to 18·4	3·29 to 4·65	136 to 1,120
7	11·0 to 18·2	3·44 to 5·21	230 to 2,500
8	15·3 to 19·4	3·12 to 3·76	180 to 540
9	13·0 to 16·1	3·33 to 4·25	190 to 790
10	13·1 to 18·6	3·24 to 4·28	160 to 820

It is rightly urged in extenuation of this condition of things, that the very unequal grading of supply voltages in this country renders it impossible for makers to supply lamps at a reasonable price which shall

* By "useful life" is meant, here, the time run before the candle power has fallen to 80 per cent. of its initial value. In view of the fact that lamps tested for life under the Standards Committee's Specification are those which are, initially, very near the standard candle power, "useful life" in this specification is defined as the time run before the candle power has fallen to 80 per cent. of its *Standard* value.

† The values given in column 4 are deduced from the curve shown in Fig. 17.

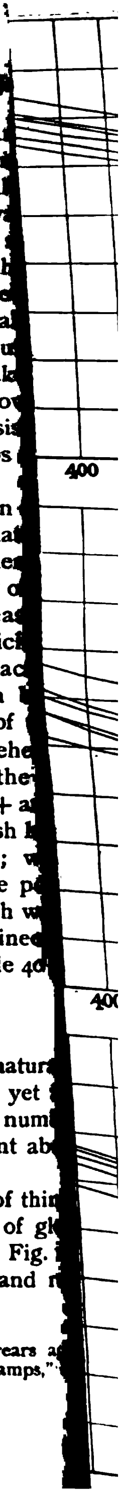
fall within the same narrow limits that are imposed under the A system. It is not possible in lamp manufacture to predict, eve fairly wide limits, what will be the voltage at which a lamp wi certain candle power and consume a specified number of w candle. There must, therefore, of necessity be a considerable of lamps which would be more suitable for, say, 215, and ot 225 volts, which are, nevertheless, rated for 220 volts, becaus small demand for lamps for the odd pressures, and the natura to keep down the number of outfalls to a minimum. Bu taking this into consideration, the fact that some English mak able to turn out a much more uniform product than others, sho improvement would be possible if the large buyers of lamps insi having them supplied to specification, and took proper steps that they did not fall short of the guarantees.

The author does not propose to enlarge here on the question grading of supply voltages, a subject which has been raised la Mr. G. Wilkinson * and Sir William Preece,† and discussed at le the technical papers. He merely points out what the grading o ages and the enforcement of a rigid glow lamp specification ha for the American consumer, and some of the consequences whic follow from adopting a similar practice in this country. The fa voltages in this country are so unevenly balanced, as shown William Preece's diagram, has made necessary the setting of wider limits than in America, and the lack both of a compreh specification and of the means of enforcing it has produced the of affairs shown by the diagrams in Fig. 13. If the American + a limit be applied to the average of each batch of these English shown in Fig. 13, about 45 per cent. of them would be rejected; if they are applied on the assumption that the average candle p is to be 16, there would only be 35 per cent. of the lamps which w pass the test. Even the wider limits specified by the Engine Standards Committee would, under these circumstances, exclude 4 cent. of all the lamps.

LIFE TEST.‡

The question of a satisfactory life test for glow lamps is natur one of much greater difficulty than that for initial rating; yet maintenance of candle power on life—or, in other words, the num of useful candle hours obtained from a lamp—is the chief point ab which information is required when buying lamps.

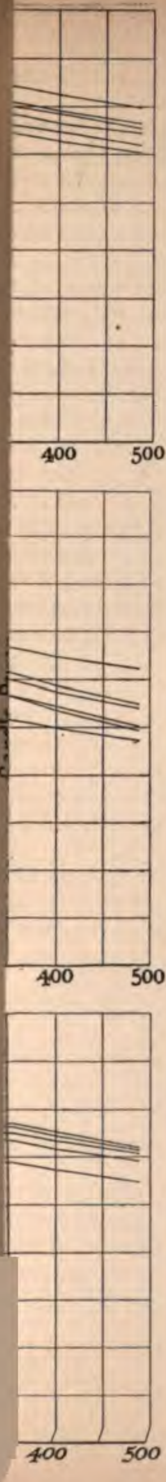
The diagrams shown in Figs. 15 and 16 illustrate the state of thi at the present day in this country as regards the performance of gl lamps on life test. Six out of each make of lamp shown in Fig. which came nearest the mean, were selected for life test, and



* *Journal Institution of Electrical Engineers*, vol. 37, 1906, p. 52.

† *Report of British Association*, 1906.

‡ For tests showing the typical performance of 100-volt lamps ten years a see *Proceedings of the Physical Society*, vol. 13, p. 439, "Tests of Glow Lamps," Professor Ayrton and Mr. E. A. Medley.


400
500
400
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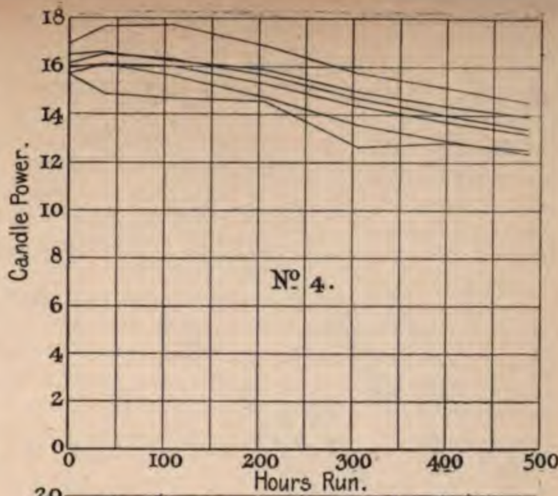
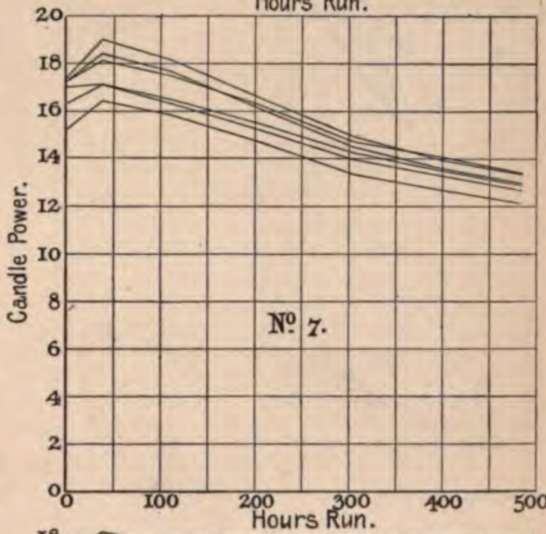
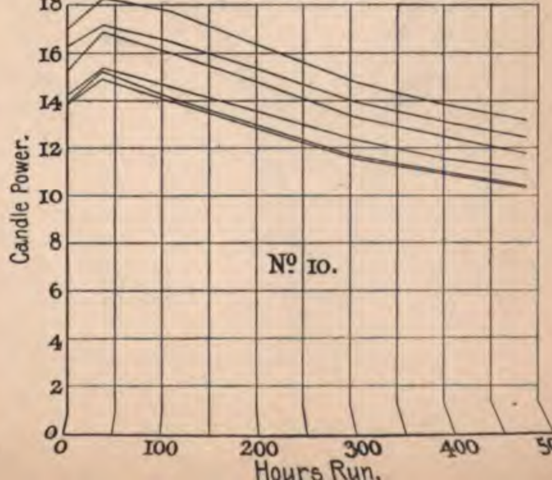
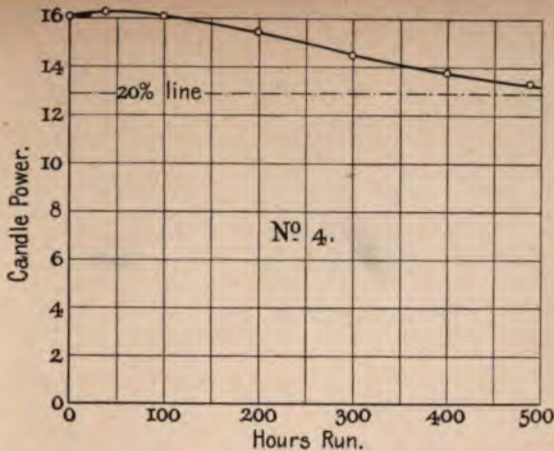

Candle Power.
18
16
14
12
10
8
6
4
2
0
0
100
200
300
400
500
Hours Run.
Nº 4.

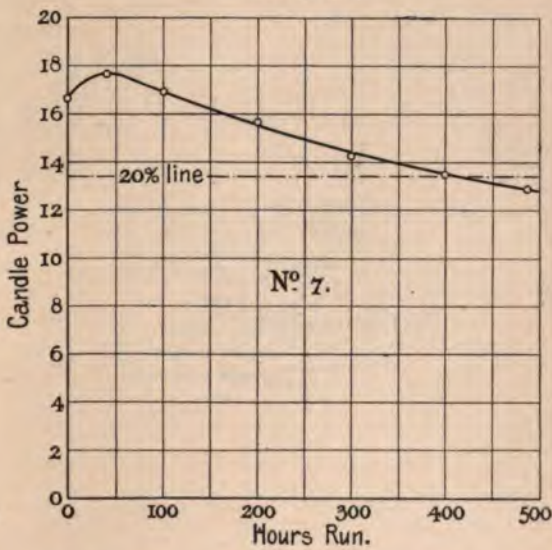

Candle Power.
20
18
16
14
12
10
8
6
4
2
0
0
100
200
300
400
500
Hours Run.
Nº 7.


Candle Power.
18
16
14
12
10
8
6
4
2
0
0
100
200
300
400
500
Hours Run.
Nº 10.


500


Candle Power.
20% line
Nº 4.
Hours Run.
0
2
4
6
8
10
12
14
16
100
200
300
400
500


0
500


Candle Power
20% line
Nº 7.
Hours Run.
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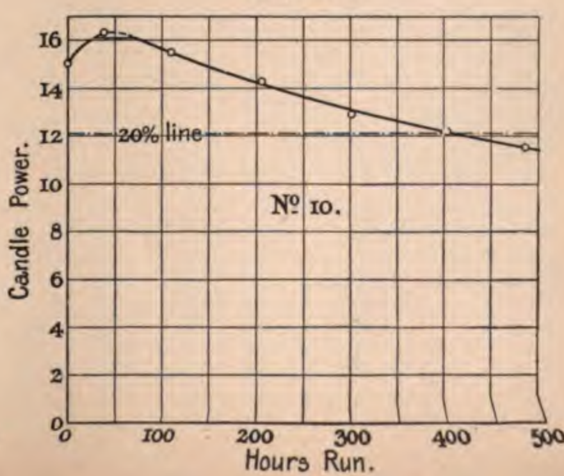
Candle Power.
20% line
Nº 10.
Hours Run.
0
2
4
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8
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12
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500

under identical conditions. The lamps were set up on the racks, and the voltage on each was so adjusted that the watts per candle at the commencement of its life were exactly 3·8. The test was considered at an end when the candle power of the lamps had fallen to 80 per cent. of the initial value.

Fig. 15 shows the performance on life test of individual lamps, and Fig. 16 the average of each set, candle power in these diagrams being plotted as ordinates and hours run on life test as abscissæ. The useful life obtained from the different groups will be seen to vary between the limits of 150 and 550 hours. In view of the great difference in quality which these results show to exist between different makes of filament, the desirability of some form of life test becomes apparent, in order to prove the reality of lamp guarantees.

In Table IV., column 2 gives the actual average useful life found for each batch—that is to say, the time the lamps will run before falling to 80 per cent. of their initial value. The results given in column 2, however, are not strictly comparable, since, as the filaments varied slightly in shape, the ratio of mean horizontal to mean spherical candle power was not exactly the same for all the types. As, therefore, the voltage on life test was adjusted so that the watts per *mean horizontal* candle power were 3·8, the lamps which emitted relatively more light in a horizontal direction than in a vertical one would really be running at a lower temperature than those which gave more light in a vertical direction. The values of $\frac{\text{M.S.C.P.}}{\text{M.H.C.P.}}$ for each type of filament are therefore given in Table IV., column 3, from which it will be seen that the average ratio was about 0·865. A correction has been applied in each case to the values in column 2, and in column 4 the average life is tabulated of each batch of lamps, on the assumption that its ratio of spherical to horizontal candle power is 0·865.

TABLE IV.

COLUMN 1. Make of Lamp.	COLUMN 2. Actual Life.	COLUMN 3. Ratio $\frac{\text{M.S.C.P.}}{\text{M.H.C.P.}}$	COLUMN 4. Probable Life Corrected to Ratio 0·865.
Batch.	Hours.		
1	140	0·88	153
2	370	0·884	416
3	487	0·846	426
4	535	0·862	525
5	225	0·876	241
6	315	0·84	262
7	415	0·878	450
8	570	0·88	626
9	415	0·868	422
10	415	0·86	402

Lamps tested for life in this country are usually run at their rated voltage until they either break down or their candle power has dropped to 80 per cent. of its initial value. This method, however, is open to serious objections. A lamp can only be guaranteed to have a life of given length when the filament is run at some definite temperature (see Fig. 17). This can be adjusted to a near degree of accuracy in carbon filament lamps by running them at some known watts per candle. If, for instance, there are a number of lamps in a batch which vary in efficiency from 2·6 to 3·6 watts per candle, the useful life of the latter may be expected to be about seven times longer than the former. If their life has been specified to be a certain length at 3·10 watts per

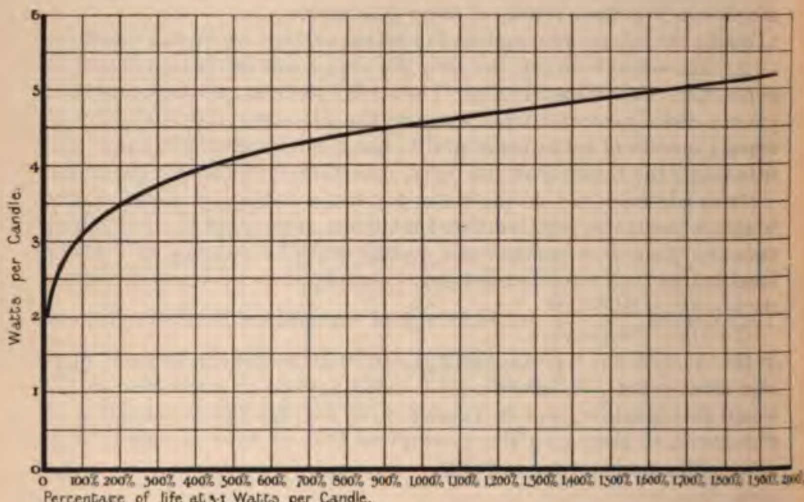


FIG. 17.—Curve showing the Percentage Variation in the "Useful Life" of Lamps when run at different watts per candle.

candle, then it will only be necessary to test, say, five lamps at this efficiency, and take the average of the five. If, however, the run should be made at the marked voltage instead of at the specified watts per candle, it would be necessary to test five or six times the number of lamps in order that there might be a reasonable probability that the life deduced from the mean should be within 5 per cent. of the true value. This is to be expected if it is borne in mind what large variations in the life are possible when the watts per candle approach the extreme values.*

* Mr. F. J. Selby, M.A., investigating the errors to which this method is liable, finds that, taking a certain type of lamp for which the limits specified of watts per candle power are 2·6 and 3·6, and the life respectively 140 and 960 hours—the possible variation from the mean life is ± 410 hours. Assuming a straight-line law, and supposing further, as is justified by observations, that it is twice as likely that a lamp

It has to be noted that the law connecting watts per candle and life is not a straight-line law. Even, therefore, if a large number of lamps were tested for life at their marked voltage, the average life found in this way is not the life of the lamp whose watts per candle is the mean of the lot, but would be some 10 or 12 per cent. longer (Fig. 17).

If there are enough lamps to choose from, it may be possible to find some which have their standard efficiency at the marked voltage, but it must be remembered that great accuracy is required in the watts per candle, as 1 per cent. above or below the mean will cause a 5 per cent. error in the life. The specification reckons drop in candle power from the standard, and not the actual initial candle power of a lamp. It is not often, therefore, that as many as five lamps can be found which are 16 candle power, and sufficiently near the standard watts per candle to insure the necessary accuracy in determining the length of life.

An inspection of the curves in Fig. 15 shows that when run at a definite initial efficiency, the candle power of individual lamps of a given make will keep very close together throughout their life—the average results obtained from five or six lamps presenting trustworthy data from which the whole consignment may be judged.

It should be observed when examining Fig. 15 that any divergence which occurs between curves of the same set is largely due to differences in the initial candle power. Had this been approximately the same for all lamps in a set, most of the groups would have presented the same compact appearance as No. 2.

To illustrate this point Table V. has been drawn up. It is assumed in this that all the lamps commenced their life at 16 candle power. Had all the lamps in a batch been the same as regards durability, they would then have followed the same life curve. If such were the case it would only be necessary to test one lamp for life out of the consignment. There are, however, slight differences between lamps of the same make which render it necessary to test more than one lamp. In the case in point six lamps of each type were tested. The area of each of these hypothetical curves starting at 16 candles, exactly, has been integrated up to 400 hours, the maximum plus and minus deviation of the area of these curves from the mean in the case of each group being given in column 2.* In column 3 the probable error in life is given if five lamps out of six be taken. Five lamps only are considered instead of six, as that is the minimum number which can be tested from a consignment under the new specification.

The table shows that five lamps being taken from each consignment, and being run on life test at definite initial watts per candle, in six cases out of nine the probable error is under 1 per cent., whilst the

should fall within the limits $\pm$ 205 (half the possible variation) as that it should fall without these limits, then the *mean* error due to taking the average of the lives of twenty-five lamps as the mean life is 5·5 per cent. In the majority of cases the error will of course be less than this, but in a certain number of cases it will be greatly exceeded.

* The freak lamp in Group 5 has been ignored for the purposes of this table.

maximum probable error is 1·3 per cent.; so that in practically all the tests made in this way results may be relied upon to within 2 per cent.

TABLE V.

COLUMN 1. Make of Lamp.	COLUMN 2. + and − Individual Maximum Deviations from the Mean Candle Hours to 400-Hour Point.	COLUMN 3. Probable Error in Candle Hours for Mean of Five Lamps.
Batch.	Per Cent.	Per Cent.
1	—	—
2	+ 3·2 and − 2·7	0·7
3	+ 3·2 and − 6·8	1·2
4	+ 3·9 and − 5·1	1·0
5	+ 6·1 and − 6·3	1·3
6	+ 2·9 and − 3·9	0·8
7	+ 2·7 and − 4·5	0·8
8	+ 3·2 and − 1·9	0·6
9	+ 3·7 and − 2·7	0·8
10	+ 2·2 and − 1·3	0·5

The cost of power in such cases, worked out on the basis of five lamps tested for life out of a consignment of, say, one thousand, comes to 0·36d. per lamp. This is reckoning that the power will cost 3d. per unit, voltage being specially regulated.

The test usually has to be made by arranging a short length of resistance wire in series with each lamp. Run under these conditions the pressure across the lamp terminals will not be more than 1 or 2 volts above or below the normal. It is obvious, of course, that a lamp which shows better results than another under these conditions will also prove better when run on an ordinary lighting system, with perhaps a slightly different and variable voltage. The additional advantage, among others, is gained that all tests on glow lamps have a common basis for comparison as regards upkeep of candle power during life. It is not a serious matter to group lamps on the life test racks so that only a foot or so of resistance wire is required in series with each lamp in order to reduce all to the same initial watts per candle.

The system adopted at the Electrical Testing Laboratories of New York is to adjust the voltage initially on each lamp so that it is burning at its standard candle power. The method insures that the 16-candle lamp, for instance, has the advantage of the full amount of drop from 16 to 12·8 candles before its useful life is considered at an end. The disadvantage is that on the life test it may be running at slightly above or below the correct watts per candle. The author prefers the method adopted by the Engineering Standards Committee of having the watts per candle rigorously correct. Lamps should then be chosen for the

life test which are nearest to the standard candle power. In practice, however, the difference between the two methods amounts to very little.

The average candle hours of useful life for each make of lamp are given in Table VI. The values have been corrected, as explained above (page 295), to allow for differences in candle-power distribution for the various types of filament. On the lines of the Glow Lamp Specification, a 3·8-watt 16-c.p. lamp should have 6,750 candle hours of useful life.

TABLE VI.

COLUMN 1. Make of Lamp.	COLUMN 2. Candle Hours of Useful Life.
Standard	6,750
Batch No. 1	2,280
2	5,650
3	6,650
4	7,700
5	3,560
6	4,150
7	7,150
8	9,540
9	6,040
10	5,700

VOLTAGE REGULATION.

Good voltage regulation during the life test is of vital importance. If the pressure fluctuates about a mean value, an excess voltage for a given time will not, as regards the life of the lamp, be fully balanced by a corresponding diminution in pressure for the same time. Fluctuations in pressure should, therefore, be kept to within + or — the half of 1 per cent. The accurate setting of the mean voltage is also of great importance, since it is not always appreciated that with ordinary glow lamps an increase of only 0·2 per cent. in the volts produces a variation of 1 per cent. in the life.

Mr. Howell, of the British Thomson-Houston Company, has kindly given the author permission to reproduce here the curve shown in Fig. 17. This curve shows the connection which exists between the length of the useful life of the lamp and the watts per candle at which it is started on a life test. The curve has been used by the author in this paper to calculate the length of life which glow lamps will have under other than the actual conditions of running. In the curve a 3·1 watt per candle lamp is supposed to have a life of 100 per cent., so that the percentage life at any other efficiency may be read off the curve. The

diagram illustrates how rapidly the length of life changes as the watts per candle vary.*

The curve suggests the question whether it is not possible to shorten life tests very considerably by running lamps at a high efficiency, and by deducing from the curve what would be the length of life at any other efficiency. The author is not aware whether curves exist similar to this one and based on a large amount of data, showing the performance of lamps manufactured by other makers, or whether such curves would agree closely with this one or not. It must be remembered that the relation shown by this diagram, although the result of tests on a very large number of lamps, only gives the performance for one make. It has yet to be proved whether all types would be the same in this respect.

It is likely, however, that down to a certain point the curve would give correct results within very narrow limits. When the over-running is increased, until the watts per candle reach as low a figure as 2 or under, other effects will probably come in and render the test untrustworthy (see Fig. 19).

Comparison between Normal Life Test and an Overrunning Test on Glow Lamps.

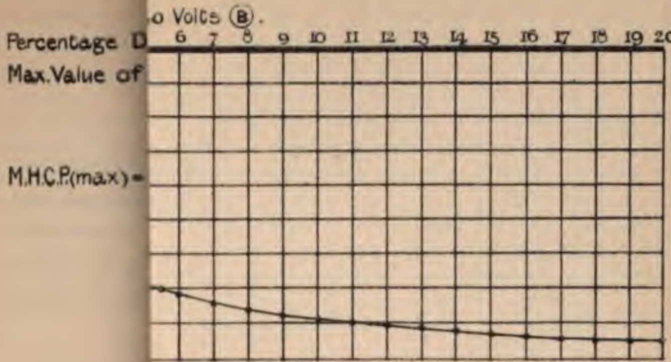
About two years ago a series of tests was carried out at the National Physical Laboratory on behalf of one of the Government Departments, in order to determine if it is possible by an overrunning test to predict the percentage drop which lamps would have after a given time if run under ordinary conditions. Lamps were ordered on a specification which entailed a life test for one hour at 40 per cent. increased voltage.

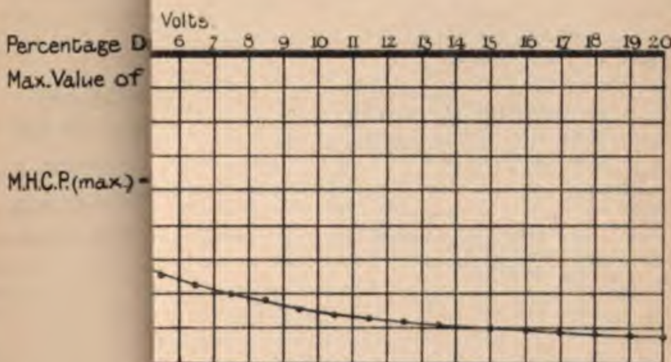
The specification is a modification of that which Sir William Preece adopted at the Post Office for testing the resistance of the filament to withstand rupture. His specification was based on an exhaustive set of measurements,† from which he found that in the case of the better class of lamps which he tested an increase of 70 per cent. in the voltage over a period of 2½ minutes did not appreciably diminish the candle power of the lamps; whilst a gradual increase of 100 per cent. in the pressure, over 3 minutes, should not cause rupture or failure of any kind.

Under the first specification mentioned above, samples are taken from a consignment of lamps and run for 60 minutes at 40 per cent. above the normal voltage. This, in the case of an ordinary glow lamp, is equivalent to about 1·3 watts per candle. At the end of this test the candle power must not have fallen more than a given amount from its rated value. It was desired to compare various sets of lamps tested under these conditions with an exactly similar set run on ordinary life test at the rated voltage.

* For curves connecting voltage, etc., and length of life, *see* paper by L. Gaster, *Journal of Society of Arts*, vol 54, 1906, p. 322.

† *The Electrician*, vol. 37, 1896, p. 733.





40 per Cent. increased Voltage

For the purpose of the test, three different makes of lamp were supplied, from which a careful selection was made, in order to obtain from each type lamps which matched as regards watts per candle. Six from each make were then chosen for the overrunning test, and six for the run at normal voltage.

As the lamps in question had filaments which varied considerably in form, it was found that the watts per mean horizontal candle power did not form a sufficiently accurate measure of the intrinsic brightness of the carbon, and it was deemed desirable to make all comparisons on the basis of watts per mean spherical candle power. Polar curves of two of the lamps which differed most in their light distribution are given in Fig. 20.

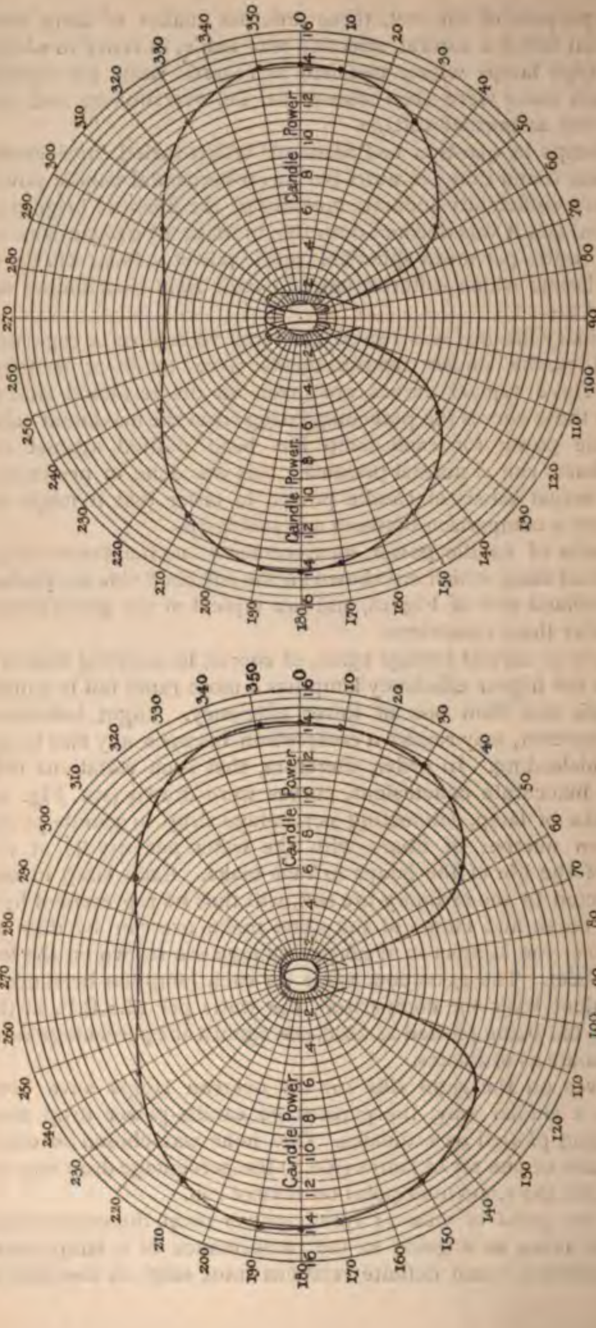
The ordinary life test was run at the rated voltage on a regulated circuit, and candle power measurements were made at frequent intervals. Three typical curves are given on the left-hand side of Fig. 18. In these curves the percentage drop from the maximum value of the candle power for each lamp has been plotted against the number of hours run at normal pressure ; this was done in preference to plotting actual values of candle power, in order that it might be easier to draw a comparison between any two lamps.

The results of candle power measurements on the three lamps which matched those which are shown on the left-hand side are plotted on the right-hand side of Fig. 18, and are typical of the performance of lamps under these conditions.

The curves at normal voltage agree, of course, in showing that in a general way the higher efficiency lamp has a more rapid fall in candle power on life test than one of lower efficiency. Slight individual variations, however, may render a comparison between any two lamps somewhat misleading. In order, therefore, that such variations may not lead to inaccurate conclusions, curves were drawn (see Fig. 19) for each make of lamp, connecting percentage drop in candle power after a given number of hours with the watts per candle at the beginning of the life of the lamps in that make. Each lamp in one make, subjected to the ordinary life test, will thus be represented by a dot on one curve and lamps of the same make overrun by dots on another curve ; the lamps run at normal voltage are shown on the left side of the sheet. The 1,000-hour value for each lamp has been taken as a convenient basis for comparison, the watts per M.S.C.P. at the beginning of life being plotted as abscissæ, against the percentage drop after 1,000 hours as ordinates.

The curves on the right side for the overrun lamps have been deduced in a similar way, the percentage candle-power drop after one hour being plotted as ordinates. One hour was chosen in order that in the case of one set of lamps (No. 3) the percentage drop may be the same as for the 1,000-hour point on normal run.

If, from the point of view of candle-power drop, the overrunning test is to be taken as a guide to the performance of a lamp under normal conditions, some definite relations must exist, in the case of



Reduction Factor 0·84.

Reduction Factor 0·90.

FIG. 20.—Candle Power Distribution Curves for Looped Filaments, showing Extreme Differences in the Ratios of Mean Spherical to Mean Horizontal Candle Power.

lamps by different makers, between those run on life test at normal pressure and those which are overrun. For instance, it will be seen that in make No. 3 (Fig. 19) a 4·5 watt per M.S.C.P. lamp gives a 24 per cent. candle-power drop, either when run for 1,000 hours at 100 volts or for one hour at 140 volts. The other lamps would, therefore, be expected to show a similar equality in this respect. This, however, is not the case, as may be seen from the following table, in which percentages are obtained from the curves :—

TABLE VII.

COLUMN 1. Initial Watts per M.S.C.P.	COLUMN 2. Make of Lamp.	COLUMN 3. Percentage Drop in C.P. on Life at Normal Volts at 1,000 Hours.	COLUMN 4. Percentage Drop in C.P. on Life at 40 per Cent. Increased Volts after 1 Hour.
	No.	Per Cent.	Per Cent.
5·0	1	22	20
	2	13	11
	3	12	16
4·5	1	33	29
	2	21	13
	3	24	24
4·0	1	44	38
	2	29	16
	3	36	32

From these figures it may be seen that not only does the fall of candle power in the two tests disagree in different types of lamps, but that the slope of the curves is different for the same make of lamp, so that equality in candle-power drop in the case of a low-efficiency lamp may be consistent with a difference of 40 per cent. for one of higher efficiency. Hence, from the point of view of maintenance of candle power, a 40 per cent. overrunning test on a lamp cannot be regarded as a certain guide to its behaviour at normal voltage.

This conclusion only applies to such an excessive overrunning as 40 per cent. in volts, corresponding to about 1·3 watts per candle.

It is quite possible that a life test at 2·5 watts per candle would give results so nearly consistent that all life tests could be run at this figure, enabling the length of the test to be curtailed from 400 hours to about 120 hours. It is hoped that a series of tests may be undertaken in the near future to prove whether this is possible—and what agreement may be expected between different makes of lamps in this respect.

MEASUREMENT OF MEAN SPHERICAL CANDLE POWER.

For the measurement of mean spherical candle power in the tests described on page 301, there was no apparatus at the Laboratory for

taking spherical candle-power observations in one reading, and it was at first found necessary to use a point by point method. The lamp is supposed fixed at the centre of a sphere, and the surface of the latter is divided up into zones by eighteen equidistant parallel planes at right angles to the vertical axis of the lamp. These planes will form parallels of latitude on the surface of the sphere. Eighteen candle-power observations were made at intervals of 20° round each of these circles, comprising in all 324 readings for each lamp. If the average for each zone is plotted in a polar diagram the mean candle-power distribution curve for each lamp is obtained. The usual Rousseau diagram is then plotted and the ratio of the mean spherical to the mean horizontal candle power deduced.

The laborious nature of the candle power measurements in these tests led the author to try the accuracy, by actual measurement, of a method for determining mean spherical candle power originated by Mr. Alex. Russell,* who was the first to point out that by selecting the correct angles in the vertical plane, the accuracy of the result would not be appreciably diminished if the sphere be considered divided up into considerably fewer than eighteen zones, thus greatly reducing the number of observations necessary.

The table given below shows the mean spherical candle power of two glow lamps, obtained by taking readings at 2, 4, 6, 8, 10, and 20 angles respectively, from which it will be seen that the error introduced in these cases by taking 4 instead of 20 readings is only 2 per cent., and by taking 6 instead of 20 is only 1 per cent.

TABLE VIII.

Angles of Depression and Elevation from Horizontal in Degrees at which Candle-Power Readings were taken.	Mean of Readings at Given Angles (M.S.C.P.).	
	Lamp 1.	Lamp 2.
30	12·8	14·9
14·5, 48·6	$12{\cdot}2_5$	$14{\cdot}4_5$
9·6, 30, 56·4...	$12{\cdot}1_5$	14·3
7·2, 22, 38·7, 61	12·1	$14{\cdot}1_5$
5·7, 17·5, 30, 44·4, 64·2	$12{\cdot}0_5$	14·3
2·9, 8·6, 14·5, 20·5, 26·7, 33·4, 40·5, 48·6, 58·2, 71·8...	12·1	$14{\cdot}1_5$

The above test could have been shortened without appreciably affecting its accuracy by rotating the lamp and measuring what may be called the conical candle power of the lamp in various directions.

* "Mean Horizontal and Mean Spherical Candle Power," *Journal Institution o Electrical Engineers*, vol. 32, 1903, p. 631.

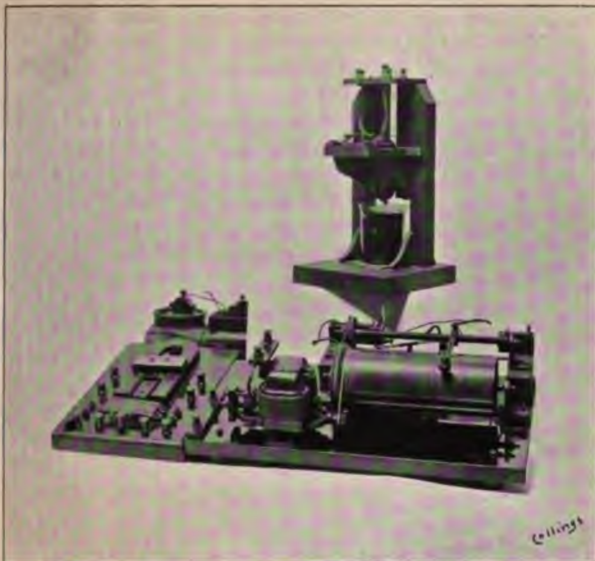


FIG. 21.—General View of Automatic Voltage Regulator.

FIG. 22.—Automatic Voltage Regulator—View of Rocker and Mercury Cups.

We may define the mean "conical" candle power at an angle θ, as the mean candle power in a direction making an angle θ, with the equatorial plane of the lamp. If readings had been taken at angles of 9·6°, 30°, 56·4° above and below the equatorial plane of the lamp, the mean of the six readings would have given the M.S.C.P. with an inaccuracy of about 1 per cent. This is the principle used in the Russell-Leonard integrating photometer.* Mr. L. W. Wild † has made the ingenious suggestion of measuring the candle power in directions in the vertical plane, making equal angles with one another and multiplying the candle power in the various directions by certain factors before taking the mean value. This method, however, like the Rousseau method, entails spending an equal time in taking measurements near the pole and near the equator. The Russell method of increasing the number of readings as the equatorial zone is approached is preferable from the theoretical point of view, and the arithmetic is simpler. It should be noted, when employing this method, that in taking six readings at the angles indicated, no light is considered lost through the presence of the lamp socket, which actually cuts off a certain amount of light. The method assumes that the distribution curve is the same at the socket end as at the tip end of the lamp. If the mean spherical candle power of the lamp as a whole is required, the ratio by this method will be slightly high. If, however, it is desired to obtain a measure of the intrinsic brightness of the filament the Russell method will give more accurate results.

Voltage Regulator.

A voltage regulator for life tests has been in use for the last three years at the National Physical Laboratory. It was devised by Mr. Rayner and the author for automatically varying a resistance in series with the field circuit of the alternator which supplies current to the life test lamps. It is not suggested that the regulator will work continuously without attention or that it is in a form in which it could be used in an engine-room; as a piece of laboratory apparatus, however, it works satisfactorily and adjusts alternating-current voltage automatically to within the limits of + or — one half of 1 per cent.

A photograph of the arrangement is shown in Fig. 21. It consists in the main of three parts :—

(1) A primary alternating-current relay (shown at the back of the figure) actuated by means of Leclanché cells

(2) a second relay (shown on the left). This starts—

(3) a small motor either forwards or backwards which drives a drum of resistance wire, rotating under a sliding contact brush. The automatic adjustment of this resistance, which is in series with the generator field, raises or lowers the voltage so as to keep it constant.

* A. Blondel, "Mesophotometers and Lumenmeters" (*Société Internationale des Electriciens*, Bull. 4, 1904, p. 659 ; *Science Abstracts*, vol. 8 B, p. 223).

† *Electrician*, vol. 55, 1905, p. 936.

The first relay is built up in the same way as a Siemens dynamometer, with two coils at right angles to each other. These are connected in series with a resistance and put across the alternating-current voltage which is to be regulated.

The movable coil is suspended by a silk fibre and controlled by a coiled spring. It has a long arm attached to it, and if the torsion on

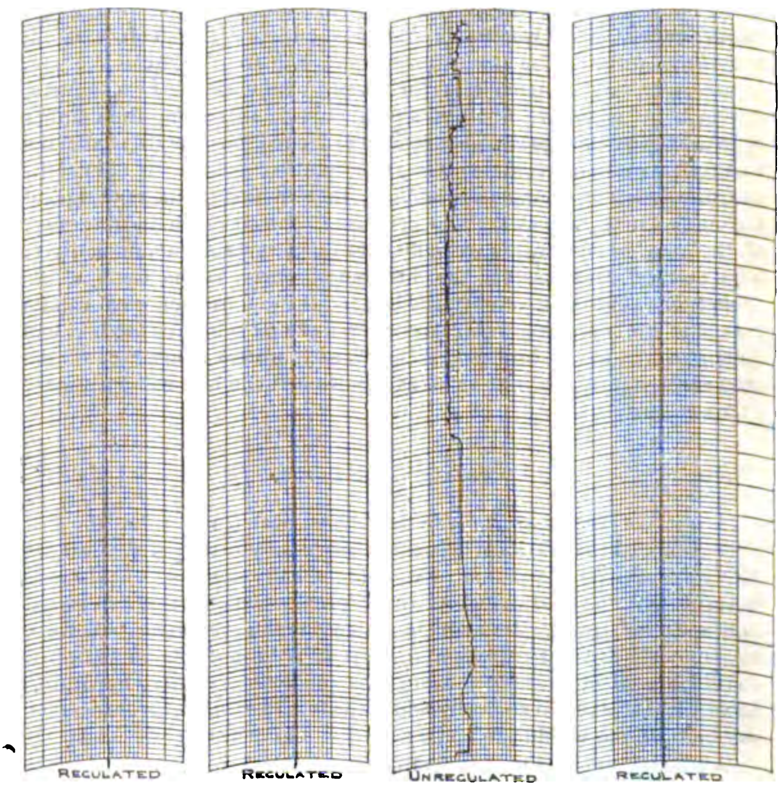


FIG. 23.—Voltage Charts on Regulated and Unregulated Circuits.

the spring is correctly adjusted this arm floats midway between two stops when the voltage is normal. If the voltage is too high or too low, contact is made on one side or the other, and current passes from the Leclanché cells to actuate the second relay.

The arrangement was found very unsensitive and unreliable in its working when the pressure between the arm and stops alone was used for making contact. The device was therefore resorted to of using a

long, fine steel-wire rocker, actuated by the arm, and dipping either side into mercury cups according as the voltage was above or below normal (see Fig. 22). This improved the sensitiveness of the apparatus, but due to the reluctance of the steel rocker to leave the mercury the arrangement still had a tendency to "hunt." This was due to the fact that the main voltage on the relay had to be raised or lowered by the small amount necessary to produce, at the end of the arm, sufficient force to pull the rocker out of the mercury. As soon as it left the mercury it of course rocked over to the other side, and the same process was repeated.

The trouble has been remedied by permanently magnetising the steel-wire rocker. The current from the Leclanché cells which actuates the second relay is then made to pass round one or other of the two soft iron rings shown at the top of the mercury cups. As these are wound with a number of turns of fine wire, a field is produced as soon as contact is made, which repels the rocker with a force equal to the "stiction" of the mercury. The rocker, therefore, both touches and leaves the mercury without the necessity of applying any extra force at the point of contact.

The second relay, being actuated by direct current, did not offer any real difficulties. It consists of a horizontal moving coil which rocks over one side or the other, making contact between four mercury pools, and driving the motor either forwards or backwards. The circuit is broken under oil, which covers the surface of the mercury.

A set of volt records, three with the regulator working, and one without it, is shown in Fig. 23.

* * * * *

In conclusion, the author wishes to emphasise the importance both of accurate photometry and accurate electrical adjustments in connection with the rating and testing of glow lamps. The specification recently issued by the Engineering Standards Committee provides a sound basis upon which glow lamps may be bought and tested, but it is essential, in view of the sensitiveness of the glow lamp to small changes in pressure, that all measurements under it shall be conducted with a maximum degree of accuracy.

The waste of money per annum in this country due to using unsuitable lamps must be very great, and the only way to stop the waste is to encourage those manufacturers who rate their lamps properly and who give an effective guarantee of their quality. It should further be noted that, in order to obtain all the advantages accruing from the accurate rating of glow lamps, it is essential that supply engineers should regulate* the voltage at the consumers' terminals within the narrowest possible limits.

In connection with these tests the author desires specially to acknowledge the obligation he is under to Dr. Glazebrook, Director

* Sir William Preece, *Report of British Association*, 1906.

of the National Physical Laboratory and Chairman of the Sub-Committee on Physical Standards which has been engaged in the work of drawing up the Glow Lamp Specification. He also wishes to thank Mr. Rayner for his ready assistance and advice, and to acknowledge his indebtedness to Mr. Selby and other members of the Laboratory staff for their counsel and help.

Discussion.

Dr. Fleming. Dr. J. A. Fleming: The paper presented to us is undoubtedly one of great value, containing as it does a large store of information on incandescent lamps and standards of light gathered as a result of most careful work. In fact, one may say that nowhere except at a National Physical Laboratory could such work have been done. In a teaching laboratory like my own, where any research work has to be conducted intermittently, it is impossible to give the time and exclusive use of appliances required for this class of work. Hence I am much interested in noting the results obtained in connection with matters to which I have myself given considerable attention.

In the first place the author has made a very thorough examination of the difficulties and peculiarities of existing flame standards of light, and he piles proof upon proof confirming the statements I made in my paper* four years ago as to the great effect of moisture, vitiated atmosphere, and mode of usage on the light emitted by the pentane and amyl acetate and other flames, giving numerical data and correcting formulæ more complete than those I was able to furnish.

We then see that, if these corrections are not applied and if these flame standards are taken haphazard on any day of the year, the illuminating power may be uncertain to the extent of 10 per cent., and the observer may be reckoning as 10 candles that which is really only 9, and that, apart from a careful use of the barometer, hygrometer, and sufficient ventilation even a pentane standard may mislead you as to light as much or even more than an ordinary common commercial ammeter may mislead you as to current. Even when the atmospheric condition is carefully noted and corrections are applied, I find there remains a difficulty with regard to the exact position of the top of the flame. When you look at a pentane flame through the little mica window in the chimney, you see that it is not sharp and square at the top, but ends in little tongues of flame continually dancing up and down, and, apart from great experience and constant attention, one cannot adjust it to the right height; and although this part of the flame does not send out light to the photometer, it influences that which does, and affects the light of the lamp.

It was in the attempt to overcome these difficulties that I was led to propose the use of the large bulb incandescent lamp standards which

* *Journal Institution of Electrical Engineers*, vol. 32, 1903, p. 119.

Mr. Paterson has also carefully examined. In the first place let me say that I still maintain (in spite of some contradiction) my opinion as to the value of the large glass bulb, which I believe I was the first to suggest. The loss in light of an incandescent lamp with age is very nearly all due to the obscuration of the glass, and it stands to reason that the larger the glass surface and the farther it is from the filament, the less will be that obscuration for a given use. In the next place I believe that in the large bulb carbon filament lamp (provided it is properly made and aged to begin with) we have a far more perfect means of preserving a standard of light than any flame standard yet made. We have no corrections to apply for atmospheric conditions; all we have to do is to pass through the lamp a known measured current. I am glad to see that Mr. Paterson uses invariably soldered leads to these lamps, as the use of caps, though convenient, may lead to errors; but with soldered leads, a potentiometer to measure the current, and the double weighing method of photometry suggested by me four years ago, very consistent results can be obtained. The manufacturing difficulty is that of securing uniformity in the structure of the carbon filaments used for making the lamps. The result I aimed at was to secure that after the first small rise of candle power the emission of light for a constant current should be as nearly as possible constant for several hundred hours. The slight initial rise in candle power is due to a slow annealing of the carbon by which its resistance is reduced so that it takes more current at constant volts, and therefore gives more light. If you look at the life curves the author has given for the six Fleming-Ediswan standards, you will see that lamp No. 15 almost perfectly fulfils my ideal. There is a small initial rise in candle power, of not more than 1 per cent., then a constant period for nearly 300 hours, and then a slight falling off in candle power, current being meanwhile practically constant. Lamp No. 9 exhibits very nearly the same performance, but the initial rise and final fall in candle power are a little more pronounced, being 2 and 3 per cent. In lamps Nos. 10 and 14 the practically constant period seems limited to about 100 hours, and for some reason or another the period of rise of candle power amounting to merely 3 per cent. has been prolonged from something about 100 to more nearly about 300 hours. Lamps Nos. 8 and 11 are certainly abnormal. In lamp No. 11 the constant period has never been reached at all, and during the whole 600 hours the candle power is continually rising. The total increase in 500 hours amounted to about 10 per cent. in lamp No. 11 and 6 per cent. in the case of lamp No. 8. In any less careful hands than Mr. Paterson's I should have been inclined to doubt the results, but in view of his experience I can only assume that in these two lamps less care has been taken in the preparation of the filaments than ought to have been the case. Dr. Fleming.

In my paper on Photometry I specified that the filaments should be run in ordinary bulbs for about 50 hours at 5 per cent. above their marked voltage, and then those which showed no defects should be

Dr. Fleming. cut out and remounted in the large clear bulbs. It may perhaps be best to specify this preliminary ageing not by a fixed number of hours of running, but up to such a time that the candle power shows no signs of increasing. If a certificate to this effect were furnished with each lamp by the manufacturer, it would be a guarantee against the sending out of lamps as photometric standards which have not received sufficient preliminary ageing.

I find by inquiry that the large bulb Fleming-Ediswan lamps which Mr. Paterson has tested were run at the factory for 50 hours as specified by me before sending them to him. It is evident, however, that it is not sufficient to specify a hard and fast number of hours for the ageing of a photometric standard lamp. I am inclined to think it should be run for at least 100 hours, and photometric measurements taken for 24 hours. If, then, four such observations should show that the initial rise in candle power has clearly ceased, the lamp might be sent out with the manufacturer's certificate to this effect, a photometric certificate being then obtained from some standardising laboratory. If, however, the result, say of 4 days' running, is to show that the candle power is continually increasing, then it should not be sent out, or if it is sent out, the fact should be stated.

Nevertheless, even taking the very worst case, it is easy to see that a lamp of this kind once certified in some way as to its actual candle power affords a far better means of conveying to another place or reproducing a known candle power than by the employment of a flame standard. If such a photometric standard is used simply to set the photometer in the way I have specified, such setting occupying ten minutes or so, we can use it 500 or 600 times before its total time of burning will have added up to 100 hours. Taking, then, the worst lamp of the batch tested by Mr. Paterson, viz., the Fleming-Ediswan standard lamp No. 11, its change in candle power in the neighbourhood of 13 candles is about $\frac{1}{4}$ of a candle power per 100 hours, in other words, 2 per cent. Hence the user of such a lamp could employ it at least a couple of hundred times as a standard before its actual value altered 1 per cent. On the other hand, for the best lamp, No. 15, it could be employed 1,000 or 1,500 times before the candle power altered by much less than 1 per cent., apart altogether from any personal errors in the process of making the measurements. I should, however, incidentally like to ask the question how the candle powers of these large bulb lamps were determined during the progress of their life? Were they measured against a pentane standard, or were they measured against other large bulb standards which were used only occasionally?

In the next place I should like to refer to the question of the ratio between the Hefner unit and the pentane unit. Mr. Paterson gives this ratio as 0·914, in other words, H.U. = 0·914 P.U. In order to determine this ratio for ourselves, in July last Mr. Clinton kindly took three of our large bulb Pender laboratory standards to Berlin and had them tested at the Reichsanstalt at 95·9 volts, corresponding to our

96 volts, to allow for the difference between the Berlin and British values of the Clark cell, and these three lamps were marked respectively P_4, P_6, and P_{10}, and were certified at Berlin as 15·1, 12·6, and 15·4 Hefner units. The same lamps were sent ten days later to Mr. Paterson, who kindly measured them at the National Physical Laboratory at 96 volts, and they were certified respectively as 13·41, 11·12, and 14·07 P.U. or candle power. These three measurements, therefore, give the ratios respectively as 0·888, 0·882, and 0·885. This ratio is about 3 per cent. less than 0·914. If we take the factor 0·914 for converting Hefner to pentane units, then the Berlin measurements of these lamps are 3 per cent. greater than the N.P.L. measurements. Dr. Fleming.

Mr. Clinton measured these lamps a day or two ago against one of our standards P_3 which the N.P.L. had certified to be 12·28 P.U. at 96 volts, and he found the three lamps P_4, P_6, P_{10} in terms of P_3 to have the values 13·64, 11·17, 14·28 candle power.

Thus the ratios of the candle powers of these three lamps are found to be nearly the same in all three laboratories. I should, however, like to ask Mr. Paterson if he can throw any light on the reason for this large difference in the ratio H.U./P.U. found by him and by me.

In regard to the absolute values of our standards, in 1896 we determined the value of the large bulb lamp we called P_3 against pentane standards, but without moisture or barometric correction, and made it to be 12·75 candle power as mentioned in my Institution paper, 1902. In 1904 Mr. Paterson gave us the value of this lamp as 12·28 P.U. According to the recent ratio measurements made of P_3 against P_4, P_6, and P_{10} combined with the absolute values given at Berlin for the three latter lamps, the value of P_3 should be 13·58 H.U., and with a converting factor of 0·914 this gives 12·4 candle power, which differs less than 1 per cent. from the value 12·3 assigned to it at the National Physical Laboratory. These differences, however, are photometric differences; the values of the currents taken by lamps at the same voltage are in close agreement as measured at all three laboratories—Berlin, the National Physical, and my own.

As regards the demand for a high-voltage photometric standard, Mr. Paterson's curves show that there is a much greater difficulty in meeting it. I must confess I cannot see why the low-voltage lamp cannot always be used. Any photometric testing worthy of the name must be done off secondary batteries and with potentiometer measurements, and there is no difficulty in employing a low-voltage lamp with a resistance in series provided we determine the candle power by the current and not by the volts.

In conclusion, let me say time does not permit me to touch upon many other points of interest in Mr. Paterson's paper, but I think the information he has given us on standard lamps will be useful in assisting further efforts to produce a standard photometric glow lamp far more convenient for electric purposes than any flame standard.

Mr. C. J. Robertson: This paper is valuable as showing the great care and precautions necessary in making anything like a fair com- Mr. Robertson.

Mr. Robertson.

parison between various batches of glow lamps. The Engineering Standards Committee have just issued a standard specification for carbon glow lamps, which has been drawn up with the assistance and co-operation of the largest British lamp-makers, who were unanimous in their approval of such a specification. It should be of considerable value to the electrical profession as a basis of what can be guaranteed to their clients in the future as to the performance of present-day carbon glow lamps, and it is hoped it will do something to keep out lamps of unknown origin, which manufacturers are very keen to see brought about. In this respect I think a very valuable suggestion was made in this room by Mr. Hirst, that in connection with this specification there should be some authorised form of "hall-mark" which could be placed on lamps which actually comply with the specification. As to the flame standards which have been given in detail by the author, they show, I think, that none of them are really likely to be used by engineers in general for the testing of glow lamps, owing to the great reliability of the carbon glow lamp when properly standardised as recommended by Professor Fleming. He has shown that we have there a means of obtaining accuracy to within such a point that it is ridiculous to go further. In their general behaviour such high-voltage standards, as compared with low-voltage ones, give much the same results as do ordinary high-voltage lamps compared with low-voltage lamps. The comparative life tests of big bulb glow lamps of high and low voltage would have the same ratio to one another as the life tests of similar filaments in smaller bulbs, but great allowance has to be made for the size of the bulb. The figures given show the remarkably fine behaviour of filaments when put into extremely large bulbs, and are a lesson as to the great advantage that might accrue if all filaments could be put into larger bulbs than those which are generally in use. On page 290 it is stated that "The makers miss the incentive to improve the quality and uniformity of their goods." I think this is not the case, as there is, beyond a competition in price, an ever-growing competition in quality, and this point is receiving far greater attention than was given to it in earlier years. As long as circuits are so very variable a standard specification will do no more than give us a theoretically better batch of lamps; for, after all, if a nominal 200-volt circuit is mostly run at 210 volts, it is the lamp-makers who first acquire this information and then have to supply a slightly higher voltage lamp to suit it, whereas a consumer on such a circuit would specify for 200-volt standard specification lamps and not get such good results as he previously obtained. This shows that, before the specification can be of general use, there must also be some standardisation of average and extreme voltage pressures, so that in any case the consumer will know whether a 400-hour lamp or an 800-hour lamp is the most suitable for a given circuit. Fig. 14 shows a diagram of what is typical of American lamps; there is no doubt that, when the English makers have the same conditions of approximate equal voltage grading, they can show the same results,

but at present we have not those conditions. There is no difficulty in reproducing daily such lamps here now, but owing to the restricted field for certain outside voltages in England as compared with America, and as was very well illustrated by Sir William Preece's diagram that has been shown to-night, this would entail an increased price. Table III. also shows considerable variation, some of the conditions which tend to these results being mentioned further on. The only relief for such a condition of things is, as I have said, to adopt the American conditions of voltage grading. If this is not possible, then a narrow specification similar to the American one will entail a higher price than is paid at present. We cannot get rid of lamps unless we have an approximately even grading of all the different voltages. From a lamp-maker's point of view there ought to be as many voltages of one kind as there are of another, but at present they are so terribly uneven. There are large numbers of lamps being made of one particular voltage, and the outfalls on either one side or the other are of no use whatsoever, there being no voltage near them. As to life testing at efficiencies different from the actual efficiency to which the lamp is being made, the Robertson Lamp Company have carried out a series of tests on this question, and have tried to cut down the period of running so that lamps are overrun by a certain per cent., but our results with various makes of lamp are the same as those mentioned by the author—there is no reliability in an overrunning test in comparison with normal running tests, and individual lamps give varying results which are most peculiar. I agree with the author that, to compare the quality of different lamps, they must be run at the same efficiency in watts per M.S.C.P., or if the factor from the mean horizontal is known, then in watts per M.H.C.P. The objections to comparative tests at fixed voltage or fixed candle power are fully enumerated in the paper, but the purchaser will expect to get lamps suitable for the nominal voltage of his current, and will naturally want to know their life behaviour at that nominal voltage. With time and greater attention to voltage regulation it may be possible to restrict even further the limitations of this specification. Mr. Robertson.

I think that the ultimate possibilities of carbon filaments have by no means been exhausted, and that there is a great field for the improvement of the carbon lamp in the future, for one has only to remember that the volatilising point of carbon is twice as high as that of the newer metals being tried, and also that its specific resistance is much higher, and is, to a certain extent, controllable between the non-conductive lampblack stage and the highly conductive graphitic stage, and thence onward to a second non-conductive stage as we approach nearer the state of the diamond.

Mr. A. RUSSELL: I was particularly interested in the tests on the Carcel lamp described in the early part of the paper. Considering that the Carcel lamp was invented in 1800, that is, eight years before Murdoch's tallow candle, I was surprised to find what an admirable unit it is. It is satisfactory to note that the intensity of the Mr. Russell.

r. Russell. light emitted horizontally by this lamp is practically identical with that given out by the modern 10-c.p. Harcourt pentane standard. It makes it easy to interpret the results obtained by Fresnel, Dumas, Regnault, and other French physicists. It is interesting for us to remember that Fresnel was also an engineer. The author points out the importance of measuring the M.S.C.P. of glow lamps. It has now been admitted for many years that the method of measuring the efficiency of glow lamps in terms of the M.H.C.P. per watt is unscientific and inequitable. What we want is the M.S.C.P. per watt. This can be easily done by an integrating photometer or a lumenmeter. If no appliance of this nature is available, then the lamp has to be rotated and about ten measurements* taken at given angles. This, however, takes time. Dr. Fleming suggested in a paper † read to the Physical Society about two years ago that it would be possible to calculate the ratio of the M.S.C.P. to the M.H.C.P. with sufficient accuracy for practical purposes, and so the M.S.C.P. could be found from the M.H.C.P. at once by multiplying by this ratio, which is called the reduction factor. An inspection of the author's diagrams of candle power distribution curves (Fig. 20) has suggested to me a method of computing the reduction factor of carbon glow lamps from two measurements only, namely, the M.H.C.P. (I_H) and the vertical c.p. (I_V).

Let us suppose that S is the source of light and that a vertical section of the c.p. surface consists of the semicircle S L A, and the arc of a circle A E B and the symmetrical curves on the other side of the vertical. The equation to A E B is—

$$R = r \cos\theta + a \sin\theta,$$

where S A $= r$, S B $= a$, and θ is the angle which the radius vector

[redacted]

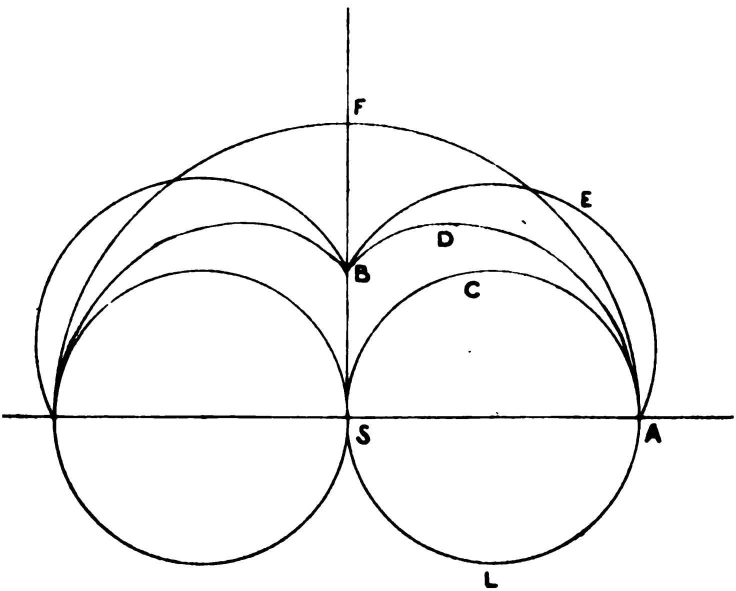
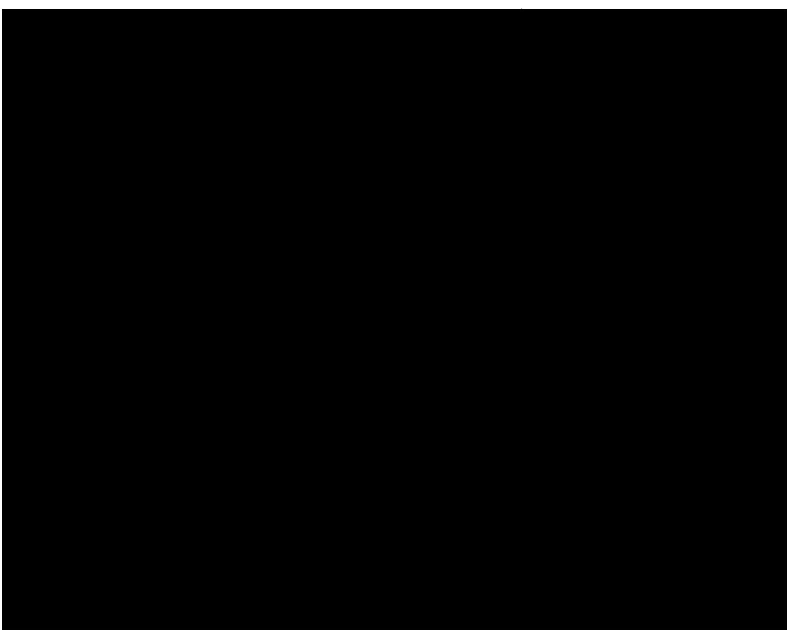


DIAGRAM.



that is— Mr. Russell.

$$0{\cdot}785 + 0{\cdot}125\,(I_V/I_H) \text{ approximately.}$$

It is to be noticed that in the above proof no assumption has been made as to the distribution of the flux of light in the horizontal plane. The hypothetical c.p. surface has the same equatorial curve and the same poles as the actual c.p. surface. Now there is obviously an infinite number of surfaces that will satisfy these conditions, but if we make the limitation that they are not to be very different from the curves shown by Mr. Paterson in Fig. 20 we may use the approximate formulæ we have obtained, as an appreciable variation in the shape of the surfaces does not much alter the reduction factor. For instance the equation to the curve B D A is—

$$R = r\cos\theta + a\,(1 - \cos\theta),$$

and hence by the method given above we find that the reduction factor equals—

$$0{\cdot}785 + (1/2 - \pi/8)(I_V/I_H),$$

that is—

$$0{\cdot}785 + 0{\cdot}107\,(I_V/I_H).$$

The large alteration in the shape of the upper c.p. surface has thus diminished the reduction factor by only about 1 per cent. If the upper part of the vertical c.p. curve had been the circle A F the reduction factor would have been $(1 + \pi/4)/2$, that is, 0·89 nearly. If it had been the two semicircles of which one is A C S it would have been 0·79. In practical work, therefore, we may use the formula—

$$0{\cdot}785 + 0{\cdot}11\,(I_V/I_H),$$

with considerable confidence that the maximum inaccuracy of the results is only about 1 or 2 per cent.

I should like to point out that appreciable errors sometimes arise in practice owing to the assumption that the law of inverse squares, obtained on the assumption of a spherical source of light, can be used when carbon filaments of appreciable length *enclosed in glass bulbs* are being compared. I have previously shown that the horizontal intensity of the illumination at a given distance round a glow lamp varies in an extraordinary manner, owing mainly to the reflections of the light from the bulb. The shapes of the curves of horizontal c.p. obtained at different distances are different, and so in certain cases it is not safe to assume that the law according to which the intensity of the illumination varies is that of the inverse square.

Mr. Lancelot W. Wild: With regard to the pentane lamp, the author has determined the effect of humidity, the barometric pressure, and CO_2, and he has now promised an inquiry into the effect of a variation in the amount of oxygen. I would also suggest one other subject which certainly requires investigation, and that is the effect of the dimensions of the photometer room, more especially the height, as I Mr. Wild.

Mr. Wild. am rather inclined to think that the height has a considerable effect upon the chimney draught. In comparing some tests which I carried out with considerable care, and which were checked in the National Physical Laboratory, I found that a particular pentane lamp when tested in the Westminster Electrical Testing Laboratory, all allowance being made for humidity and barometric pressure, gave 4½ per cent. less candle power than it did at Teddington. I put this down to the fact that my photometer room was less lofty, or else that there was less oxygen in the air at Westminster than in Bushy Park. With regard to the question of incandescent lamps for use as standards, I see that the author has obtained very good results with Dr. Fleming's low-voltage incandescent lamp, but when he tried high-voltage lamps of various makers the results were not so satisfactory. The demand at present is for high-voltage lamps, and this demand will have to be satisfied. As I have supplied various lamp-makers and central-station engineers with a considerable number of high-voltage standard lamps, I thought it was incumbent upon me to make a test of them. So I took six of these high-voltage lamps of various voltages up to 250; I took them at random out of stock after they had been aged; they were all of about 16 candle power, and about 4½ watts per candle. I ran those lamps at a steady voltage for 100 hours, and tested them at intervals of about 16 hours. The greatest variation found in those six lamps from their original candle power was 0·1 of a candle power, or about 0·6 per cent., and four of the lamps did not vary more than about half this amount. With regard to the question of the rating of incandescent lamps, I would rate the lamps by their maximum horizontal candle power rather than by their mean horizontal candle power. The reduction factor for obtaining the mean spherical candle power from the mean horizontal varies with different shapes of filament from about 0·8 to 0·9, whereas in all the lamps I have tested I have found this reduction factor of the maximum horizontal candle power to vary only from about 0·805 to 0·825. That is obtained on about ninety lamps of many different makes taken at random. I should like to ask this question: Why should we not, when we are testing samples only and not testing every lamp, test the mean spherical candle power straight away? If we rotate the lamp with an electric motor, and have an arrangement for tilting it at the same time, we can get the mean spherical candle power in five or six measurements, and this is the only real measure of the total light of the lamp. Turning now to the question of life tests, I fail to see the utility of measuring the useful life of a lamp and saying it is no use after it has gone down 80 per cent. of its original candle power. Taking the average life curve, I find that 1 per cent. of error in the last measurement would produce about 10 per cent. of error in the apparent useful life of the lamp, and I think this is rather large. I do not think there are very many photometrists who can measure ordinary commercial lamps and guarantee their measurements nearer than 1 per cent. every time. I would much rather carry out the life test for a

standard time, say 500 hours, and measure the candle-power watts, etc., at regular intervals and take the average throughout the run. With regard to this question of starting the test at the standard efficiency instead of the marked voltage, if the test is carried out in the way I suggest, the lamp which starts at a little over the standard efficiency comes out very much the same as a lamp which starts a little under the standard efficiency, provided one keeps within reasonable limits. It seems hardly necessary, therefore, to carry out the tests on the standard efficiency instead of the marked voltage. In the standard specification, twenty lamps are picked out for the rating test, and five of these for life test, so that it ought to be a fairly easy matter to get out of those twenty lamps five that are not very far from standard efficiency. There is another point as regards these life tests, namely, that the reduction factor varying from 0·8 to 0·9, if we measure the mean horizontal candle power and rate the lamp by that, it is not fair to the lamp which has a reduction factor of 0·9 to be tested on the same basis as that which has a reduction factor of 0·8. As a matter of fact, when one comes to work this thing out one finds the lamp with the filament wound to give a reduction factor of 0·9 will have but 30 per cent. of the useful life of the other. That is to say, if we take two lamps of exactly the same quality of filament but bent into a different shape, the one having a reduction factor of 0·8 and the other of 0·9, and test the mean spherical candle power, the useful life comes out exactly the same for both lamps; but if the mean horizontal candle power be tested, it will be found that the one has three times the life of the other, owing to the difference in their true efficiencies. This is worked out from the curve in Fig. 17 of the paper. With regard to the calculation of mean spherical candle power, the author has referred to three methods of integrating the candle-power curve—the Rousseau method, that of Mr. Russell, and one which he credits to me, though I was not the originator of it. I have used all three methods repeatedly, and if the mean spherical candle power only is required, I should not have much hesitation in preferring the Russell method. But it frequently happens that we not only want to know that, but we want to know something about the distribution of the light, and then it is much more convenient to make the tests at regular stated intervals, such as every 15°. Mr. Paterson states that this greatly increases the amount of work to be done in carrying out the test. This is not really the case. The increase is not nearly so great as one would at first suppose. Of course, if one takes twenty measurements round the circumference of the lamp to get the mean horizontal candle power, when one comes to 60° below the horizontal only half the accuracy is required, and we need only take ten measurements, and when one comes to 90° from the horizontal we do not need any measurements at all; one generally takes one measurement, however, for form's sake. Mr. Wild.

Mr. HAYDN T. HARRISON: I think we ought to be very grateful for this paper, not only because it is exceedingly interesting, but because it is another step in the direction of doing away with flame standards Mr. Harrison.

Mr. Harrison.

for general photometric work. I have had a good deal to do with flame standards, and I do not want to have much more to do with them. In this paper the author deals at length with the troubles of working with these standards, by which one is led to only one conclusion, namely, that they can only be used by an exceptionally experienced person who has everything at his fingers' ends which is necessary to keep them in the right condition. In fact, it comes to this, that one can hardly breathe in the room, or the flame will vary in some way or the other. I think Mr. Swinburne's paper last week gave us a little more hope, because the fact that a metal can be used for the filament of a lamp will probably result in a standard of light which can be defined and worked to under ordinary conditions. For instance, with regard to tantalum, we know now that German chemists have been able to prove that it is a ductile metal; that is to say, it is possible to draw a wire of a given length, a given section, and a given resistance, which, when heated in a vacuum by a given current, will give definite candle power. Therefore the probability is that in a short time we shall be able to define a standard of light which is produced by passing a given current through a specific metal of a given length and sectional area. If that is so, not only will it save a lot of trouble to the national laboratories who have to do all this work, which is very trying, but it will make it more international in every way. The next part of the paper which interested me very much was Mr. Paterson's reference to tests by means of overrunning lamps, commonly called the Preece test; that is to say, when it has to be decided as to which type of lamp in a large batch is the best, and there is no time to go through the ordinary life tests, by overrunning them for an hour at 40 per cent. higher pressure one gets a criterion of what those lamps are likely to do. It is very interesting to note in Table VII. that, whether these lamps had been run through their long-life test taking three weeks, or only a test of one hour, the same conclusion would have been arrived at, namely, that lamp No. 2 was the best lamp. This shows that the test, though not scientifically accurate, has a commercial value.

DISCUSSION AT MEETING OF FEBRUARY 7, 1907.

Professor Ayrton.

Professor W. E. AYRTON: Mr. Paterson has given us a very interesting paper from many points of view. It contains a great number of apparently carefully conducted experiments, and the conclusions are of importance to many of us. One might, however, feel inclined to suggest that a little more time might have been given by the author to a consideration of what his predecessors had done on the same subject, and also that the reflection that he has been able to bestow on the results does not equal the exertion and energy that he has devoted to carrying out the tests. For example, like many other users of glow lamps, I must take exception to a statement which occurs at the beginning of the part headed "Life Test"

Professor Ayrton.

on page 294: "Or, in other words, the number of useful candle-hours obtained from a lamp is the chief point about which information is required when buying lamps." It strikes me that is rather like saying that, in buying ordinary candles, the time that they take to burn down to the candlestick is the most important thing to the purchaser, and that the price of paraffin wax, and whether there are eight or twelve candles to the lb., is of no importance. What it is necessary to know in estimating the relative value of types of glow lamps is the useful life, which you find in the usual way, plus the amount of energy that the lamp consumes during that period. It is quite conceivable that it might be worth the consumer's while to change a lamp every night, and indeed generally with glow lamps the first cost of the lamps and that of carbon lamp renewals is quite unimportant compared with the cost of the energy. Energy is the important thing to consider. Even taking a 16-c.p. lamp under present conditions, say, costing 1s., if the useful life could be enlarged from 500 hours to 1,000 hours there would obviously be a saving of 6d.; but if by so doing the power were increased by ½ watt per candle—that is, 8 watts for the lamp—then at 3d. a unit 2s. more would be spent; 6d. would be saved and 2s. wasted. It is an obvious truism that one cannot neglect the watts per candle taken during the useful life; and the average watts cannot be got by merely taking what the author has called the extreme values which he gives in Table III. We must, as we all know, have the efficiency curves as well as the candle-power curves. While on the subject of candles, I notice that at the last meeting Dr. Fleming expressed astonishment that the comparison of the amyl-acetate standard with the pentane standard appeared to differ to-day from what it gave a few years ago. That was a subject to which a good deal of attention was devoted in the discussion on Dr. Fleming's paper four years ago. I find I have some tests which I made myself on the comparison between the amyl-acetate standard and the pentane standard twelve years ago—1895. In this case, however, it was a 1-candle pentane standard; and then the ratio of an amyl-acetate was 0·98 of a Harcourt 1 candle as given by the 1-candle pentane standard. I did not know at that time what I pointed out in the discussion to which I have just referred, that different Harcourt 1-candle pentane standards differed so much among one another. I had three standards, all supposed to give 1 candle, and I compared them, and found that you could get as much as 12·5 per cent. difference between them. The detailed results are described on page 202 of volume 32 of the *Journal* of the Institution. One would like to know in this comparison which is given by Mr. Paterson on page 285 of his paper, showing the difference between the 10-candle pentane standard and the amyl-acetate standard, how the pentane standards at the various places mentioned, the National Physical Laboratory, the Reichsanstalt, the Laboratoire Centrale, and the Laboratoire d'Essais, had been compared with one another. No doubt either our President or Mr. Paterson will be able to tell us that. Has a pentane standard been

Professor Ayrton.

sent from one place to the other to enable a comparison to be made, or have the standard lamps been constructed in the various countries according to certain dimensional drawings which have been agreed on? In the case of the 1-candle standard, many dimensions are given in Professor Harcourt's patent specification, but I found in the case I have referred to that the dimensions of the three lamps did not agree, and that no one of the three 1-candle standards in question had the dimensions detailed in the patent specification itself. Another point to consider is, How is the quality of the pentane defined? We know that every careful motorist takes a pocket hydrometer about with him to determine the specific gravity of his petrol. Does a careful photometerist, or whatever he may be called, have a pocket hydrometer to determine the specific gravity of his pentane? Also, what is the standard specific gravity? I presume that has been carefully defined. In 1895 the Board of Trade appointed a Committee to deal with the question of standards of light, and that Committee came to this conclusion: "We therefore recommend that a pentane air flame, furnished with a Dibdin argand burner having the form and dimensions set forth in the appendix, section 9, and used in the manner there defined, be accepted as giving the light of 10 standard candles, and that this flame be authorised and prescribed for official use in testing the illuminating power of the gas supplied by the London Gas Companies." I thought it would be interesting to-day to compare with the Harcourt pentane standard the Dibdin standard, of which I have one, and which was kindly tested by Mr. Dibdin himself, against the actual Dibdin standard examined by the Board of Trade, as mentioned in the discussion of Dr. Fleming's paper in 1903. Mr. Mather has just given me the result of the comparison between this Dibdin standard, which is a 10-candle standard, and the Harcourt 10-candle standard, and there is a difference of 4 per cent. Taking the Harcourt as 10, the Dibdin is 10·4. There is a difference of 4 per cent. at once between what the Board of Trade recommended and what is now stated as being the official standard, namely, the one which is in the custody of the National Physical Laboratory. If those sort of differences occur, it is quite easy to see that tests made at various times and in various countries of comparing the amyl-acetate, or Hefner, standard with the Harcourt 10-candle standard will give different results.

Perhaps the most striking things in the paper are the curves shown on Fig. 16, giving the average of ten makes of 200-volt lamps. We see that only two of the makers whose lamps are given on the sheet have succeeded in turning out lamps which have a useful life of as much as 500 hours. That is rather the sort of experience that several of us have with the present 200-volt lamp. It is probably within the memory of many present that, in March, 1901, the Board of Trade held an inquiry to ascertain the conditions under which the electric supply companies in London should be allowed to change over from 100 volts as their declared pressure to 200 volts. That inquiry was held quite near here. Several of us had tested 200-volt lamps in 1900-1

Professor Ayrton.

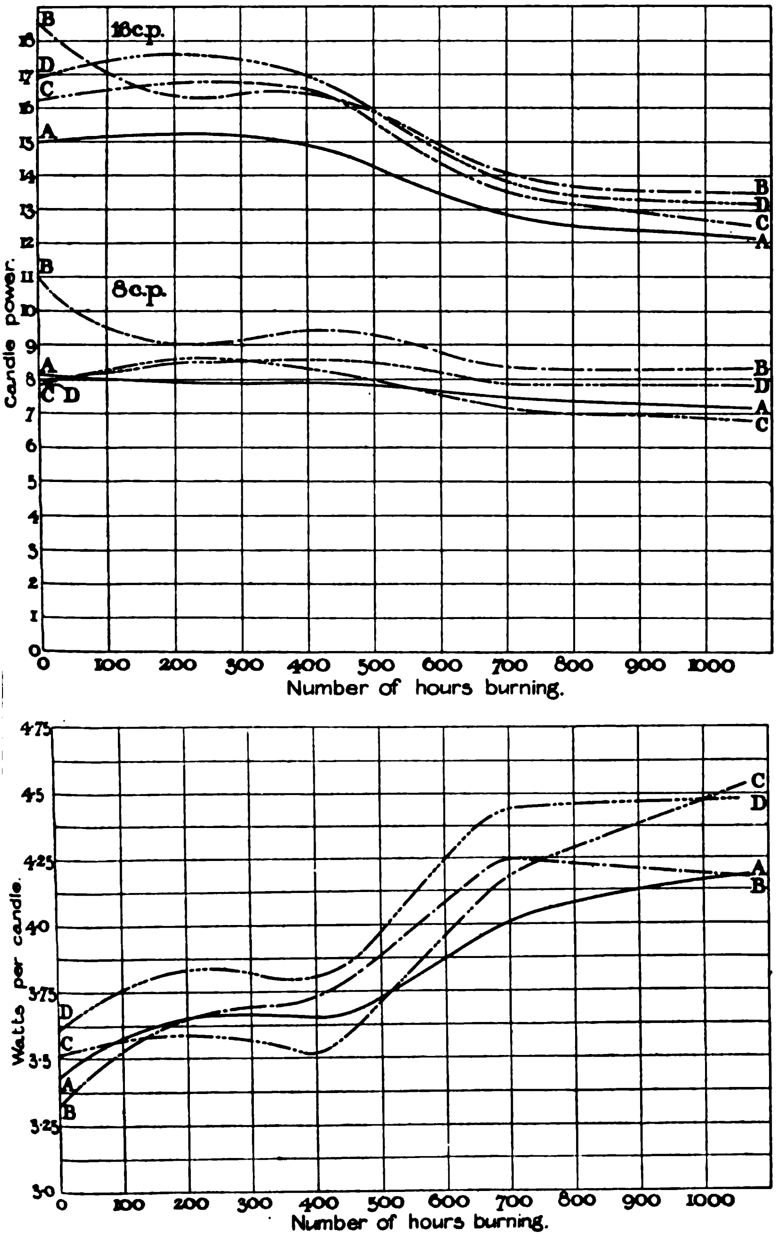


FIG. A.—Curves exhibited at the Board of Trade Inquiry, March, 1901, showing results of four tests of types of 200-volt lamps.

Professor Ayrton.

(I amongst others), and had found them very inferior to the then 100-volt lamps. Sir Alexander Kennedy, on the contrary, representing the Westminster Company, showed the two sets of curves in Fig. A., giving results of tests on four makes of 200-volt lamps.

These curves for 200-volt lamps make your mouth water even to-day, after there have been six more years to improve the manufacture of 200-volt lamps. Look at the straightness of the candle-power curves! For 1,100 hours there is hardly any falling off in candle power, especially in the case of the 8-c.p. lamps. They not only all give over 80 per cent. of the initial candle power at the end of 300 hours, but the light hardly falls at all even in 1,100 hours.

Taking the watts per candle, they are seen to rise from $3\frac{1}{4}$, taking the A and B curves for example, to about 4 or 4·8 watts per candle. These tests bring us to the most painful conclusion that the 200-volt lamps, in spite of all the standardisation, have gone down in quality. Of course I realised then that during the next few years—that is, in the interim between 1901 and 1907—it was quite possible that the 200-volt lamp would become as good as the 100-volt lamp then was. But, on the other hand, it seemed to me quite possible that it would not—that it would stay the sort of defective instrument that Mr. Paterson's tests show it is to-day. So I laid great stress on my experience up to that time in giving my evidence before the Board of Trade Committee, and I urged very strongly on the London County Council, the body that I was then advising, not to consent to any change being allowed which did not take into account the possible continued inferiority of the 200-volt lamp compared with the 100 volt, and the loss to the householder that the use of the 200 volt might bring on him, quite apart from the cost of putting in new switches, new ceiling roses, and new fuses. In consultation with counsel I drafted a clause which all the electric supply companies that were represented at the conference agreed to, and which was embodied in the Board of Trade new Electric Light Regulations of that date, the most salient part of which is the following :—

Electric Lighting Regulations.

The Board of Trade, after consideration of the representations made at the recent inquiry held at the Westminster Town Hall with respect to the regulations made by them under the Electric Lighting Acts for insuring a proper and sufficient supply of electrical energy, have decided to make further regulations in the following form, amending those previously made :—

"But where the consumer withholds his consent after the undertakers have offered to comply with the general terms and conditions imposed by the Board of Trade (County Council, local authority), and if not required to do so under those terms and conditions, also to pay the reasonable cost of or incidental to the change (including compensation for any loss or damage incurred in consequence of the change), the undertakers may appeal to the Board of Trade, and that Board

may, if they think fit, give their consent to the change on such terms and conditions as they impose, and the consent of the Board so given shall for the purpose of this regulation have the same effect as the consent of the consumer." Professor Ayrton.

Made by the Board of Trade this day of , .

Assistant Secretary, Board of Trade.

Take the second paragraph only: "Where the consumer withholds his consent after the undertakers have offered to comply with the general terms and conditions imposed by the Board of Trade, and if not required to do so under those terms and conditions, also to pay the reasonable cost of or incidental to the change." I laid great stress on putting in the words "or incidental to the change, including compensation for any loss or damage incurred in consequence of the change." That was published in a number of newspapers at the time, and it became a Board of Trade regulation; but the public did not realise what vested interests that gave them, and they allowed it to glide away, Esau-like accepting a mess of pottage for their electric birthright. Remember, "incidental to the change" means that if your 200-volt glow lamp takes more energy per candle-hour than a 100-volt, then the electric light company which has required you to change over to the 200 volts pressure has to reimburse you for your loss. That was made quite clear at the time; I am not reading it in now, because it clearly says, "Also to pay the reasonable cost of or incidental to the change, including compensation for any loss or damage incurred in consequence of the change."

In the following year I moved into a house which had not been previously electrically lighted. Luckily it was in a neighbourhood where the Metropolitan Electric Supply Company was supplying current on the 3-wire system, with 200 volts between the outers and 100 between either outer and the middle. They asked me which I would have, 100 or 200, and I replied "Both." So they gave me both. Hence it is no disadvantage to me to-day that I can only easily get metallic filament lamps taking 200 volts, and I can get all the advantage that is obtainable from the 100-volt lamp still remaining a better article than the 200-volt lamp. A point which was raised in the discussion on Mr. Swinburne's paper was improvements in the filaments of lamps other than metallic filaments, and he advocated the use of carbide of silicon. Dr. Harker, I understand, showed an experiment tending to prove that carbide of silicon would fuse too easily to make a successful filament for a glow lamp, because the silica was turned into vapour in an arc. But, as a matter of fact, carbide of silicon lamps have been made in large numbers, and with extremely good results; in fact, Dr. Harker's argument appears to me to be fallacious. We have carbide of silicon, commercially called carborundum—it is quite a common thing—made at the Niagara Falls Carborundum Works, with the arcs there each of 1,000 H.P., and because Dr. Harker's specimen would fuse in an arc, that is no reason why one should not make

Professor Ayrton.

a filament out of carborundum to glow at a much lower temperature than that of the electric arc.

I have placed on the accompanying diagram (Fig. B.) some results which I obtained in 1896, eleven years ago. The dotted line represents the candle power and the continuous line the watts per candle. If you take the second one you will see that the watts remain under 3 the whole time, and that the candle power remains practically constant. I had a number of those lamps sent to me from Germany to test, and being rather sceptical that picked lamps were being submitted, I made a condition that no report, not even for private circulation, should be printed until the following conditions were carried out, namely, that a private assistant in whom I had absolute confidence should spend some weeks in Berlin studying the method of manufacture under Mr. Langhans' supervision, bring back to London *all* the filaments that

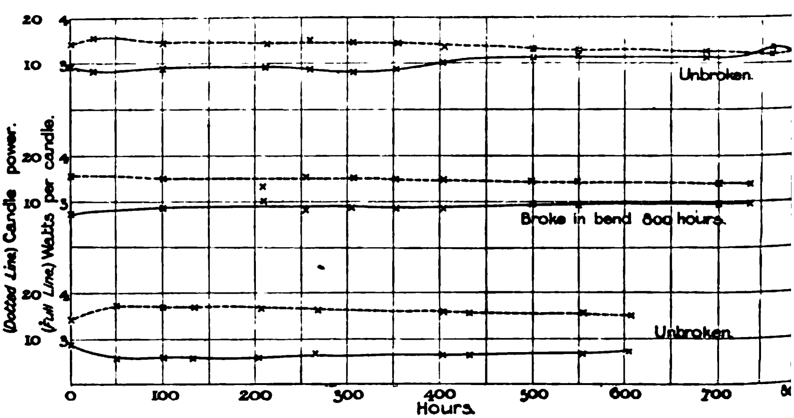


FIG. B.—Tests of Lamps containing Silicon Carbide Filaments, 1896.

he had constructed and flashed, mount them, and have the bulbs blown round them and exhausted there under my supervision, and that no lamp should be discarded from the tests—I mean that we should not merely record the results of the best of the lamps, but of the whole of them. Of course I have not drawn all the curves on the diagram hung up on the wall, but the curves give an idea of the sort of results that we obtained. They were tested on the mains of a certain direct-current supply company in London, and the pressure varied at times, as shown by the recording voltmeter, by as much as 10 per cent. above the declared value, and therefore 10 per cent. above what the filaments had been made for. A company was formed, called the Premier Lamp Company, with a factory near Liverpool. That this was really a compound of carbon and silicon was proved by Dr. Moody, the chief assistant of the Chemical Department of our College, who analysed the broken filaments at the end of the trial, and found that the

composition was as stated. Later on, in 1900, Dr. Moody himself brought me some other carbide of silicon filament lamps, and they were even better. Some of them gave a candle for 1·13 watts, which is as good as has been obtained with any metallic filament lamp. What is the objection to those lamps? I have been going into that question very fully during the last few days, and as far as I can find out the main objection is that there is a certain amount of difficulty in turning them out in large numbers of exactly the same quality. Filaments as good as are recorded on that diagram can be obtained, but one then gets a batch of filaments which are not nearly as good. In addition to that, in spite of what I said about the overrunning by 10 per cent., they found in Liverpool that the filaments were not so good as ordinary filaments in connection with overrunning, so that they have not made the success that was expected. Experiments, however, I am told, are still being carried on with this method of making filaments, and therefore I hope that Mr. Swinburne's expectations will be realised, and that we shall later on have carbide of silicon filaments which will be quite as good, even better perhaps, than metallic filament lamps. Professor Ayrton.

The PRESIDENT: Before I call on the next speaker perhaps I may answer one question which Professor Ayrton has put. The fundamental standard to which pentane lamps are referred is the standard of the Gas Referees. The lamp used at the National Physical Laboratory has been repeatedly compared with that standard. At the Reichsanstalt they have two lamps, one of which was compared with the standard of the Gas Referees, and the other agrees with it very closely. I understand also that one at least of the French lamps was compared with the same standard, though of this I am not quite certain. The President.

Mr. A. P. TROTTER: The old "Parliamentary candle" was an official standard, and if it was a legal one, it was in a very limited sense, for it was only authorised for testing London gas. Similarly, the Carcel lamp produced in 1800 was adopted by Dumas and Regnault for gas testing; its value was about 9½ c.p. Mr. Trotter.

The German Association of Gas and Water Industries adopted a paraffin candle, the *Vereinskerze*, in 1868. Other standards are mentioned in Dr. Fleming's paper, and detailed information about them is to be found in Palaz's well-known book. By way of using a lump of chalk to measure chalk, Methven, in 1878, introduced his excellent system of screening the flame of a Sugg's argand gas-burner and a carburetter, and the combination was called a "standard." In the previous year Vernon Harcourt described his pentane burner before the British Association, and again in 1883 and 1885. At this time Woodhouse and Rawson made a 1-candle and a 2-candle pattern. Finally a 10-candle pattern became the official standard for London gas testing, and replaced the Parliamentary candle.

The subject of a standard of light was discussed at the International Congress at Paris at 1881, and several French *savants* upheld the virtues of the Carcel lamp. At this Congress, the Violle standard,

Mr. Trotter. consisting of one square centimetre of platinum at the temperature of solidification, was proposed, and at the International Commission at Paris in 1883 it was virtually adopted, although the practical difficulties were so great that it never came into use. Draper had suggested the possibility of using an incandescent platinum wire in 1844. and Zoellner, Schwendler, Werner von Siemens, Liebenthal, and Petavel spent fruitless labour on the problem. Physical researches ending in negative results are well known, but I cannot recall any investigation on which so much time, expense, and good work has been spent with so little result. Not even a by-product of a new hypothesis or an improved photometer.

The Hefner lamp was introduced in 1884, and was soon recognised as an apparatus of great value. It is not only an official standard, but the legal standard in Germany.

In 1896 an unofficial International Congress at Geneva settled various photometric units or measures, and symbols for them. These are clearly explained in Carl Hering's valuable Ready Reference Tables. The Violle standard was never determined accurately, but it was about 20 c.p. The Geneva Congress adopted the *bougie decimal* as one-twentieth of the Violle, and thus the unit which the old Parliamentary candle tried to represent reappeared. The second International Electrical Congress in Paris in 1889 adopted the *bougie decimal*, and defined it as one-twentieth of the Violle.

At the Chicago Congress of 1893 a unit of light was discussed, and the Hefner unit was strongly recommended. It was not definitely adopted, but it was accepted tentatively by the American Bureau of Standards and by the American Institute of Electrical Engineers.

In the discussion upon a paper which I read before the Institution of Electrical Engineers in 1892, Mr. Swinburne suggested * that a screen with a pin-hole of known dimensions should be placed in front of an arc, and he thought that a standard quantity of light would pass through the hole. Professor S. P. Thompson communicated in writing to the meeting the same suggestion,† and proposed that the unit of light should be the light emitted by a square millimetre of crater. Professor S. P. Thompson read a paper at the Chicago Congress on "The Swinburne-Thompson Unit of Light," and it is probable that the possibility of realising this unit in the near future prevented the adoption of the Hefner standard by the Chicago Congress.

Illumination photometry had been a hobby of mine for some years, and I undertook to investigate the Swinburne-Thompson unit, and began experiments on ordinary photometry. I was soon led away from the original subject by a phenomenon on which I read a paper before the Royal Society in June, 1894. I found that, when an arc is apparently steady and quiet, there is generally, if not always, a rotation of a bright patch. The experiments were extremely interesting,‡ but had the result of knocking this unit of light on the head.

* *Journal Institution of Electrical Engineers*, vol. 21, 1892, p. 384. † *Ibid.*
‡ Professor S. P. Thompson, *Journal of the Society of Arts*, vol. 43, 1895, pp. 973–5.

In November, 1893, a special committee of the American Institute of Electrical Engineers was appointed to take up the determination of a practical unit of illumination,* but they apparently did nothing more than provisionally adopt the Hefner lamp. Mr. Trotter.

Mr. Paterson shows that corrections for humidity, for proportion of oxygen to nitrogen, and for barometric pressure are less for the Hefner lamp than for the pentane. He gives only two advantages for the pentane lamp, viz., that the colour is whiter and that the candle power is larger. He alludes also to the greater ease of adjustment. It is quite possible that if the screened or Methvenised arc unit of light had been realised, people would be found to complain that it was too white.

A Clark cell is not much good for electric bells or Wheatstone bridges. A fitter uses a steel rule, a carpenter a wooden one, a consulting engineer an ivory one, a tailor a tape; a draughtsman measures paper drawings with a printed paper scale, and pretends that he does so because it expands and contracts with the drawing. Nobody but Major MacMahon at the Standards Office uses a platinum-iridium bar with microscopic lines engraved on sunk gold plugs. It should be remembered that the ultimate legal standard is not necessarily a useful, every-day, practical man's instrument.

A flame lamp, particularly one which uses vapour as fuel, is excellent for testing gas. All the corrections (I understand) cancel out, and may be neglected. As a secondary standard, for electrical purposes, nothing has been proposed which is better than Dr. Fleming's glow lamps. It is necessary that these should be compared in a suitable laboratory with a primary standard. In such a laboratory the trifling advantages of a pentane lamp do not appear to warrant the setting up in this country of a standard which differs so materially from the legal standard of Germany and the recognised standard of the United States, and one which is so widely used among scientific men of all countries.

The Hefner lamp is also well adapted for ordinary commercial laboratory use. Like a Clark cell, it must be treated with some regard for its peculiarities. It is inexpensive and small, and compares very favourably with the Fleming lamps, which need a potentiometer and a large set of accumulators, and other expensive apparatus.

My own work has been confined to experimenting with illumination photometers, including in that an investigation of Lambert's law. I needed a good standard for one evening only, and rarely applied barometrical and wet and dry bulb thermometer corrections. For out-door illumination work a precision greater than 2 per cent. would have been wasted, and the screened-arc "unit" of light varied at least 10 per cent. I do not claim to have carried out any photometric work to fractions of 1 per cent., but I strongly advocate that if a standard of light is to be adopted, it should be the Hefner lamp, and that the pentane lamp should be left to its useful province in the skilled hands of the Metropolitan Gas Referees.

* *Electrician*, vol. 32, 1893, p. 133.

Mr. Cooper.

Mr. W. R. COOPER: There are a few points that I should like to raise on this very interesting paper. The author has given a table showing the effect of carbon dioxide upon the pentane standard, and in obtaining his results he has assumed that any variation of oxygen in the atmosphere may be neglected. This, however, seems doubtful if we take Liebenthal's result, showing that a change of 1 part of carbon dioxide in 1,000 parts of air makes a difference of 0·7 per cent. in the light, and his calculation that an equal change of oxygen of 1 part in 1,000 causes a change of 0·4 per cent. in the light. The effect of a change in the oxygen is of the same order of magnitude as the effect of a change in the carbon dioxide. Consequently it would seem that a change in the oxygen cannot be neglected. It may, of course, happen that the two changes are generally so far dependent upon each other that the combined effect may be expressed as due to a change in the carbon dioxide alone. But whether this is so or not is not clear from the paper. I should also like to ask how the change in carbon dioxide was produced, and how a fair sample of the effective part of the atmosphere was ensured. The results in Table I. do not look consistent, for although the effect with time is large to begin with, it becomes much smaller later on. I presume that this is due to diffusion. I imagine that the effect of quality of atmosphere can be divided into two parts in the case of the pentane standard, namely (1) the effect of the air which mixes with the pentane to supply the burning mixture, and (2) the effect of the air on the flame as the atmosphere in which it burns. I should like to know whether these effects have been separated. As regards the first, there ought to be no great difficulty in supplying air of constant quality to the pentane. The method of ventilation as a whole which the author has adopted seems primitive.

With regard to life tests, the accuracy arrived at seems rather high as compared with working conditions when it is remembered that, according to the author's statement, 0·2 per cent. increase in the pressure makes a difference of 1 per cent. in the life. Owing to the variation of pressure allowed by the Board of Trade, the efficiency at which lamps may be run on ordinary networks may vary by, say, 15 per cent. As to the method of test, this involves running the lamp at a nominal efficiency. It is therefore a test of the filament rather than of the lamp from the consumer's point of view. Although a test at the nominal pressure instead of at the nominal efficiency introduces difficulties, I think it is a pity that a method of testing at the nominal pressure cannot be introduced, so as to have the tests rather more under the conditions of actual use.

Mr. Le Maistre.

Mr. C. LE MAISTRE: I think Mr. Paterson's paper might well serve as a model on which the results of lamp tests should be presented.

Fig. 13 shows the utter inefficiency of the high-voltage lamp. On the other hand, I venture to think, the paper generally shows the efficiency of the lamp-testing department of the National Physical Laboratory, which augurs well for the industry in view of the position which we all hope that department will shortly hold. Users and

makers are agreed that it is a very essential matter for the whole industry that there should be only one Standard, and not numerous standards all over the kingdom. Professor Ayrton mentioned that in Fig. 13 there were only two batches of lamps which had a useful life of about 500 hours. Three, however, appear to have a useful life of about that figure. It is the opinion of a great many that the users generally will not insist on tests being carried out. I venture to suggest, therefore, that if we could have periodical target diagrams and life tests published in the Press, it would be of the very greatest assistance. In that way one would see what the lamp maker was doing, and it would be an incentive to the lamp maker himself to improve his lamps, because no doubt he could obtain information privately as to the performance of his own particular lamp. One speaker referred to the grading of voltages as if that were the panacea for all the ills of the makers. He compared American with English practice, rather, it would appear, in the hope that English practice might be brought into conformity with American practice. But as Americans sell *light*, and we only sell *current*, it would seem that, until the laws are altered, that is a practical impossibility. Pressure regulation, on the other hand, may do a great deal to help the maker as well as the user, and more attention should be directed to this subject. In a paper read by Mr. Alex Dow at the International Electrical Congress held at St. Louis in 1904, the author mentioned that the pressure regulation was generally maintained within plus or minus 2 per cent. I should like to direct the attention of all station engineers to that paper, especially paragraph 34, which appears on page 622 of the second volume of the *Transactions* of the Congress. We have had lately some interesting papers from users of lamps; cannot we persuade some of our friends the lamp makers to come forward with a paper from their side of the question? They know the ins and outs far better than the user does, and they themselves tell us that there are very few secrets now from the lamp manufacturer's point of view. A paper from such a source would make a valuable addition to the *Proceedings* of the Institution. Mr. Le Maistre.

Mr. C. Wilson: I am afraid my experience may not perhaps tend in the way Mr. Le Maistre has just desired that it should. There is just one thing I should like to say before we pass away from the question of the flame standards. We, as practical lamp manufacturers, are perfectly content to leave the professors of the laboratories to fight among themselves about the flame standards. We, as manufacturers, have to come down to the hard commercial conditions which exist to-day. For that purpose I will proceed straight away to pages 291 and 292 of the paper, where the questions of commercial testing and initial rating are referred to. I think most of the controversy on the paper should rage round these points. We continually hear it said that lamp makers should supervise their manufactures more closely, and grade their lamps better. It seems strange to me that it has never struck the people who are constantly raising this point that they are beginning at the wrong end of the wire. Why do not they Mr. Wilson.

Mr. Wilson. begin at the other end of the wire and start supervising the voltage? It is all very well for gentlemen to order 200-volt lamps to a specification, and test them on an actual 200-volt circuit. But a lamp maker to-day, if he has 200-volt lamps ordered by a consumer, and if he knows his business, will find out the conditions of the circuit used by the consumer, and in nine cases out of ten he will supply lamps suitable to that circuit, stamped for the nominal voltage in order to satisfy the consumer. I have placed six actual curves on the wall taken with recording voltmeters. I would like you to examine those curves very particularly. The top curve, for instance, is a nominal 200-volt circuit, and for at least six or seven hours in the day—and they are the lighting hours—that circuit is running at anything between 217 and 220 volts. What is the lamp maker going to do if he follows out the theoretical idea and supplies an actual 200-volt lamp when he gets on to a circuit like that? Who will be blamed—the man at the engine end of the wire, or the man who stamps the name on the lamp? The latter, because the consumer thinks he is to blame. Then take the bottom curve, which represents a nominal 200-volt circuit. It will be noticed that for the major part of the time it is 196. Supposing we supply an actual 200-volt lamp for that circuit, this is the sort of thing we hear, "So-and-so's lamps give no light." We send down to the engineer and complain, and then he runs up the voltage, and what do we get then? That is a fact which I think should be brought more prominently forward in connection with all these discussions, which we welcome, concerning present-day lamps. Mr. Paterson goes on to say that the manufacturers miss the incentive to grade their lamps properly. Coming from Mr. Paterson we accept that statement, but as a manufacturer I have my own private views. We have to make lamps for the commercial circuits as they are given to us, and not for the theoretical circuits that are put down by the testing laboratories. There is no getting away from that fact. The obvious remedy for that condition of things is to grade the voltages. If you cannot grade your central stations and help the manufacturers, and at the same time help yourselves, then you must grade your circuits. I know of two or three instances in England to-day where the central station engineer has taken the trouble to go round to his districts and study the question, and at the present time we are receiving orders from such gentlemen for 226, 228, 230, 232 and 234 volt lamps. The engineer knows his average voltage in a particular district, and he takes good care that the consumer does not get lamps marked for the nominal circuit, but lamps marked for the actual voltage which exists in the district. It is trouble to the central station engineer, but it is a thing which has occurred in America and been overcome, and I hope sooner or later it will be overcome in England. Coming to the question of the two American curves shown, these are two very splendid curves. I venture to say that any English lamp manufacturer who was applied to would produce you a batch of lamps, not of 175, but of 17,000, which would give just as good a curve as these. I do not think that is any better

Mr. Wilson.

than the English lamps which are on the English market, and we have to work under very different conditions. The author states, on page 294, that it is almost impossible accurately to predict the voltage of a lamp. That is an absolute fact—we will not deny it. How are we going to get over that? We had a curve thrown on the screen last week, made by Sir William Preece, giving the actual nominal voltages of the circuits in England; and it will be noticed they all centre around 200 or 220 volts; there is nothing in between. If the lamp manufacturer cannot make lamps to come out at a certain definite voltage, what is he going to do with the outfalls? I have written to America in connection with this subject, but I am sorry to say I have not yet received a reply. I had hoped to put on the screen a corresponding curve of the American voltages as compared with English. What do we find out there? I had a visit, some three weeks ago, from the manager of one of the largest carbon lamp manufacturing companies in the United States, and he told me it was an absolute fact that, as Mr. Le Maistre said, they have formed themselves into a body of light suppliers. The central station engineers and the manufacturers act together, and they grade their voltages in order to assist one another. Do we find such a thing in existence here? I believe Mr. Gaster has tried for some considerable time to turn the manufacturers and central station engineers into suppliers of light, and that is what we have to be if we intend to be successful. No, we have not reached that state of affairs yet. The engineers blame the lamp makers, and the lamp makers blame the engineers, but the poor maker must keep his views in his pockets, and do the best he can under existing circumstances, and the lamps are therefore not graded as well as we know they should be. To take four lamps, or even ten lamps, out of a batch of the millions of lamps on the open market and make a test on them is obviously unfair to the lamp makers. We know most of the districts and the conditions under which they are run, and, as we know our business, when we get an order from such a district we entirely disregard the nominal voltage. We take the district and we supply lamps accordingly. If we know a man overruns, he gets that class of lamp. For instance, in one of these cases, if a test of the lamps on the nominal voltage will give 12 c.p., on a 220-volt circuit they will give 16, 17, or 18 c.p. Mr. Paterson also speaks about the waste of money which is going on and the necessity for accurate photometry. All this money is undoubtedly being wasted, and will continue to be wasted until we form ourselves more into a body of illuminating engineers, and act as the gas people do. Do the gas people ever bring up a faulty gas mantle and give us particulars with regard to it, saying that it gives a bad light, and exposing all their faults to competitors? Of course not; but we are doing just that kind of thing. Electric light engineers are simply exposing the faults of the industry to the merriment and profit of our friends in the gas world, and the latter are scoring all along the line. Why cannot we meet together and see if we cannot alter

Mr. Wilson. the present condition of things, instead of publishing all our faults to the world? I say it is the fault of the central station engineers. There is just one other point to which I should like to refer with regard to this curve. Supposing one has nominal 200-volt lamps, and the voltage is 210, which is a very fair average for a 200-volt station to run at; if the 200-volt lamp is run on the 210-volt circuit, what is the result? The actual watts increase with that lamp is 12·7 per cent., and the increase in the cost of the lamp is 10·6 per cent., a total increase to the consumer of 23·1 per cent. That is why one hears complaints when the bill is sent in to the consumer, and people say that the 64-watt lamps take 70 watts. With good English lamps run at the marked voltage they do nothing of the sort. The 64-watt lamps take 64 watts; but a 200-volt lamp running on a 210-volt circuit does take 70 watts.

Mr. Wigham. Mr. J. C. WIGHAM: To those of us who use a photometer for the purpose of determining whether a lamp is 8 or 16 c.p.—a fact which is not always determined by examining the mark on it—Mr. Paterson's paper is a most delightful one, dealing as it does with accuracies of photometric testing down to plus or minus half of 1 per cent.

The company with which I am connected have adopted the free distribution of lamps to their consumers in two or three cases. We have, therefore, now considerable experience as to what we may expect to get from makers of incandescent lamps, and, so far, the results which have been obtained, especially as regards uniformity of candle-power and life, can only be described as perfectly disgraceful. It may be due to want of grading; but, whatever it is, we get a most alarming variation in candle power, even from the same maker.

It is rather disastrous in some cases. A year or two back we obtained a batch of 8-c.p. lamps and they were mostly in the region of 10. After running a number of hours a good many of the older ones were giving 8½ c.p. The engineer recommended that they should be changed, as they were very black. The new lamps were put on, and the consumers began to grumble immediately, saying that they were very bad lamps. They were taken into the photometric room and tested, and it was found that the candle power of those 8-c.p. lamps was only 5¾. That is not a very successful way of renewing 8½-c.p. lamps, even if they are black.

This year we have been a good deal troubled with failures of lamps owing to the breaking of the filaments. That is a point, in our opinion, to which the lamp makers ought to pay a little more attention than they have done recently. We have endeavoured to make some tests to discover whether these lamps were breaking unduly soon or not, and we carried out in one of our stations a considerable number of tests in this fashion:—

The lamps, after they were returned to the store, were all graded according to the colour—that is, the blackening of the bulb—and sorted into three piles, each pile consisting of lamps which compared in blackness with a lamp which had been known to have run 200, 400, and 600

hours respectively. It was found that a very large number of these lamps had broken before they had become as discoloured as a lamp which had run 200 hours. These we are endeavouring to get the makers to renew free. The same subdivision was adopted of the lamps returned from consumers with unbroken filaments, and they were then tested for candle power and watts per candle. In the 8-c.p. lamps especially there is undoubtedly a great difference in the drop of candle power in proportion to the blackening of the bulb, due, we maintain, to badly made filaments. The tests have not been carried out sufficiently far to know whether this method of testing is reliable or not; but we are in hopes that we can enter into a contract with the lamp makers under a guarantee to renew lamps which do not come within a reasonable candle power in proportion to the blackening of the bulbs. The blackening does not necessarily tell exactly the number of hours the lamp has been burnt, because, if overrun, the blackening would be a great deal more; but inasmuch as the test is dependent on the blackening and not on the actual number of hours the lamps were in use, we are of opinion that this method of testing, especially the small candle power lamps, may prove very useful, and enable the lamp makers to discover which of their lamps maintain their candle power under service conditions to the best advantage. After all, the supply company chiefly want lamps of a uniform candle power to start with, which will lose their candle power at a uniform rate. Mr. Wigham.

Mr. A. VERNON HARCOURT, F.R.S. (*communicated*): The knowledge that atmospheric conditions have an important influence on the candle power of flame standards is not quite recent. It must have been present to the mind of all who have thought out and arranged these standards, though an accurate determination of the correction to be applied for such atmospheric variations as are material was no easy task. For the approximate measurement of the light of flames, such as that of an oil lamp or gas burner, the probability that the standard flame and that whose light it is used to measure will be similarly affected by atmospheric variations is sufficient to make the correction of each unnecessary. When, however, a flame standard is to be used for standardising glow lamps, the corrections to be applied must be determined; and Dr. Liebenthal and Mr. Paterson have—not, I think, discovered an error—but done an essential service which few had so good means of doing in making these determinations. Mr. Harcourt.

In 1885 I examined the influence upon the 1-candle standard, which I had proposed, of variations in atmospheric pressure, by comparing it with a small glow lamp whose current was adjusted by means of an electro-dynamometer. I found that the pentane flame, which gave a light of 1 candle when its height was 63·5 mm. and the atmospheric pressure was 760 mm., must be raised or lowered in inverse proportion to the rise and fall of the barometer for its light to be constant.

My experiments upon the influence of atmospheric moisture began earlier, in 1879. I had then no glow lamp for measurement, and only

Mr. Harcourt.

compared the light given by a stored sample of coal-gas, burning from a London argand in a box photometer, when the air was dried and undried. Trays of quicklime were placed beneath the opening at the bottom of the box, the light was 16·6 candles; the trays of lime were removed, the light was 15·9 candles. On another day the gas was tested, and the yield of light was 16·2 candles; the trays were then filled with slightly warm water, light 15·4 candles; the trays were removed, the light was then 15·85 candles. Many such observations were made, but the attempts to measure the humidity with wet and dry bulb thermometers were unsatisfactory. It is certainly very difficult to treat the air in any way without interfering with its free flow, and thus affecting the light of the burner. I agree with Mr. Paterson that it is best to wait for natural variations.

There is another atmospheric condition, not named by Mr. Paterson, whose effect I hope he will investigate, namely, temperature. By the variation of this condition the flame standard and the glow lamp are both, however unequally, affected. A 10-candle gas lamp was placed in the centre of a cylinder 18 ins. in diameter and 20 ins. in height, within which was a long coil of metal pipe through which hot or cold water could be run, lined inside with black cloth. An 8-in. opening in its lid gave free passage to the air. Two small lateral openings served for the light going to the photometer and for observation of the flame. At temperatures 36°, 32°, 29°, and 24°, the light was 10·6, 10·4, 10·3, and 10·2 candles.

The temperature of flame is far removed from the temperature of the air, but the principle of the hot-blast comes in, and particles of carbon are measurably brighter at, say, 1,510° than at 1,500°. That the glass envelope of a glow lamp is hotter on a warm day is certain. The exchange of temperature between it and the filament through a nearly vacuous space, shown by the warmth which the glass maintains, though continually cooled by the air around it, must be much slower than that which is due to the free and rapid access of air to a flame. Is the effect of the temperature of the air around on the light of a glow lamp measurable or not? If the answer is not already known it can easily be obtained by experiment.

Since when the air is warmer the proportion of steam in it is generally greater, and since the light of a flame is increased by warmth and diminished by a greater proportion of steam, these simultaneous variations must tend to balance one another. But that which is needed, and which has already been partly supplied by Mr. Paterson, is a formula for correcting the photometric results obtained on comparing a glow lamp with a particular flame standard, by which correction may be made for the variations of pressure, temperature, and moisture, as the observed volume of a gas measured over water is now corrected.

Mr. Morris.

Mr. J. T. MORRIS (*communicated*): It is clear from Mr. Paterson's paper and from previous work on flame standards that proper ventilation of the photometer room is important. Reference is made to the

subject on pages 282, 283, and 287 of the paper, and it would appear that it has not been found possible to burn either the pentane or the Hefner lamp continuously without the candle power falling, and apparently this effect is chiefly due to lack of efficient ventilation. Surely it should be possible in an efficiently darkened photometer room, where work of the highest accuracy is carried out, to obtain a very slowly ascending column of fresh air in which the flame lamp is immersed while being used—the air moving so slowly as to cause no flickering of the lamp. Possibly the following arrangement might be found to work: Deliver fresh air slowly and steadily by a fan through a fair-sized grating (say 2 ft. square) in the floor directly under the flame lamp, and remove it at a similar grating in the ceiling directly above the lamp by means of another fan. Mr. Morris.

On page 288 the difficulty of finding a suitable resistance to use in series with a low-voltage lamp is alluded to. This difficulty can generally be overcome by placing a similar lamp in series with the standard running under the same conditions. I have myself made use of this method with tantalum lamps and found it satisfactory, the second lamp being worked (say in a cupboard) in the same room.

The *part* life curves of large bulb Fleming-Ediswan lamps given in Fig. 9 of the paper form a valuable addition to our knowledge of the behaviour of these secondary standards. As the author points out, the candle power continuously rises for a period of from three to four hundred hours. Has he reason to believe that the total life of these lamps would be of the order of several thousands of hours? It would be of great interest to know what the shape of the *whole* life candle-power curve would be. In an ordinary glow lamp the rise of candle power is generally complete within some forty hours of the start. Does the author consider that the large bulb lamp would give a similar life curve, only multiplied throughout by ten? I would suggest that the filaments of these six lamps which have been run for some four or five hundred hours should be carefully examined (without opening the lamp) to ascertain whether there is any change in their appearance either locally or throughout their entire length, so that an idea might be formed as to their probable future length of life.

Mr. IRWIN HOWELL (*communicated*): With reference to the statement on page 300 of the paper regarding a shortened life test of glow lamps by running them at some other efficiency than their standard efficiency, I would say that it is the standard practice of the British Thomson-Houston Company, Ltd., and of the General Electric Company of America, and, I believe, of some of the Continental lamp factories, to burn lamps on life test so that results may be obtained in approximately fifty hours and upwards. The curve shown in Fig. 17 is based on the law of the variation of life with change in candle power, and as well-known laws exist showing the variation of candle power with change of voltage, amperes, etc., the average variation of life and watts per candle may be calculated within limits of commercial accuracy. Mr. Howell.

Mr. Howell. I believe the law of variation of life due to change in candle-power is the same for all types of ordinary carbon filaments, whether flashed or unflashed, and I have yet to see reliable data disproving this statement. The original law of this curve (the life varies inversely as the c.p.$^{3 \cdot 65}$) was determined, I think, by Mr. John W. Howell, of the General Electric Company of America, in 1883, and has been re-determined several times since that date, and the elaborate and exhaustive tests that have been carried out from time to time seem to confirm the accuracy of the original exponent. This means that a 16-c.p. lamp of any voltage or efficiency, having a life of unity, will have a life of 12·55 if burned at 8 c.p., and a life of 0·08 if burned at 32 c.p.

Mr. Gaster. Mr. LEON GASTER (*communicated*): The paper read by Mr. Paterson deals so exhaustively with many questions relating to electric lighting that I will only refer in general terms to a few points which appear to be of practical importance.

In the first instance I should like to mention that it is very gratifying that the establishment of the National Physical Laboratory has rendered possible a closer co-operation with similar Institutions on the Continent and in the United States in making scientific researches of international importance, so that results can now be compared with those obtained in the other countries.

In the second part of the paper, which deals with tests made with 200-volt incandescent lamps, I cannot refrain from expressing my disappointment at the bad results obtained with most of the lamps supplied to the market. It has been said, with a great deal of justification on the part of the lamp makers, that it is to the extraordinary voltage fluctuations at which the current is supplied to the consumer in different parts of the country that the short life and bad efficiency of the lamps are largely due, and accordingly, until the central stations endeavour to regulate the voltage within more reasonable limits, it is useless for the lamp makers to supply lamps to any rigid specification, seeing that under present conditions in actual use the lamps will be prematurely destroyed. On the other hand, the central station engineers have a reasonable cause of complaint against some lamp makers for the many bad lamps supplied, which have been responsible for the dissatisfaction of consumers, and have thereby retarded the more extensive use of electric lighting. Under such circumstances they think it useless to go to the trouble of keeping the voltage steadier than they are doing at present. The consumer, although sympathising with the apparent *bona-fide* arguments of the two parties, has in the meanwhile to put up with indifferent illumination, not knowing upon whom to fix the blame. It is for this reason that I am speaking to a certain extent from the point of view of the consumer in stating that the results of Mr. Paterson's tests are unsatisfactory regarding the present supply of lamps, and in urging lamp makers as well as the central station engineers to do their very best to improve matters soon.

I should like to ask what excuse can the lamp makers give for such

irregular grading of their lamps as shown in the diagrams? especially when one has to consider the fact that the tests have been carried out on an almost ideal, steady, and perfectly regulated circuit. The excuse has been given by some that although the lamps are marked at a certain voltage and certain candle power, in reality these markings are only nominal; they must make the lamps suitable for the voltage known by them to exist in the respective districts in which the lamps are intended to be used. This explanation does not seem to be quite borne out when examining the diagrams given by Mr. Paterson. For a good lamp which was rated at 16-c.p., 200 volts showing only 11 or 12 candles, the life curve ought to have appeared quite different if the lamp was underrun, and it should not show such rapid decline in the life test. Mr. Gaster.

Why do not the lamp makers insist that something more is done at the central stations to regulate the voltage within the limits prescribed by the Board of Trade? I am at a loss to understand why those limits have been prescribed at all if they are not to be enforced. One must not forget that the bulk of the electric current supply in this country is still used for lighting purposes, and that the carbon lamps cannot stand high voltage fluctuations without being damaged. This state of affairs, I think, ought not to be allowed to continue. The consumer has every right to ask that lamps marked at certain candle powers, voltage, and energy consumption should be as near as possible true, and it should be made illegal and punishable to sell lamps which are wrongly marked, due allowance being made for errors in accuracy of testing and the manufacture of lamps. The lamp makers may come forward and point out to the consumer that in order to supply properly graded lamps a higher price will have to be charged for them. I believe, however, that it will be infinitely better for the consumer to pay the few pence more asked for good lamps, because the difference in the initial cost of the lamps is more than counterbalanced by the economy derived from the current consumption during the life of the lamp, and also by keeping up its useful life a much longer period. The recommendation to use high-efficiency lamps and to adopt a system of frequent renewals deserves careful consideration at the hands of the consumer where energy is charged at rather high prices and the voltage does not vary too much.

Some of the lamp makers I know have complained of those who are trying to show up the bad qualities of the lamps supplied, because they say the opposition makes capital out of those condemnations, and they thereby harm the development of the electric light industry. To my mind the effect ought to be the reverse. The genuine manufacturers will be grateful to such, because they are doing a great service in showing up those manufacturers who trade on the ignorance of the public in these technical matters and supply bad lamps, and also because they are pointing out that there are some good lamp makers who are in a position to supply the right article if paid the proper price. From the results shown by Mr. Paterson, some of the lamps are certainly of superior quality. It is only due to the publication of results obtained

Mr. Gaster. by such unbiassed and scientific experiments as those carried out by Mr. Paterson at the National Physical Laboratory that the lamp makers themselves have an opportunity of getting acquainted with the true nature of the quality of the lamps they are making, or supplying to the consumer. If the lamp makers say that owing to the great foreign competition, and on account of the present fiscal conditions, it is possible to import into this country any worthless lamp, and sell it at a ridiculous price with which the home product cannot compete, it may be a simple matter, I should venture to think, for the lamp makers to combine and approach the Board of Trade and ask for protection against such unfair dealings, by demanding, for instance, that not only should the boxes containing the lamps be marked with the country of origin, but also that the lamps imported should bear a distinct mark by which they may be recognised. The lamp makers could then warn the public against any bad lamp so imported, and I have no doubt that, should their claims be proved correct, the public will understand where its own advantage lies; but until the marking of the lamps has a legal meaning attached to it, so as to become like a "trade mark," I am afraid that the grading the lamps will be continued to a very large extent in the same haphazard manner, and the electric lighting industry will be left in the same chaotic state in which it finds itself at present.

Regarding the attitude which is expected to be taken up by the municipal authorities and private companies who own electric supply concerns, I should think that it would be advisable for them to adopt a scheme by which the lamps supplied to the consumer are tested according to the recommendations of the specification, and then either to supply the lamps free of charge to the consumer or have them, after being tested, sold through the local contractors. In this way the bad lamp will be kept out of the market to a very large extent. It might be argued that for this testing work heavy expenditure would be incurred. My suggested remedy for this is that all the different municipal authorities and companies should contribute annually a fixed sum of, say, £10 to £15 towards the maintenance of a central testing department, which should be run for the benefit of the public. This testing bureau should be conducted under the supervision of a committee composed of representatives of central station engineers, lamp manufacturers, consumers, etc., and there the lamps should be tested according to an adopted specification. For any cases of dispute which might arise concerning tests, the National Physical Laboratory should act as the court of appeal. To my mind this will be a step in the right direction, and may help to put the lighting question on a sounder basis.

I will not discuss at this juncture the *bona-fide* claim of the "illuminating engineer," whose business it will be to advise the consumer how to use the lamps to the best advantage and to get the desired illumination at the least expense, but I should like to mention that this new profession of the "illuminating engineer" would form a very

valuable link between the lamp makers, the central station engineer, and the consumer. Whether the carbon lamp has a long lease of life or not, in my opinion a fair trial should be made of the recommendations embodied in the specification for carbon glow lamps prepared by the Engineering Standards Committee. As these recommendations are only to be applied for one year, and are subject to revision, I think every opportunity should be given for introducing them, and in accordance with the experience gained during the year's trial, the necessary alterations should be made. Mr. Gaster.

Mr. T. A. ROSE (*communicated*): There is only one point in Mr. Paterson's extremely interesting and instructive paper to which I wish to refer, and that is his apparent assertion, on page 295 and elsewhere, that the quality of an incandescent electric lamp depends entirely on the make of the filament. In reality, the quality of a modern carbon filament lamp depends quite as much, if not more, on the vacuum as on the filament. At the present time there is very little difference in the filaments used by the leading lamp makers, and therefore the differences observed in their lamps are more likely to be due to the different methods employed for exhausting the bulbs. Mr. Rose.

This question of the vacuum is also one of the main causes why lamp makers, as Mr. Paterson points out, find a difficulty in making lamps to come out exactly as intended with regard to voltage and candle power. As the paper under discussion is not one dealing with the technicalities of lamp making, I cannot now describe and compare the different methods used for exhausting lamps, especially as I am afraid I should be prejudiced in favour of my own process, but it certainly is a fact that some methods produce much more uniform and better lamps (using the same filaments) than others.

Mr. J. S. DOW (*communicated*): It seems to be generally admitted that the Harcourt 10-c.p. pentane standard and the Hefner amyl-acetate standard are the best flame standards at present available, and the pentane standard seems to me to be, in almost every respect, the best lamp of the two. Ten c.p. is a more convenient unit than 1 c.p. The white colour of the Harcourt flame is preferable to the red colour of the amyl-acetate flame, and, as Mr. Paterson points out, the use of the chimney in the Harcourt lamp gives a very much steadier flame than the flickering flame of the Hefner lamp, which is so very sensitive to draughts. Moreover, a change of only 1 millimetre in the height of the Hefner flame means 3 per cent. difference in the candle power of the lamp, while a variation of 5 millimetres on either side of the correct position of the flame does not materially affect the light given out by the Harcourt standard. Mr. Dow.

My own experiments quite confirm all that Dr. Fleming said in 1903 about the necessity for good ventilation, but the Hefner lamp is probably quite as sensitive to vitiation of the atmosphere of the photometer room. When I visited the Reichsanstalt in Berlin last summer, I was told that they never used the Hefner lamp for longer than half an hour in making tests, for this reason. The correction for water vapour is about

Mr. Dow. the same in the two cases. The one advantage which the Hefner lamp does seem to possess is that it is practically independent of barometric pressure. An alteration of 1 centimetre in barometric pressure only alters the candle power of the Hefner lamp by 0·1 per cent., but the same alteration would alter the candle power of the Harcourt lamp by 0·8 per cent. On the other hand, Frankland, as far back as 1861, found that under the same conditions the candle power of coal-gas flames changed no less than 2 per cent. I should like to ask Mr. Paterson, therefore, if he can explain exactly what is the reason for these differences, because it seems possible that a slight variation in the Harcourt lamp might lead to improvement in this respect.

I cannot agree with Mr. Harrison that this paper is one more step towards doing away with the flame standard. The Harcourt lamp will certainly continue to be our standard for some time to come, and, it seems to me, every piece of additional knowledge we gain as to its vagaries, so far from tending to displace it, adds to its value. Our primary standard must have two qualities. It must be easily reproducible, and must not be easily permanently altered in candle power. At present glow lamps are not identically reproducible, and there is probably no glow lamp in existence which could not be rendered worthless as a standard by being overrun for a few minutes. Mr. Wild and I have both found that glow lamps do not give exactly the same results after being overrun, if only for a few minutes, and—worse still—the lamps gradually alter afterwards. This, to my mind, constitutes a fatal objection to the method of testing the life of lamps by overrunning for a short period. Such treatment must put a strain on the lamps, and their subsequent recovery must be in evidence afterwards.

Mr. Paterson's results shown in Fig. 18 come as a surprise to me. In 1903 I tested a very large number of lamps for Professor Ayrton, when he was investigating this method of life testing, and found that the lamps behaved most erratically. Some lamps rose in candle power after being overrun 25 or 30 per cent. for one hour; others fell. But I have almost always found that overrunning a lamp for a very short period causes a *rise* in candle power and wattage, whereas Mr. Paterson's curves show a continuous fall.

As regards secondary standards, it seems to be the correct plan to test glow lamps against glow-lamp standards, and gas lamps against gas standards. If glow lamps are tested against a gas standard all possible errors occur—errors due to atmospheric influence on the gas standard and errors due to faulty regulation of the P.D. across the glow lamp (which must now be known to at least 0·2 per cent.). But exactly the same remarks apply to testing gas lamps against a glow-lamp standard. Glow lamps are tested against glow lamps, so that a slight variation in the P.D. across both will not affect the results. In the same way gas lamps ought to be tested against a gas lamp of the same nature, for then atmospheric conditions affect both in the same way.

On the other hand, in a laboratory in which very exact electrical measurement is possible, a glow lamp is probably the best standard to use when testing other lamps the candle power of which does not vary with the P.D. according to the same law as for glow lamps. But in such cases it seems better to measure the *current* through the lamp and not the P.D. across it. According to Mr. Paterson's curves in Fig. 9, the current through the standard almost invariably rises as the candle power rises. Therefore, if the current through the lamp had been maintained constant, the candle power would have been much more uniform. This method has other advantages. It gets rid of all danger of "contact" troubles in the lamp holder, it minimises the possible errors introduced by inadvertently overrunning the standard, and (in the case of carbon filament lamps) the current through the lamp need not be known so exactly as the P.D. across it. Mr. Dow.

The divergences in behaviour of the standard high-voltage lamps shown in Figs. 10, 11, and 12 are very curious. Why is it that there is an initial rise in candle power in the life of some lamps and not in the case of others? Does this mean that some of the lamps were run for a certain period by the manufacturers before being submitted for test, so that the initial rise in candle power was worked off? The divergences are the more surprising because the ordinary 200-volt lamps, the life tests of which are shown in Fig. 15, behave very uniformly. One would therefore have expected picked standard lamps to give *more* uniform results.

Another question affecting the use of standard lamps is that raised by Mr. J. T. Morris in the discussion of Dr. Fleming's paper in 1903. Mr. Morris described experiments which apparently showed that for every 9° C. rise in the temperature of the photometer room the candle power rose 1 per cent. Dr. Fleming, however, in a subsequent paper before the British Association, came to the conclusion that the effect was inappreciable. Therefore I should like to hear what is Mr. Paterson's experience on this point.

Finally, there are two points of interest in the carrying out of Mr. Paterson's life tests. From the casual reference to the regulator "in series with the field circuit of the alternator," I gather that Mr. Paterson's life tests were made on an alternating P.D. Is it an established fact that a life test of carbon filament glow lamps on a direct or an alternating P.D. leads to the same results? And, if so, what must be the limiting value of the frequency for this to be true?

Secondly, has the position of a glow lamp (whether sideways or upside down, etc.) during a life test any effect on the results? If the surprising results of Messrs. Cravath and Kansingh,* who found that the mere frosting of lamp globes reduced the life of glow lamps considerably, be confirmed, this point is worth consideration.

Mr. C. C. PATERSON (*in reply*): The comments made by those who have joined in the discussion on this paper fall naturally under two heads: those concerning flame standards and their ratios, and those Mr. Paterson.

* *Electrical World*, vol. 47, 1906, p. 567.

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concerning the general behaviour and testing of electric lamps. I propose to reply to the remarks under these two heads.

Dr. Fleming pointed out a difference observable between the ratio of the Hefner to the pentane lamp as found by the direct comparisons described in this paper and as deduced from the values of electric lamps measured in Germany against the Hefner unit, and in Bushy against the pentane standard. I have also found a difference in the same direction in lamps which I have myself taken to Berlin. We have this matter at present under investigation, and I cannot, therefore, offer any complete explanation. There is no doubt that the average of the ratios given in the table on p. 285 is correct to a fraction of 1 per cent. It must be remembered that in comparisons through the medium of electric lamps, the assumption is made that the condition of the atmosphere is reproducible in the two localities and also that the methods are identical of measuring the composition of the atmosphere in the various places where the flame standards are used. There is a difference, for instance, in the standard methods of measuring humidity in this country and in Berlin, which may account for a portion of the discrepancy.

Mr. Wild points out a very large discrepancy between his pentane lamp, tested first at Bushy and then at Westminster, and suggests testing in rooms of different sizes and heights, as well as near to, and away from, walls. The new laboratories at Bushy are well adapted to such measurements, and I shall try, at an early date, whether these causes have any effect on the light. It is quite clear that much remains to be done in the way of further investigation before the reliability of any of the present flame standards can be considered satisfactory from the point of view of the electrical engineer who has to deal with sources of light which are unaffected by atmospheric conditions.

I agree with Mr. Harrison that flame standards are not suitable for commercial electrical photometric work, but from the point of view of the gas industry it must be remembered that the 10-candle pentane lamp is a very suitable unit indeed, more useful than a violle or metallic filament standard which is unaffected by atmospheric conditions. I am very much interested in Professor Ayrton's figure comparing the Dibdin standard and the 10-candle Harcourt standard. I have not had occasion to work with either the 1-candle pentane lamp, or with the Dibdin standard. If, as Professor Ayrton's figures show, the Dibdin standard makes the unit of candle power larger than the Harcourt 10-candle pentane lamp makes it, then the ratio of the German to the English candle as given by the Dibdin unit is nearer the old value of 0·88 than the newly determined ratio of 0·92 given by the 10-candle Harcourt lamp, burning in an atmosphere containing 10 litres of moisture per cubic metre of pure air.

Mr. Trotter adversely criticises my advocacy of the 10-candle pentane lamp as a better standard of light than the Hefner unit, and speaks of the trifling advantages of the pentane lamp over the Hefner. After having used both lamps for standardisation purposes rather than

for general commercial laboratory use, I cannot agree with him that the advantages are trifling. The difficulty of accurate flame adjustment and the lambent character of the flame render measurements with the Hefner lamp liable to greater errors than is the case with the pentane standard, and the difficulty of accurately stepping up from the 1-candle unit to the 16- and 30-candle lamps used in practice is a very real one. Although the pentane flame is affected to a greater extent by barometric changes, I do not regard this as a serious disadvantage. Barometric pressure is a definite quantity and easy to measure and as, for standard work, a correction must be made anyway, it does not much matter if its amount is 0·1 per cent. or 0·8 per cent. As regards humidity correction, which is the more serious of the two, there is very little difference between its amount in the case of the two standards. I also cannot follow Mr. Trotter's reasons for wishing to alter the British candle in order to conform in dimension to that used in Germany. The British candle is practically identical in size with that used in France (see table, p. 285) and in America, the candle in the United States being derived from the Hefner unit by increasing the latter in the ratio of 0·88 to 1. Why, then, should these three countries alter the dimension of their light unit in favour of a standard which, owing to its small candle power, will become increasingly more difficult to use as the magnitude of the lights employed commercially increases? Mr. Paterson.

I must thank Mr. Cooper for calling attention to a point which perhaps is not clear from the paper, unless the latter is very carefully read. The experiments on the effect of carbon dioxide described on pp. 273 and 274 are only qualitative. Table I. gives the observed fall in candle power due to the combined effect of increase of CO_2 and decrease in oxygen. In the second paragraph on p. 274 it is pointed out that this decrease in candle power is very little greater than when the lamp is left burning in a closed room *without* artificial increase of CO_2. It therefore follows that the effect of CO_2 is negligible under all ordinary circumstances since the amount of this gas in the atmosphere never reaches such a high figure as that attained when it was artificially introduced into the air of the photometer room and reached a value of 1·9 litres per cubic meter.

In reply to Mr. Cooper's queries, the change in CO_2 was produced by causing gas from a laboratory CO_2 kip to pass into the air of the room. The samples of air were taken from the immediate neighbourhood of the lamp by very slowly sucking air into previously exhausted 10-litre vessels at the rate of about 1 litre in two minutes. I have endeavoured to separate the air drawn into the saturator from that in which the flame burns by first introducing CO_2 directly into the tube leading to the saturator, and secondly, by feeding the saturator with fresh air from outside the room. Neither had any observable effect on the candle power, and I therefore conclude that the air which surrounds the flame when it burns is that which has the most influence on its light intensity. I agree that the method of ventilation

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sounds primitive. It must be remembered, however, that the Hefner lamp cannot be properly used in any but a perfectly still atmosphere, and I think a scheme of ventilation has to be rather elaborate to clear out the air of the room without causing sufficient disturbance to affect even such a stable flame as that of the pentane lamp. I feel sure that the method described by Mr. Morris would be found unworkable with the Hefner lamp. On a windy day, even when all doors and windows are tightly closed, the flame wanders in a most exasperating way, and observations can only be made during the short, quiet intervals when the flame is approximately steady.

I am much interested in the suggestive contribution which Mr. Harcourt has made to the discussion. As regards the effect of variation in temperature on the candle power of the pentane lamp, I suspected for the reasons Mr. Harcourt gives, that temperature would have a disturbing influence. In deducing, therefore, by the method of least squares, the corrections for barometer and water vapour changes from the observations shown in Fig. 1, I introduced a term for variation of temperature from the mean. The term, however, came to practically zero. I therefore assumed that for all ordinary temperature variations met with in practice, no correction was necessary. Mr. Norris, in a paper read before the Denver Meeting of the American Gas Light Association, October, 1900, came to the same conclusion. He had two lamps burning in two different rooms with a bar photometer between them. There was a window in the dividing wall, which allowed the light from both lamps to fall on the photometer. The tests made covered a range of temperature of from 24° C. in one room to 37 °C. in the other, and, according to his report, the relative values of the lamps remained practically unchanged.

As regards the barometric correction for the lamp, Mr. Harcourt has handed me a table showing a number of his own observations.

Barometer mm. (b)	Observed C.P.	Calculated C.P. $10-0·008\,(760-b)$.	Observed − Calculated.
744·1	9·87	9·87	0·00
747·8	9·93	9·90	+ 0·03
749·1	9·94	9·91	+ 0·03
755·1	9·93	9·96	− 0·03
755·5	10·04	9·96	+ 0·08
			Calculated − Observed.
765·3	10·07	10·04	− 0·03
767·0	9·95	10·06	+ 0·11
770·7	10·04	10·09	+ 0·05
772·9	10·05	10·10	+ 0·05
774·1	10·09	10·11	+ 0·02

They were obtained by comparing the 10-candle pentane lamp with the 1-candle standard. The flame of the latter was varied in height

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according to the height of the barometer, the necessary amount having been previously determined by Mr. Harcourt as 1 mm. for every ¼ inch of barometric variation. The results are given in the accompanying table, which Mr. Harcourt has kindly permitted me to publish.

The question which Mr. Dow raises as to the reason why barometer changes affect the candle power of flames is one concerning which I do not venture a definite reply. When, however, one considers that the vapour from the amyl acetate lamp is burnt at the moment and at the point of vaporisation, but that in the case of both the gas and pentane flames, gas, as such, is pressed or sucked through the holes in the burners, it would certainly not be safe to assume that all are going to be equally affected by changes of outside pressure.

Passing to questions which more directly concern electric lamps: in reply to Dr. Fleming, the large bulb standards were compared, during their ageing, against other large bulb standards, and not against the pentane lamp. I think there is a real demand for high voltage standards among engineers who have a reasonably steady source of supply, the variations of which will only very slightly affect the candle power, if in the setting of the inter-comparison lamp both it and the standard can be run in parallel.

Mr. Robertson suggests that, on account of the outfalls, it will not be possible to improve the uniformity of lamps until there is as large a number of supply pressures in this country as in America. One quite appreciates that the narrow American limits cannot be adhered to here, as things are at present, but I should like to ask him if he maintains that an improvement cannot be made on the basis of the wider limits defined in the specification. In view of the consistently better rating shown by the products of some makers than is the case with others, I cannot help thinking that a great advance is possible.

Mr. Russell suggests the difficulty of assuming that the law of inverse squares holds on account of a lens effect produced by the bulb. I think the fact that all commercial horizontal candle-power measurements are made when rotating the lamp should prevent any errors being introduced due to this cause.

I congratulate Mr. Wild on the results he has obtained on high voltage lamps. I wish I had included some of his lamps among the various types which I tested. I have also noticed the effect which Mr. Wild and Mr. Dow mention, due to a momentary application of excess voltage to standard lamps. I have found, however, that a rise in candle power by no means always occurs, and that as often as not the candle power falls. I think the chief difficulty arising from making candle-power measurements always in the maximum direction is that under this system a maker could reasonably claim that the candle power of his lamp should be measured exactly in the centre of one of the bright bands of light which are to be found in nearly every lamp. Mr. Wild has probably come across lamps, as I have, in which these bands are broadened out in a most curious way, giving large areas over which the illumination is about 25 per cent. brighter than over the other

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portions. In answer to his comments on life tests, the specification does not say that a lamp is no use after it has reached exactly 80 per cent. of its original candle power. The 80 per cent. line is a convenient one for the purpose of testing whether one filament is as good as another. The term "useful life" must not, I think, be taken as meaning too literally that the lamp is of no use whatever when this point is reached.

I do not agree with Mr. Wild's objection that large errors are introduced in judging when a batch of lamps has reached the 80 per cent. point. In the first place only five lamps are tested for life out of a large consignment, and measurements on them can therefore be made accurately and check readings taken at the end. Secondly, each 100-hour point during the life test represents, on the average curve, the mean of five lamps. If, further, the exceptional case happens that the life curve has assumed a nearly horizontal direction at the 20 per cent. line, it is probable that the batch will not pass under Clause 13 of the specification, which necessitates that the mean candle power during life shall not fall below a certain value. I think the advantage of testing to the 20 per cent. point is that a slightly more rapid falling off in candle power will make a relatively greater diminution in the useful candle hours. These are guaranteed, and the method should therefore be an incentive to a high average candle power. The method favours what I think is a desirable system of buying lamps on a basis of bonuses and penalties according as the candle hour performance is good or bad. The point which Mr. Wild raises as to differences in reduction factors is a real one, but the wide limits he has taken are not, I think, met with in ordinary coiled filament lamps. Certain differences, however, do exist, and the only really satisfactory method is, as he points out, to measure the spherical candle power of lamps. I cannot help feeling, however, that if this had been put into the specification, it would at once have prevented its more general use in commerce. As things are at present, makers will tend to adopt that type of filament which throws 2 or 3 per cent. more light than at present in a horizontal rather than in a vertical direction. This can be no practical disadvantage to the user.

Mr. Harrison suggests a light standard consisting of a metal filament of given resistance, cross section, and length. I think, however, he will find a difficulty in reproducing the emissivity of such filaments to the desired degree of accuracy.

I agree with Mr. Cooper that the life test (under the specification) of any individual lamp is a test rather of the filament than of the particular lamp in question. It must be remembered, however, that it is a test, the results of which represent what the average performance of the batch will be. Assuming that the lamps in a consignment fall about equally on either side of the value aimed at by the maker—that is to say, that they are distributed symmetrically about the centre point of the target—then the results of lamps tested for life at definite watts per candle will represent the true performance of the average

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lamp of the whole consignment. The initial rating test ensures that lamps fall reasonably near the values specified; the life test proves the filament and vacuum. I think what a consumer of lamps wants to know is, not so much the *exact* life which a lamp will give when run on his circuit, with its variable voltage, but rather whether the lamps he is buying are really of high grade quality. A lamp which is better than another when run under the test conditions given in the specification will be equally better when run on a circuit with variable voltage, although, of course, in the latter case the life of neither will be as long as when run under test conditions. If on test, lamps with the inevitably wide variation in efficiency are to be run at marked voltage with a varying degree of pressure fluctuation, it is obviously impossible to specify anything definite as regards their behaviour. If, however, test conditions are specified which are perfectly definite, although they may not be exactly the same as running conditions, yet they will show whether lamps are of high grade quality or not, and to what extent they will or will not give the best possible results when run on the commercial circuits.

I am glad that Mr. Wilson has spoken so plainly about the way in which manufacturers find it necessary to supply lamps which are wrongly marked in order to suit both the buyer, and the circuit on which the lamps are to be run. I cannot, however, agree with him that this is any reason for not testing lamps or supplying to specification. He suggests that we should allow important matters which concern the public welfare to slide, because the consumer or station engineer refuses to appreciate that parts of the network are run at 5 or 10 per cent. above nominal pressure. This is surely an undesirable course to take. The sooner such people, if there are any, learn the truth and have to face the facts, the better will it be for themselves, the lamp-makers, and the general public. If no lamps can be bought but those which are what they are supposed to be, there may at first be trouble in some quarters, but the causes will be investigated, and when appreciated we shall be nearer the desired end where voltages are graded in different parts of a district, according to whether they are near to or far away from a feeding point, and lamps will be bought accordingly.

I am much interested in the experiments which Mr. Wigham is carrying out of testing old lamps according to their colour. I should have thought, however, that the severe blackening which frequently takes place at the moment of failure of a lamp would have caused the tests to come out very much in favour of the manufacturer, as in this case the burnt-out lamp would appear from its colour to have run longer than was actually the case.

In reply to Mr. Morris, I should hardly think it safe to assume that the large bulb standards would last for as long as he suggests. I do not venture a definite opinion, but should think it probable that their fall in candle power would be very gradual, but that failure would eventually take place due to a short length of the filament increasing

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in resistance. It looks as if the lamps would behave very differently, and that some of them would have a more rapid drop than others.

I agree with Mr. Gaster that even if lamp markings are only nominal, as is asserted, it is no excuse for bad rating. Suppose lamps are supplied for a 210-volt circuit but tested at 200 volts, if they have been properly rated at 210 volts they will lie equally well together if tested at 200 volts. They will, of course, be uniformly low, but will not be dotted all over the diagram, as is the case with many types shown in Fig. 13. Mr. Gaster has overlooked one fact when he refers to the life curves of the lamps tested. He suggests that an under-run lamp starting at 11 or 12 candles should give a better life curve than the ordinary 16-candle lamp running at normal and higher efficiency. This is, of course, true, but all life tests shown in Figs. 15 and 16 were made, *not* at marked volts, but at 3·8 watts per candle (initially). They were, of course, tested on the bench at marked volts, but on life test the volts were either raised or lowered to bring all to a uniform efficiency. Hence all the curves are strictly comparable.

I wish, in spite of the reasons Mr. Rose gives for not entering into details, that he had given us some of his wide experience on the question of the production of vacua and the effect on the life of the lamps of slight changes in the degree of exhaustion in the bulb. It is generally realised, of course, that if the vacuum is indifferent it will considerably shorten the life of a lamp, but I know of no data giving results of comparative tests on lamps with varying degrees of good vacua. If, as Mr. Rose suggests, the varied performance of lamps on life test is principally due to different degrees of exhaustion in the bulbs, the fact is one which deserves much more attention and investigation than is generally bestowed on it.

Regarding the over-running tests mentioned by Mr. Dow, I have not been able to see any sign of a prolonged rise in candle power such as he experienced with his lamps in 1903. The over-run lamps which I tested were measured every five minutes during the first hour and seemed to go through the same cycle of operations as in the test at normal volts, only, of course, in a much shorter time. I agree with Mr. Dow that in many instances it is better to measure the current in a standard than the voltage across it. For ease and accuracy of working, however, the method of running both standards and comparison lamps at the same voltage has much to recommend it. In the case of makes D and F (Figs. 11 and 12) the lamps were run for a time before I had them, in order to get over the initial rise. As regards the effect of temperature on glow lamps, I feel some diffidence in expressing an opinion which is opposed to that of such a careful experimenter as I know Mr. Morris to be. In several lamps, however, which I have gradually raised to about 40° C., I have not been able to observe the effect which he found and published in 1902.* In reply to the question

* Since writing this, Messrs. Laporte and Jouaust have shown me the results of some experiments made by themselves, in which they could distinguish no difference in the candle power of a glow lamp, although they raised its temperature to 115° C.

of life tests on direct and alternating currents, I have not sufficient data to give an exact answer. I have, however, run lamps on a direct-current circuit for a portion of their life and then changed over to as low a frequency as 25 ∾ in order to see if any effect was observable. It was not possible, however, to tell from the life curves where the change over took place. I regret, also, that I have not data with reference to the running of lamps in an upright or pendant position. The fact that lamp-makers do not mind what position is adopted so long as it is not within 45° of the horizontal leads me to suppose that the point is not a vital one. Mr. Paterson.

The PRESIDENT: The members have already, I think, indicated by their applause their thanks to Mr. Paterson for his paper. I will, however, put the motion formally, that the thanks of the Institution be accorded to him for his paper, and to the other gentlemen who have contributed to make the paper a success. The President.

The resolution of thanks was carried with acclamation.

The following paper was then read and discussed :—

COMPARATIVE LIFE TESTS ON CARBON, NERNST, AND TANTALUM INCANDESCENT LAMPS USING ALTERNATING CURRENTS.

By H. F. HAWORTH, Ph.D. (Bâle), M.Sc. (Vict.), B. Eng. (Liv.); T. H. MATTHEWMAN, B. Eng. (Liv.); and D. H. OGLEY, B. Eng. (Liv.)

(*Paper read February* 7, 1907.)

INTRODUCTION.—Tests on Nernst and ordinary incandescent lamps have been mainly confined to the observation of their working on continuous-current circuits, and the study of the behaviour of such lamps with alternating currents has been somewhat neglected. With a view to determine whether the working of such lamps on an alternating-current circuit was in any degree different from that when using continuous current, a series of tests was undertaken in two parts, the results of which are recorded in the paper. In the first part the design of the automatic regulator is dealt with, and some preliminary tests are described. The second part contains an account of a more extended series of comparative tests.

PART I.

APPARATUS.—The greatest difficulty in making life tests on lamps is to keep the applied voltage constant, since if this is not done the value of the tests is comparatively slight, unless the actual pressure variations are recorded. The following is a description of apparatus specially designed and made for these tests in order to keep the voltage automatically constant. It consists first of a dynamometer (see Fig. 1) the fixed coils of which have a laminated iron core. Above the moving coil and fixed rigidly to it is a light, rigid rectangular aluminium frame (see Fig. 2). Across the middle of this rectangle a phosphor-bronze strip G F is stretched horizontally by means of the spring F, and on to the centre of this strip a light aluminium tube B K E is fixed, B K being 3 inches long and K E 6 inches. The coil is suspended at the top and bottom by phosphor-bronze strips. The bottom strip is attached to the dynamometer base by a spring and the top one to a bracket which carries a torsion head. This coil is controlled by a light steel spring fixed at one end to the aluminium rectangle and at the other to the torsion head, which is of the Siemens pattern. The bronze strip F G acts as a pivot for the pointer B E, and

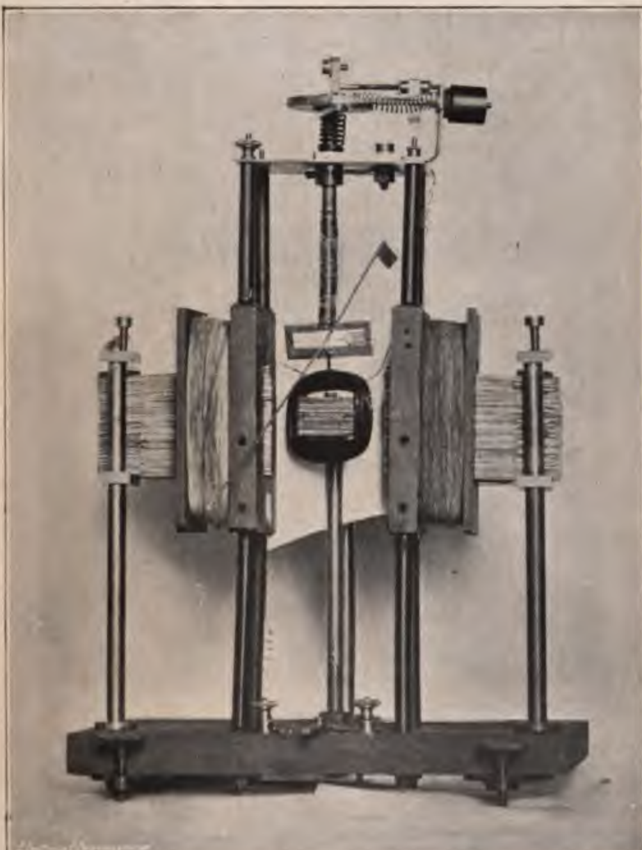


FIG. 1.

enables it to move up and down as it is deflected across the surface of the revolving drum (described below); the end of the pointer thus makes good electrical contact in all positions, the contact pressure being constant and depending on the position of the balance weights attached to the short end of the pointer.

The end E of this pointer B E moves over the surface of a revolving drum, the arrangement being similar to that used by Mr. S. G. Brown*

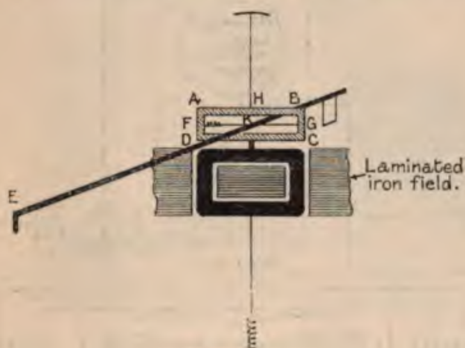


FIG. 2.

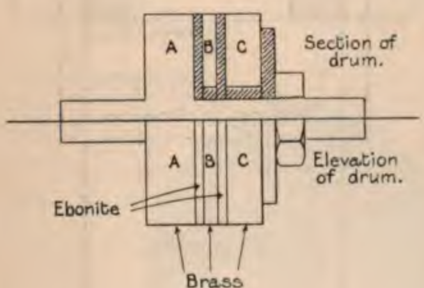


FIG. 3.

in his drum cable relay. This drum (Figs. 3 and 4) has two wide sections A and C and one narrow section B.

Contact is made to the A section through the bearings, B is insulated from A and C, and has no contacts; C is also insulated, contact being made through a springy copper brush.

Wires from the contacts to the outer sections of this drum run to a double relay in the manner shown in Fig. 4. The armature of this relay is fitted up as a reversing switch, contact being made through mercury cups.

The contact piece E of the aluminium dynamometer pointer is

* *Journal Institution of Electrical Engineers*, vol. 31, 1902, p. 1061.

made of black lead taken from a pencil. Many kinds of lead pencil are unsuitable for making good contact, and the right kind must be found by experiment. This blacklead contact does not pit the drum when it sparks like a platinum contact does (N.B.—The drum is made of brass), and it is much lighter, thus reducing the time of swing of the system.

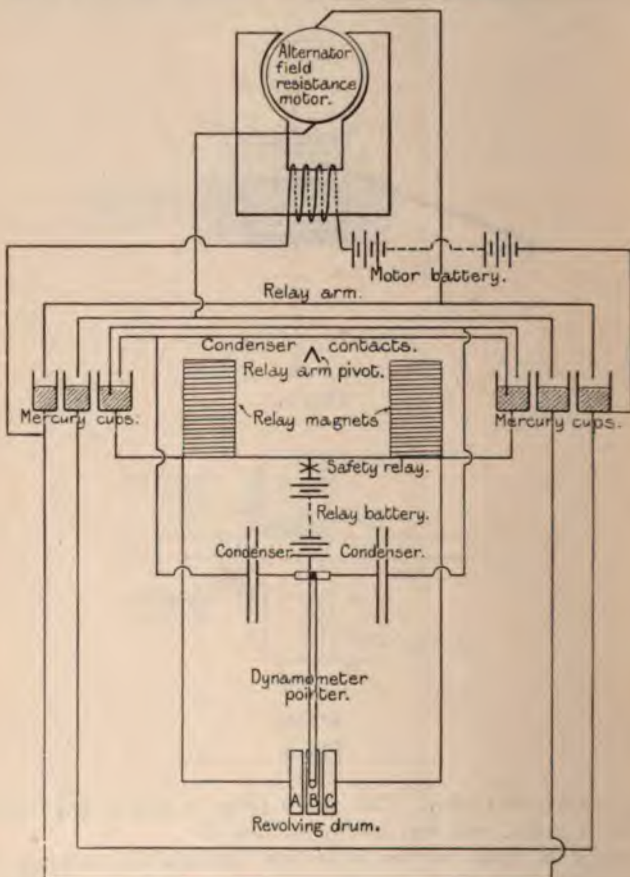


FIG. 4.—Diagram of Connections of Automatic Regulator.

Contact between the drum and pointer is facilitated by shunting condensers across the revolving contacts (as in Mr. Brown's drum cable relay just referred to), but in this case an arrangement of wires and mercury cups is fitted to the relay armature by means of which, when the relay moves slightly owing to an insufficiently good

FIG. 5.—Alternator Field Resistance and Regulating Motor.

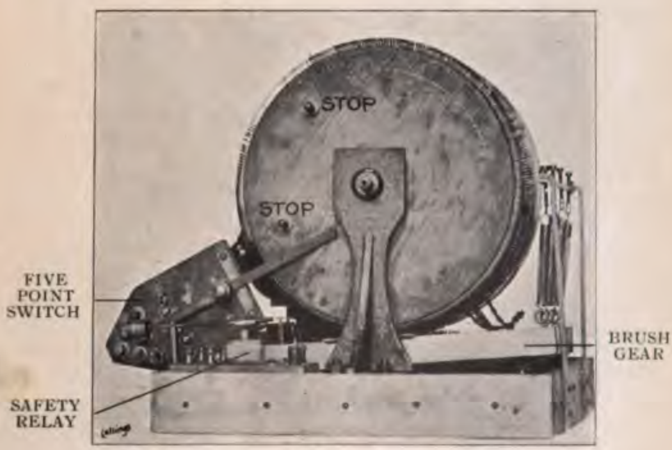


FIG. 6.

drum contact, all the condensers are shunted on the side on which the dynamometer pointer is trying to make contact, improving it and causing the relay to act fully (see Fig. 4). By this method fewer condensers are used to make good contact, or having a given number of condensers a lighter contact pressure may be used, thus reducing the friction between the drum and pointer and giving greater quickness of action.

The relay armature primarily controls the direction of rotation of a small series-wound electric motor (see Figs. 5 and 6) by reversing the armature current. This motor drives, by means of a worm gearing, an iron wire resistance, the four coils of which are fixed to four discs which are attached to an axle as shown. These four coils are permanently connected in series and rub against four horizontal brass brushes. The brush on the first coil is connected, by means of a flexible lead at the side, to the beginning of the second coil, and the second brush is connected to the beginning of the third coil, etc. The result is that when the drum is in one position all the resistance is in circuit between the two opposite terminals of the system, and as the drum turns the resistance is gradually diminished. Current for these tests was taken from a motor generator; the exciting current of the generator was sent through this rotary resistance (4 ohms), which was calculated to give a no-load voltage variation on the machine of about 8 volts either way, running at 230 volts.

The working of this system is as follows:—

The alternator P.D. is regulated by hand till the required value, 230 volts, is reached. The torsion head of the dynamometer is turned until the dynamometer pointer contact swings on to the centre neutral section of the drum, and is then switched on to the relay and motor resistance circuits. If the voltage of the alternator from any cause rises, the dynamometer pointer swings on to a live section of the revolving drum, and closes the relay circuit on one side, bringing the relay armature down on that side and thus closing, through mercury cups, the circuit of the motor driving the field resistance of the alternator (Figs. 5 and 6). This motor is set so as to revolve in such a direction that the field resistance is increased, thus diminishing the alternator field current and reducing the voltage.

When the voltage is again normal, the dynamometer pointer moves back to the neutral section of the revolving drum, thus breaking the relay circuit, which in turn breaks the motor circuit controlling the field resistance. When the voltage falls the reverse of this series of operations takes place, the pointer swinging to the other side of its normal position, which causes the other side of the relay to operate, and drives the field resistance motor in such a direction that the resistance is cut out, thus increasing the alternator P.D.

Thus we have an automatic, accurate, and sensitive method for keeping the voltage constant during life tests on lamps. This method, of course, applies equally well both for continuous and for alternating currents; the difficulty in constructing a voltage regulator for alterna-

ting-current work usually being the rather weak dynamometer force available, whereas in continuous work a very strong field might have been used, obtained either by permanent or separately excited electromagnets, with a very small current in the moving coil. It is obvious that if the rotary field resistance is at any time (when totally cut out or all in) insufficient for regulating the voltage fluctuation, the resistance will simply keep revolving until the flexible leads at the side jam. To avoid this, a five-point switch was placed at the side of the drum, which could be operated by stops placed on the outside disc (see Fig. 6). These five contacts were connected to four resistance coils which were in series with the field resistance. Normally the switch arm rested on the centre stop, and only two coils were in circuit.

If at any time the rotary resistance was insufficient for regulation, one of the stops on the drum would push the switch lever over so that three coils were then in circuit; this would probably give such regulation that the drum would return to its midway position. If this was insufficient four coils would be put in. The reverse of this takes place when the field resistance is too big. Coils are then cut out by the other stop.

The alternator field current also passes through a small safety relay (see Fig. 6). This relay closes, through two mercury cups, the battery circuit on to the dynamometer (see Fig. 4), pointer, and controlling relay. Thus it will be seen that if the total regulation of the field resistance and emergency coils is insufficient, through a big fluctuation of the accumulators supplying the exciting circuit, through breakage of any wires, or through accidental stoppage of the motor-generator from any cause, the regulating gear is automatically put out of action by the rising of the safety relay armature through either having no field current through it or having its current switched off by the drum resistance at the five-point switch. Thus the dynamometer relay circuit is broken and the lamps are switched off.

The dynamometer, relay, and revolving drum are placed in a glass case to protect the instrument from draughts and dirt, and the revolving drum is driven through a flexible piece of shafting by a small electric motor placed outside the case. The dynamometer coils of about 250 ohms resistance were placed in series with an oil-cooled manganin resistance of 2,500 ohms.

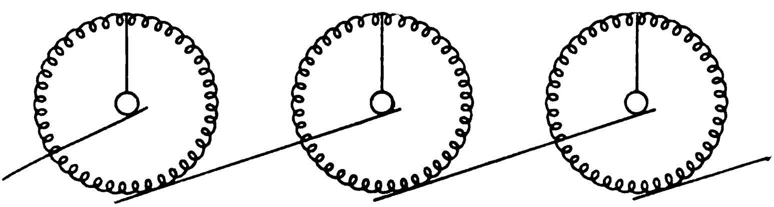
To obtain the varying voltage for the tests described in Part II. a rotary resistance was made which could be gradually introduced and taken out of the main circuit, so that the voltage across the lamps fell from 240 to 230, and then rose to 240 volts again.

The resistance consisted of three continuous iron wire spirals wound on three wooden drums (see Fig. 7). To a point on each spiral a slip-ring was connected. On each slip-ring a brush pressed, and each spiral pressed against a brush. The slip-ring of one spiral was connected to the brush of the next spiral. The figures show the positions of maximum and minimum resistance. One rotation every two minutes

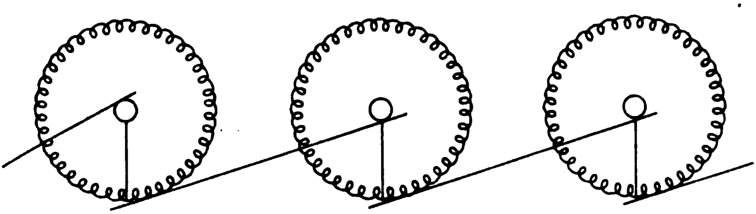
was produced by an electric motor, which drove through belts and pulleys.

Description of Lamp Holder.—In Part I. the lamps under test (Nernst lamps $\frac{1}{4}$ ampere and incandescent lamps by different makers) were placed in a circular block wooden drum 28 inches in diameter and 10 inches deep. This drum was divided into fifteen sections by iron partitions, and each section held a lamp. The drum was fixed on a vertical axle and was capable of rotation. It was surrounded by a thick black curtain, which could be arranged to cover all the sections except one, so that the candle power of the lamp in that section might be measured.

Each lamp had to be arranged so that the current passing through it could be measured, and the voltage across the glower of each



Maximum resistance.



Minimum resistance.

FIG. 7.

Nernst lamp was also determined. To measure these quantities with the minimum number of wires, the following mercury switch gear was devised (Fig. 8):—

CCC is a ring of mercury connected to one of the mains C. L is the other main, and it is permanently connected to one terminal of each lamp LLL. BBB are cups filled with mercury, each of which is connected to the terminal of a lamp LLL. The mercury cup A in the centre of the switch is connected to one terminal of a double scale ammeter A, the other terminal of which is connected to the main C.

Normally all the mercury cups B B B are connected to the outer mercury ring C C C, thus shunting the lamps directly across the mains. The connectors are pieces of copper wire with ebonite handles to lift them up with (see Fig. 8).

To find the current taken by any lamp, another connector is placed from (say) B 1 to A, the centre mercury cup. The current is now flowing to this lamp partly through the ammeter and partly through the other main C. The connector from B 1 to the outer ring of mercury is then removed, and all the current to the lamp now passes through the ammeter; no current is interrupted in doing this; the lamp is continually alight, and there is no voltage fluctuation. To disconnect the lamp from the ammeter these operations are reversed. This

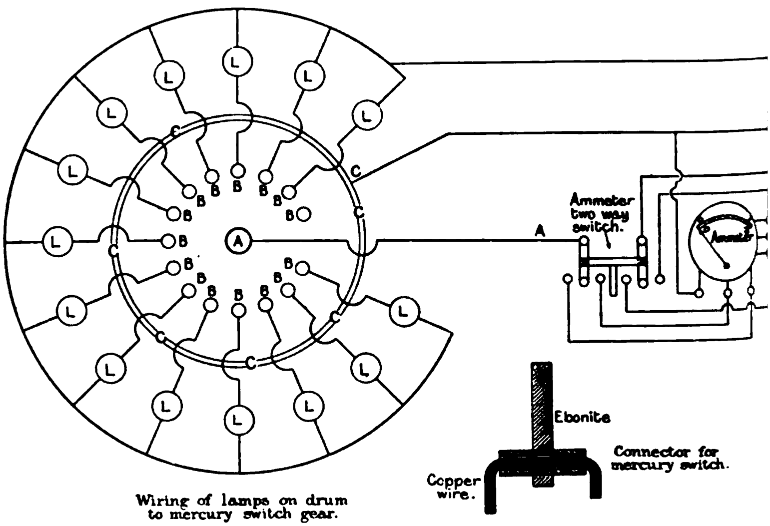


FIG. 8.

switch is oil insulated and placed on top of the lamp drum with the glower voltmeter switch. It is a four-way mercury switch, three of the cups of which are connected to the top contacts of the glowers. By these methods of wiring only four wires are needed to the revolving lamp drum.

In the use of the tests described in Part II. the lamps to be tested were mounted on an open wooden ring 4 feet 6 inches in diameter, which could turn about a vertical axis. This ring was placed in a well-ventilated, partitioned-off part of the photometer room.

To test any lamp it was brought in front of a hole in the partition and was isolated from the other lamps by a semi-cylindrical shutter so that it could be photometered.

Instruments.—The ammeter used was a Johnson and Phillips' hot-

wire instrument with two scales, one reading to 0·5 ampere and the other to 1 ampere. The scales were changed by switching a resistance in or out by a double two-way switch. The voltmeter used was a Kelvin standard multicellular instrument.

Photometric measurements were, of course, made without the ammeter in circuit. The photometric equipment consisted of a Lummer-Brodhun photometer, a bench 355 cm. long, a 2-candle power Argand burner and Methven screen used as a working standard, and a Vernon Harcourt 10-candle power standard pentane lamp used as a primary standard.

In Part II. a large bulb Ediswan standard incandescent lamp was used as a working standard of light. Its voltage was 35 and candle power 18.*

The voltage applied to the standard lamp was checked with a potentiometer.

The incandescent lamps were first tested in different horizontal directions, and the mean horizontal candle power was calculated.

The light from the lamps was measured during the life tests in one direction—usually that of maximum illumination—and it was assumed that the ratio of mean candle power to the candle power in this direction remained constant. This was checked in several cases, with satisfactory results.

In the case of the Nernst lamps the above determination was not made for each burner, as the filament was vertical in the ½-ampere type, and the illumination is sensibly the same in all directions.

Ventilation.—The ventilation of the photometric room was assisted by an electric fan.

Frequency.—The frequency was kept constant at 50 by altering the field resistance of the driving motor when necessary. Variations in frequency were observed with a Campbell's frequency indicator.

Time.—Time of tests was indicated by an electric clock, which was switched into the alternator field circuit as soon as the lamps were connected, and switched off when the main circuit through the lamps was broken.

Quantities Measured.

1. Time of observation from the beginning of run.
2. Current taken by lamp.
3. Voltage on glowers in Part I.
4. Candle power of lamp (compared with Methven screen in Part I. and with standard Ediswan lamp in Part II.).
5. Candle power of Methven screen compared with Vernon Harcourt pentane lamp, and candle power of Ediswan standard also compared with Vernon Harcourt pentane lamp.

* For a description of this standard see Dr. Fleming's paper, *Journal Institution of Electrical Engineers*, vol. 32, 1903, p. 119.

TABLE I.

Nernst Lamps.

Number of Curve.	Life of Glower (Hours).	Average Candle Power.	Average Watt per Candle.	Energy Supplied (Watt Hours).	Candle Hours.	Breakages, etc.
1	15·8	57·44	2·08	1,912	919	Top contact of glower fused.
2	26·0	53·47	2·11	2,925	1,390	Top contact of glower fused and ballasting resistance fused.
3	38·0	54·14	2·22	4,680	2,080	Same as 2.
4	42·8	57·00	2·00	4,870	2,450	Bottom contact of glower fused.
5	52·0	58·86	1·92	5,872	3,060	Top contact of glower fused.
6	61·0	65·00	1·70	6,750	3,960	Glower broke.
7	61·7	54·78	2·09	7,072	3,380	Bottom platinum wire contact fused.
8	135·5	53·20	2·04	14,700	7,200	Same as 7.
9	147·0	48·70	2·03	16,500	7,160	Same as 7. Also iron resistance fused.
10	Alive at 220·0	43·18	2·38	22,641	9,500	Glower still alive. Iron resistance fused.

[redacted]

No. Lamp and Curve.	Hours. W.P.C.	Average Watts per Candle.	Energy supplied K.W.-Hours.	Candle-Hours Total.
1 A	5·45	4·71	66·60	14,050
2 A	5·76	5·25	59·20	11,300
3 A	7·20	6·71	67·70	10,080
19 B	—	4·25	48·00	11,360
20 B	—	4·26	56·10	13,130
4 A	6·40	5·18	133·50	25,700
5 A	5·90	5·04	129·50	25,700
6 A	6·50	5·40	138·50	25,600
17 B	—	5·08	108·00	21,350
18 B	—	3·76	24·30	6,440
7 A	5·40	4·61	60·30	13,040
8 A	5·42	4·80	67·90	14,160
9 A	5·47	4·75	52·40	11,050
11 B	—	3·48	15·25	4,390
12 B	—	3·90	54·20	13,900
10 A	6·50	5·34	124·00	23,180
11 A	7·21	5·92	131·00	22,020
12 A	6·76	5·50	124·50	22,680
9 B	—	4·30	11·10	2,570
10 B	—	4·00	100·00	26,450
13 A	5·08	4·36	57·10	13,120
14 A		4·08	23·50	5,770
15 A	5·10	4·50	55·00	12,260
3 B	—	4·98	49·70	9,950
4 B	—	4·15	48·60	11,720
16 A	6·08	5·16	122·10	23,630
17 A	6·10	5·08	126·00	24,800
18 A	6·60	5·41	122·70	22,600
1 B	—	5·43	105·00	19,320
2 B	—	4·68	109·20	23,320
41 A	4·97	4·60	55·00	12,000
42 A	5·84	5·60	53·80	9,600
27 B	—	8·30	8·10	970
28 B	—	3·60	49·00	13,120
44 A	6·65	6·00	126·00	21,000
45 A	6·10	5·85	118·20	20,200
46 A	6·10	5·30	135·00	25,500
25 B	—	4·90	99·90	20,370
26 B	—	5·24	100·00	19,150

No. Lam and Curv	o Hours. W.P.C.	Average Watts per Candle.	Energy Supplied K.W.-Hours.	Candle-Hours Total.
19 A	6·73	5·90	129·5	21,900
20 A	6·12	5·34	125·2	23,400
21 A	5·47	4·65	111·1	23,800
21 B	—	5·81	114·5	19,670
22 B	—	4·77	108·0	22,640
22 A	5·72	5·30	53·0	10,000
24 A	5·25	4·80	62·6	13,060
23 B	—	5·23	55·0	10,550
24 B	—	4·22	50·9	12,000
25 A	5·90	5·00	57·6	11,400
26 A	6·70	5·70	55·1	9,690
27 A	6·38	5·43	58·5	10,760
7 B	—	5·60	52·2	9,280
8 B	—	5·20	48·7	9,460
28 A	6·30	5·82	106·0	18,200
30 A	8·65	8·73	107·5	12,300
5 B	—	4·42	99·0	22,390
6 B	—	4·32	97·1	22,540
31 A	5·54	4·75	65·8	13,840
32 A	5·37	4·70	64·5	13,690
33 A	5·92	5·10	64·5	12,650
15 B	—	6·00	55·8	10,000
16 B	—	4·50	51·0	11,370
34 A	5·96	5·14	115·0	22,300
35 A	6·03	5·08	114·5	22,550
36 A	6·80	5·50	116·5	21,100
13 B	—	5·20	96·0	18,500
14 B	—	4·80	96·5	18,700

Comparison of Results of Tests, Part I.—A comparison of the average results of Tables I. and II. shows a saving of 57 per cent. in watts per candle in favour of the Nernst lamp. The percentage saving in cost is much less than the percentage saving in energy on account of the frequent and expensive renewal of glowers and iron resistances.

Against the saving in cost we must place—

1. The much higher capital outlay on the lamp.
2. The large sizes in which it is manufactured.
3. The time it takes to light.
4. The erratic life of the glowers.

In reading the above figures it must be remembered that these tests are made with alternating currents, and that the Nernst lamp appears to work much better with continuous current.

Part II.

These tests were divided into three series :—

1. Lamps run on their constant normal voltage (230). Duration of test, 1,000 hours.

2. Lamps run on a constant voltage (240), about 5 per cent. above normal. Duration of test, 750 hours.

3. Lamps run on a voltage varying continually between 230 and 240 volts, the variation having a period of two minutes.

The lamps were run continuously from Monday until Saturday, a period of 120 hours at a time.

Some of the results obtained are represented graphically by the curves in the accompanying diagrams, and the average values of candle power and watts per candle, together with figures for energy supplied and candle hours, are given in Tables III., IV., and V.

The curves are arranged in two sets, A and B. The A set includes the lamps tested at a constant pressure of 230 volts, and the B set the lamps on a constant pressure of 240 volts, also on a pressure varying between 230 and 240 volts. Curves for lamps of one make will be found together on the same diagram, and the figures corresponding to these are tabulated together.

In the first series of tests six lamps, three 16 c.p. and three 32 c.p., of each kind were tested, except tantalum lamps, in which case two 115-volt lamps were placed in series. The Nernst lamps used were 245 volts, 0·25 ampere, this being the usual type of lamp supplied for use on the 230-volt mains in Liverpool. The Nernst lamps were marked 225 volts on the filament and 20 on the ballasting resistance, so that it would appear that they should be run on 245 volts, and it was found that they gave better results on the 240 than on the 230-volt circuit, yet they were the lamps sold for use on the 230-volt mains at Liverpool.

In the second and third series of tests two lamps, one 16 c.p. and one 32 c.p., of each make were employed.

In all cases the frequency of supply was 50.

TABLE V.

TANTALUM LAMPS.

2nd 250 Hours. C.P.	2nd 250 Hours. W.P.C.	3rd 250 Hours. C.P.	3rd 250 Hours. W.P.C.	4th 250 Hours. C.P.	4th 250 Hours. W.P.C.	Average Watts per Candle.	Energy Supplied K.W.-Hours.	Candle-Hours Total.
17·4	2·38	Broke	at 395	hours.		2·08	15·7	7,550
17·3	2·31					2·18	16·0	7,300
Broke	at 235	hours.				1·61	10·6	6,540
						2·00	9·9	4,970
Broke	at 368	hours.				1·60	17·7	11,050
						2·35	14·7	6,260

ERNST ¼-AMPERE LAMPS.

2nd 250 Hours. C.P.	2nd 250 Hours. W.P.C.	3rd 250 Hours. C.P.	3rd 250 Hours. W.P.C.	4th 250 Hours. C.P.	4th 250 Hours. W.P.C.	Average Watts per Candle.	Energy Supplied K.W.-Hours.	Candle-Hours Total.
6·90	5·60	6·70	5·84	Broke at	690 hrs.	5·38	27·2	5,040
9·45	4·15	8·35	4·50	7·15	4·8	4·30	36·5	8,500
11·05	3·90	Only tested	480	hours		3·73	19·2	5,150
Broke at	250 hours.					4·55	10·4	2,210
9·36	4·20	8·80	4·30			4·11	29·2	7,050
11·10	4·12	8·90	5·00			4·12	37·0	9,130
9·80	4·30	Broke at	560 hours.			3·84	27·2	6,830
Broke at	39 hours.					3·10	1·8	590

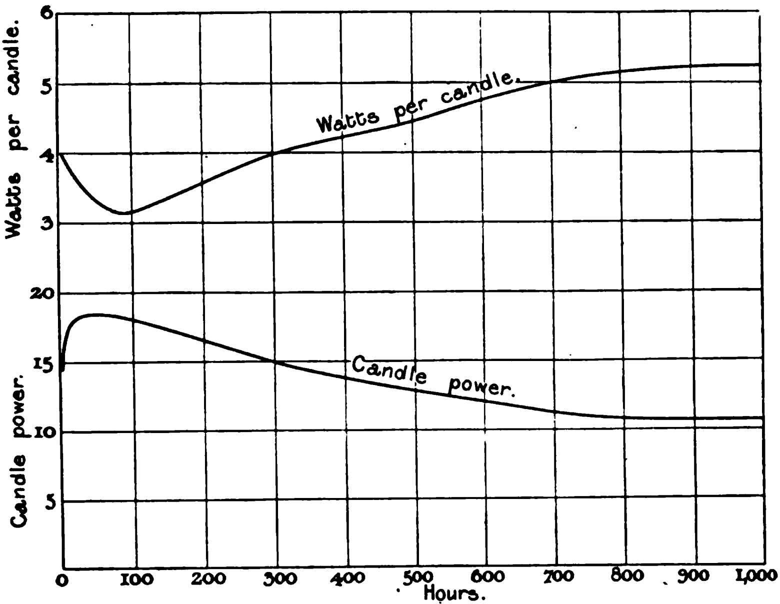


FIG. 9.—Carbon Lamp 13 A. Volts constant, 230.

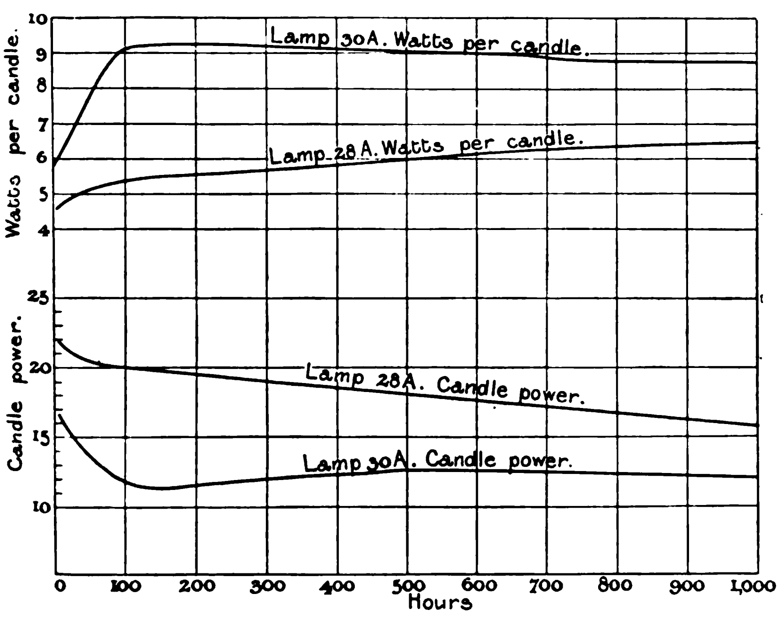


FIG. 10.—Carbon Lamps 28 A and 30 A. Volts constant, 230.

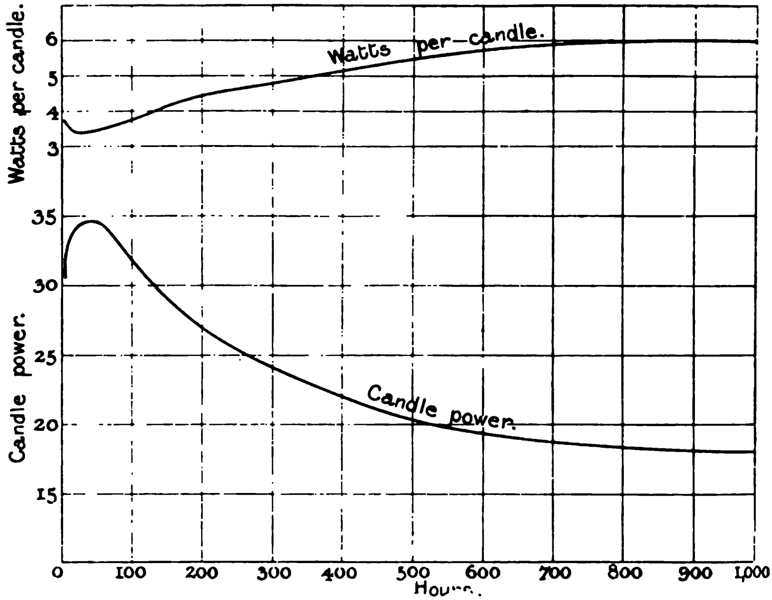
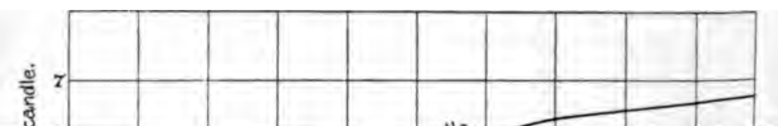


FIG. 11.—Carbon Lamp 34 A. Volts constant, 230.



[redacted]

General Conclusions.—In Table I. the voltage across the glowers was also measured. The glower voltage always increases with time.

In case	2	it rose from	195·0 to 208	volts in	26	hours	= $\frac{1}{2}$	volt per hour.
"	3	"	202·0 to 212	"	43	"	= $\frac{1}{4}$	"
"	6	"	201·0 to 216	"	60	"	= $\frac{1}{4}$	"
"	8	"	199·0 to 217	"	135	"	= $\frac{2}{15}$	"
"	9	"	200·0 to 207	"	147	"	= $\frac{1}{21}$	"
"	10	"	201·5 to 216	"	220	"	= $\frac{1}{15}$	"

Now this increase of voltage on the glower is not due to any ageing of the ballasting resistance, for when a new resistance was put in in the middle of a run due to failure of the previous one, this increase of voltage was maintained, and there was no initial reduction of voltage due to the new resistance.

This increase in resistance of the glower is therefore due to alteration in the glower itself and to deterioration of its metallic contacts, and it is interesting to note that the faster the resistance of the glower increases the shorter its life.

Referring to the tests on carbon lamps, the following points may be mentioned :—

The 4 watts per candle lamp is generally the exception on alternating-current lamps.

The cost of an incandescent lamp is an extremely small item in the cost of lighting compared with the expenditure for energy, and should not be considered in buying a lamp.

The point to be considered is, What average watts per candle will the maker *guarantee?*

In the majority of cases when used with alternating currents incandescent lamps do not give their proper candle power, and usually no two lamps made and calibrated by the same maker will agree either with themselves or with their reputed candle power. Also with some makers the small size lamps are good and the large ones very bad.

In nearly all cases there was a rise in candle power after the beginning of the run.

With carbon and tantalum lamps the maximum was obtained within 25 to 30 hours, while with the Nernst lamps the rise was evident, but not so appreciable as with the former.

The current also rose to a maximum, but reached this value later.

In the case of the higher voltage tests these maxima were reached earlier.

The lamps of one make in which the candle power attained the highest value in their group often fell off most rapidly, and were the lowest in value at the end of the test (see Curves).

Breakages generally took place when, after a long run, the lamps were switched off on Saturday for the week-end. This would seem to be due to the contraction during cooling, the filament breaking when it had become weakened by long burning.

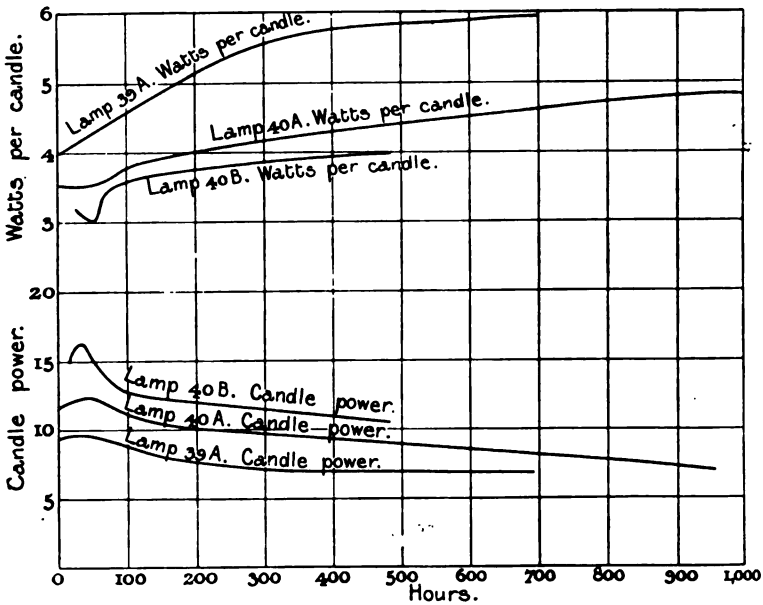
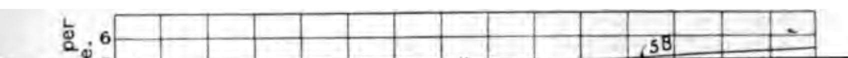


FIG. 13.—Nernst Lamps 39 A, 40 A and 40 B. Volts constant, 230. (39 A and 40 A, Opal globes; 40 B, Clear globe.)



[redacted]

The lamps tested were Brush-Vienna, B.T.-H. Edison, Ediswan, Nernst, Pope, Premier, Robertson, Sunbeam, Maxim, Behrend, and Tantalum.

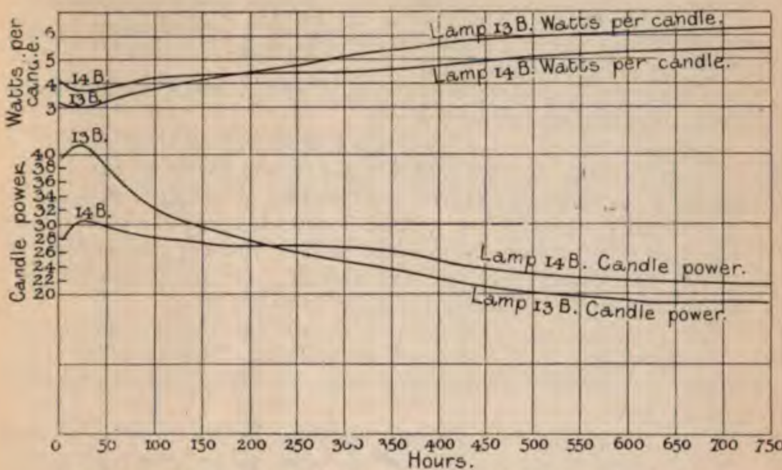


FIG. 15.—Carbon Lamps 13 B and 14 B. 13 B, Volts constant, 240 ; 14 B, Volts varying, 230–240.

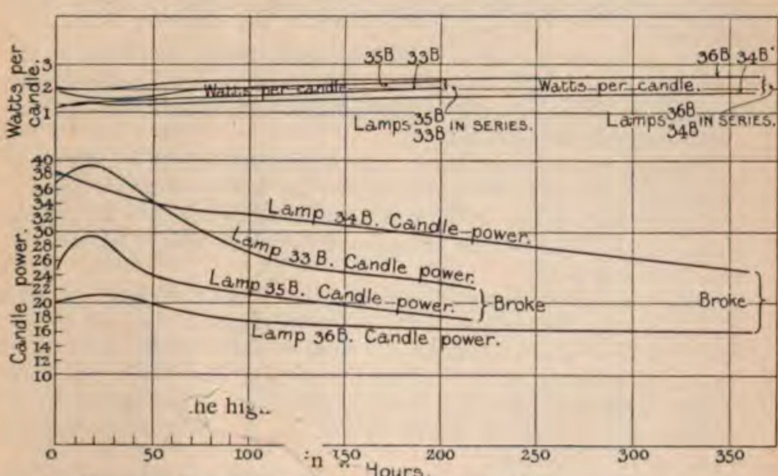


FIG. 16.—Tantalum Lamps [illegible] B and 35 B, Volts constant, 240 ; 34 B and 36 B, V[illegible] varying, 230–240.

Owing to the great number of tests only a few typical curves are given, and the chief details of the original curves are given in the table.

CURVES.

Diagram, Fig. 9.—Curve 13 A is typical of a carbon lamp run at normal voltage.

Diagram, Fig. 10.—Curves 28 A and 30 A are of bad lamps.

Diagram, Fig. 11.—34 A is characteristic of a class of lamps which start very well and finish badly.

Diagram, Fig. 12.—44 A is a typical 32-c.p. lamp, starting about 23 c.p. and depreciating very slowly.

Diagram, Fig. 13.—39 A, 40 A, and 40 B are Nernst lamp curves.

Diagram, Fig. 14.—5 B and 6 B are typical curves of two similar lamps, one overrun at 240 volts and the other on voltage varying from 230 normal to 240 volts. It will be noticed that the two curves cross one another about half-way through the test; this was characteristic of most of the curves made in this manner. Generally speaking the efficiencies work out nearly the same in both cases.

Diagram, Fig. 15.—13 B and 14 B are also illustrative of this point.

Diagram, Fig. 16.—33 B and 35 B are tantalum lamps overrun at 240 volts; 34 B and 36 B are tantalum lamps on voltage varying between 240 and 230.

The average of "the average watts per candle" for the seventy carbon lamps tested is 4·86; for the eight ¼-ampere Nernst lamps tested this average is 4·14, and for the six tantalum lamps 1·97.

The ordinary ¼-ampere Nernst lamp of commerce is thus about 15 per cent. better than the average carbon lamp, while its life is about 560 hours.

The average consumption of the tantalum lamps tested was 60 per cent. less than that of the carbon lamps, and their lives were on an average 330 hours; doubtless the tantalum lamps made now will give much longer lives than these.

Though the tantalum lamps, even on these few tests, show a much better efficiency than the carbon lamps, the makers of carbon lamps—feeling the competition of the new metal filament lamps—will, without doubt, vigorously turn their attention to the production of a 2-watt per candle carbon lamp.

This research was carried out at the University, Liverpool; the first part, including the design of the automatic regulator, the preliminary tests, results of which are given in Tables I. and II., being carried out by H. F. Haworth in the old electrotechnical laboratories; and the second, including Tables III. and onwards, was carried out by T. H. Matthewman and D. H. Ogley in the new electrotechnical laboratories of the University.

In conclusion, the authors wish to express their sincere thanks and indebtedness to Professor Marchant for his encouragement and valuable help during the progress of the above work.

Discussion.

Professor Ayrton: I would like to ask Dr. Haworth what was the sensibility of his regulator. That was not mentioned in the paper. We know that to make an alternate pressure regulator is a much more difficult problem than to construct a direct pressure regulator, because as a rule the forces to be dealt with are so much smaller. Mr. Paterson made a regulator, which is described in his paper, which kept the pressure constant to ¼ per cent. I should therefore like to know what the sensibility of Dr. Haworth's regulator is. Professor Ayrton.

Mr. C. C. Paterson: I think Dr. Haworth has given us particulars of a number of very interesting and valuable tests in his paper, but I should like to ask one or two questions. First of all, how was the candle power of the Nernst lamps determined for the purpose of these comparisons? Was the maximum spherical, or hemispherical candle power taken? On the last page the author draws a comparison between the results of efficiency tests on carbon lamps and on Nernst lamps. One always finds these comparisons on the basis of candle power very difficult to make, because the Nernst lamp throws nearly all its light in one direction. As regards the comparison shown in the curves in figs. 14 and 15, where tests are made on fluctuating and steady voltages, I should like to point out that in Fig. 15 the author has plotted two life curves of lamps, one of which has an inefficiency of 4·1 watts per candle power, and the other 3·2. Those two lamps under normal conditions would have a very wide variation of life. The curves in the other case are marked 3·8 watts per candle, and although one has a very slight rise, which would increase its efficiency rather more than the other one, yet they are probably comparable. Mr. Paterson.

In his table the author gives figures for the length of life of different lamps, but I should be interested to know if he could give us any information as to the length of life of the lamps of different makers run at the same efficiency and on the two voltages which he mentions. He made some tests at 230 and some at 240 volts. If we could get comparative tests between lamps of the same efficiency and of different makes run at those two voltages, they would be exceedingly valuable, because it would help us in the problem of shortening life tests and finding out whether it is possible to run lamps at a higher efficiency than that for which they are rated and deducing from the results what would be the life if run under normal conditions.

Mr. J. T. Morris: With regard to the cause of the shortened life of tantalum lamps on alternating circuits as compared with direct circuits, it seems probable that this is due, as I have elsewhere suggested, to cyclic changes in the length of the filament during each period of the alternating current. While the lamp is at work it is known that the candle power fluctuates during each cycle, and in consequence the filament must expand and contract. I imagine the breaking is comparable to that of a bar which is subjected to repeated alterations of stress. Mr Morris.

Mr. W. H. Patchell: With regard to the point raised by Mr. Paterson, I should like to say that, when Dr. Sharp was showing me Mr. Patchell.

Mr. Patchell. over the Electrical Testing Laboratories in New York, I distinctly gathered from him that he could tell the life of a lamp by overrunning it. He had made so many test curves that he could definitely fix what the life of a lamp under ordinary conditions would be if he raised the testing pressure. On the question of tantalum lamps, it is useful, when working on a subject, to look from one paper to another; and people who are looking through the discussion afterwards may be glad of a reference to the National Electric Light Association's Convention, held at Atlantic City in June of last year, the *Proceedings* of which have been published. J. W. Howell's paper, read before the American Institute of Electrical Engineers, has already been referred to by the author. In the *Proceedings* of the Atlantic City Convention there appeared a photograph of the tantalum lamp filament, showing that it looks like a child's bead necklace, as was shown by Professor Thompson, and also a month or two earlier by Dr. Sharp. I believe the peculiar structure was published for the first time at the Atlantic City Convention.

Mr. Campbell. Mr. A. CAMPBELL: With regard to the very interesting effect that Dr. Haworth mentions in connection with the tantalum lamp, that is to say, the shivering or shuddering that occurs at times, is he quite sure that it has nothing to do with anything in the rest of the circuit? I take it that the circuit was used exclusively for running the lamps from the alternator itself, and therefore that he has good proof that there was no high-frequency oscillation set up by external capacity and inductance. If it is something quite independent of the circuit it seems to me that it may be connected with the effects of crystallisation, such as were shown by Dr. Silvanus Thompson's microphotographs. Perhaps Dr. Haworth can throw some further light on it.

Mr. Sparks. Mr. C. P. SPARKS: There is one fact I should like to mention, namely, the bearing of price on the deterioration in the quality of lamps supplied during the last few years. We now expect to get for a very low figure a better lamp than we did years ago, when the high price allowed a larger percentage of lamps being rejected. If the public want a better lamp, it can be produced at a higher cost, but the cheap lamp seems to suit the British public at the present moment. You cannot expect to get the same results from these cheap lamps that were obtained with more expensive lamps a few years ago.

Mr. Gaster. Mr. LEON GASTER: I think that the results obtained by the authors of this paper confirm those obtained by other investigators, namely, that the present high-voltage carbon glow lamps bought in the open market are of very unsatisfactory quality, and leave plenty of room for improvement. There is no doubt that the excessive variation of voltage existing in the supply circuits has a good deal to do with the unsatisfactory illumination obtained. The automatic voltage regulator used by the authors for making the life test is a very useful apparatus. I should venture to think that there ought to be a field open for large consumers of light to install practical automatic voltage regulators, by the aid of which not only would the voltage be kept steadier, but the

Mr. Gaster. conditions would be better for the employment of higher efficiency carbon glow lamps, with the result that great economies could be secured. In the ordinary way, on account of the prevailing great voltage fluctuations, such high-efficiency lamps cannot safely be used without being exposed to damage by excessive overrunning, thereby shortening their life considerably.

Professor Marchant. Professor E. W. MARCHANT: In the first place I should like to say something about the pentane lamp, if I may be allowed to do so. It was referred to in Mr. Paterson's paper. We made a series of very careful comparisons between an Edison large bulb standard and a pentane lamp. I have here the result of nineteen tests, made over a length of time of nearly seven months, and the actual variation in the relation between the candle powers was from 1 : 1·92 to 1 : 1·97. Those are the outside limits of variation, and they seem to be well within the limits that one would expect to get, taking account of the variation in humidity that one does get in the air. The results have not been corrected in any way for humidity. These tests were made in a large photometric room, with a cubic content of something like 4,500 cubic feet, and with a constant temperature of about 18° C., and I think it is possible these conditions may be an explanation of the result. With reference to the question of voltage regulation, which has been referred to a good deal this evening, I should like to lay stress on the excellence of Dr. Haworth's regulator. I think a regulator that will regulate to plus or minus ⅛ per cent. is really a very good regulator indeed, particularly when it is one that will operate on alternating-current circuits. It is an instrument which is really a practical instrument, one which may be of service in connection, for example, with large buildings where there is a large number of lamps in use. It might add considerably to the life and efficiency of lamps in a large building if a regulator of this kind were installed, even though it meant that a certain amount of energy was lost in the resistance it would be necessary to put in to maintain the constant pressure. With reference to the point Mr. Wilson made as to the variation of voltage on the electrical circuits in this country, I should like fully to bear out his remarks with reference to Liverpool. I do not think any one can describe the regulation of Liverpool as good. We very often have a pressure as high as 245 volts on a circuit which should give 230, and sometimes our pressure will fall to as low a value as 220. I do not know whether the lamps that Mr. Wilson may have sent to Liverpool were graded to suit the Liverpool conditions, but if so it is just possible that the results which were obtained were not so good as they would have been had they been fitted for the actual conditions that existed in our tests. Professor Ayrton has just told me, with reference to the carbide of silicon lamps to which he referred, that the manufacture of them in Liverpool was discarded on account of the large fluctuations in pressure that were found on the circuits there—that it was practically no good for the lamp makers to make good lamps (and these, of course, are extremely good lamps) if the variation in pressure was so bad as to

Professor Marchant.

neutralise entirely (by shortening the effective life) any advantage that could be obtained by increasing the efficiency of the lamp. I think the lamp makers have something to say to the central station engineers, but, on the other hand, I think the central station engineers have a good deal to say to the lamp makers. No one can describe the result of these tests, I think, as satisfactory. The tests were made entirely from the point of view of the consumer. The attempt was made to gain as much information as possible as to what the carbon incandescent lamp was doing at the present day, and it was for that reason that the tests were made in the way in which they have been carried out—that is, the tests were made at 230 and 240 volts, and with the actual varying pressure that one gets on the circuit. As Mr. Paterson has said, it is extremely difficult in making tests in this sort of way to generalise at all as to the results. The results are there, and they are available for reference. I should like very much to work out these results in view of the curves and figures which have been given us by Mr. Paterson. I should like to see, for example, how his life curve, which has been based, I understand, on the result of a large number of tests, agrees with the results that we have obtained in this series of experiments.

We have not had sufficient time yet to check our own results against his in regard to various other matters, but I hope that may be done before long.

Mr. Patchell.

Mr. W. H. PATCHELL: I understand the Liverpool works are owned and run by the ratepayers. I believe they also carry an enormous tram load in addition to the lighting load, so that if they are carried for nothing they cannot expect to have their volts regulated to ½ per cent.

Dr. Haworth.

DR. HAWORTH (*in reply*) said: There is only one point to which I wish to refer with regard to those curves (Fig. 15, 13 B and 14 B) which cross over the 240 volts over-run and the 240 to 230 volts. Mr. Paterson wanted to know why they were not over-run at the same efficiency. As a matter of fact, those curves referred to lamps which were supposed to be similar lamps supplied by the same maker. On each sheet of curves the lamps referred to are made by one maker, and those two curves are lamps of one maker. One of them, the 240 volts over-run, started at about 3·9 and went up to 6·2 watts per candle. The varying voltage lamp started with higher watts per candle, 4·25, and it rose to a smaller amount than the other did, namely, 5·3 watts per candle. But the average watts per candle over their total life are nearly the same. The over-run lamp has 5·2 watts per candle, and the varying voltage lamp 4·8. I do not know what the other curves would work out to, but I have just taken the two shown on the screen. The lamps were run at constant voltage and not started at the same efficiency. With regard to the question Mr. Campbell raised as to the quivering of the filament, the circuit we had was used exclusively for these lamp tests; we had nothing else on it.

In reply to Professor Ayrton's question, the sensibility of the regulator, working under the best conditions of contact, was plus or

ıus ⅛ per cent., and working under normal conditions it was about s or minus ¼ per cent. Dr. Haworth.

The PRESIDENT: I will ask you to convey your thanks to Dr. worth and the other authors for the paper they have been good ıugh to present. The President.

The resolution of thanks was carried with acclamation.

Proceedings of the Four Hundred and Fifty-first Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, February 7, 1907, Dr. R. T. GLAZEBROOK, F.R.S., President, in the chair.

The minutes of the Ordinary General Meeting held on January 24, 1907, were taken as read, and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

TRANSFERS.

From the class of Associate Members to that of Members :—

William W. Cook. | Cecil W. Kennaway.
John S. Peck.

[redacted]

As Students.

Ernest Adamson.
Cyril Dean Adshead.
James Affleck.
Leonard Cecil Baldwin.
Cecil Shannon Bell.
John Bilsland.
Francis W. Bissett.
Francis William Brecknell.
Norman Elling Brevig.
Charles Graham Campbell.
Frederick Henry Clark.
Harold Cooper.
Frederick Augustine Corea.
John James Creasey.
John Davidson.
Rex Marsh Everett.
Fred. Farrar.
William Edward Flower.
Theodore Gerrard.
Reginald Alfred Giffen.
Thomas Carlyle Grisenthwaite.
Douglas Hay.
William Henderson.
Frank Reginald Hoggett.
John Morton Jackson.
Hermon Amos Kelsall.
Allen Neave Kingsbury.
James Charles S. McIntyre.
Duncan John MacKellar.
Reginald St. Clare Marston.
Arthur Wellesley Mason.
Hugh Charles May.
Kurt John Nebel.
Edward Rowden Newman.
Kenneth Lancaster Osborne.
John Robert Pannell.
Archibald Parker-Smith.
Leslie Orr Gibb Pearce.
John William Perkins.
Charles Sydney Richards.
George William Richmond.
Archibald Stuart Ross.
Ernest Walter Sale.
Jacques André Schopfer.
Sooyii Kwang Shen.
Maurice E. F. Shuttleworth.
Robert Rowley Smail.
Herbert John Swain.
Victor H. Winson.
Edward L. Wood.

Donations to the *Library* were announced as having been received since the last meeting from The American Philosophical Society, Audel & Co., C. Barnes, Professor J. Epstein, M. O'Gorman, Dr. A. E. Kennelly, Sir Oliver Lodge; to the *Building Fund* from J. H. Rosenthal and L. C. B. Trimnell; and to the *Benevolent Fund* from H. Alabaster, S. E. Britton, H. W. Clothier, J. Gavey, D. Henriques, S. Insull, H. W. Kolle, J. P. Lawrence, Sir H. C. Mance, H. W. Miller, W. G. T. Pope, C. Stirling, H. W. Turner; to whom the thanks of the meeting were duly accorded.

The discussion on Mr. C. C. Paterson's paper was concluded (see page 318), and Dr. H. F. Haworth's paper was read and discussed (see p. 350).

The meeting adjourned at 9.45 p.m.

LEEDS LOCAL SECTION.

REGENERATIVE CONTROL OF ELECTRIC TRAMCARS AND LOCOMOTIVES.

By Alfred Raworth, Associate Member.

(*Paper read at Sheffield November* 22, 1906.)

During the last two years the attention of all tramway authorities has been turned towards the subject of regenerative control. In some cases their interest has been awakened by the high prices they have to pay for electrical energy, and in others by the difficulty of providing effective braking appliances. It is generally admitted that if a practical system of regeneration could be evolved, it would go far towards the removal of both difficulties.

Actual experience has shown that even on level lines regeneration produces a saving in current of over 10 per cent., and wherever the country is undulating from 20 to 30 per cent. may be expected. As a brake the regenerative motor is unsurpassed, because the retarding effort, whilst very strong, is completely under control and is free from objectionable jerks.

Many of the advantages of regeneration were clearly foreseen by Sprague as early as 1885, as will be gathered from the following extract from an article written by him* on his early experiments :—"Meanwhile my interest in the electric railway problem had become active, and after a study of the movement of trains and the conditions of operation upon the Manhattan Elevated Railway, I schemed out a system, and in December, 1885, read a paper before the Society of Arts in Boston, advocating an equipment with motors under each car, and using shunt-wound machines to enable current to be returned to the line when decreasing from the higher to more moderate speeds."

In those early years, when the attention of engineers was being directed towards the practicability of electric traction, Mr. W. M. Mordey was also very strongly impressed with the advantages to be gained by the use of the shunt motor, and there can be no doubt that Sprague, with his intimate knowledge of electrical theory, and with his

* *Street Railway Journal.*

great ingenuity and indomitable energy, would have solved the problem, had it not been for the inherent defects of motors as they then existed. At the time of Sprague's experiment no motors would run with a fixed line of commutation, and the position of the brushes had to be changed when the motors were reversed. This condition was, of course, fatal to any proposition for producing regeneration.

Later attempts appear to have been only of a desultory character, and there is no evidence that any inventor ever attempted to produce a field by means of a shunt winding which would be commensurate with the field produced by the ordinary series winding. So long as they omitted to do this their failure was a foregone conclusion.

At the present time there are some two or three examples of regenerative working existing in Germany. The motors, I understand, are pure shunt, but no attempt whatever is made at speed regulation, and the lines upon which they are working are not street tramways.

The first attempts in the direction of developing the present system of automatic regenerative control were made at Devonport in the autumn of 1903. The result of the experiments was to show that the system was perfectly practical even without the advantage of putting the motors in parallel. The range of speed, however, was somewhat limited, a defect which was particularly noticeable in hill climbing; consequently with a view to removing this, attempts were made to devise a series parallel controller, which at first resulted in failures, but so much valuable experience was gained that a satisfactory controller was eventually produced and is now in operation on some forty-five cars.

On the whole it is a happy circumstance that the earlier development was done entirely with motors in series, otherwise the difficulties of the problem would, in all probability, have stifled the invention altogether.

The difficulty of changing from series to parallel or *vice versâ* with shunt-wound motors, consists principally in the fact that the speed of the armature varies inversely as the strength of the field and directly as the voltage across the brushes. It is obvious, therefore, that in changing from series to parallel the field strength must be practically doubled, otherwise the moment the armature circuit is closed across 500 volts a violent attempt will be made by the motors to accelerate the car.

The operation of strengthening the field can be carried out fairly quickly, but not quite quickly enough, consequently it is necessary to insert some resistance in series with the armatures to check momentarily the rush of current due to the inexactitude of the field strength. This will be referred to later. It is also necessary to insert some series field winding in series with each armature, for without this precaution the motors would not work in parallel.

Further it is an essential condition that in breaking the current either when going out of series or when coming out of parallel, there should be no resistance in circuit, otherwise there will be heavy flashes in the controller. In this respect shunt motors differ entirely from

series motors, resistances being necessary in combination with these latter when opening the circuit.

The method by which the resistances are put in circuit before closing the armature circuit and taken out again before breaking the circuit will be described later on. Apart from this little difficulty, the operation of this controller is simplicity itself.

It first puts on the shunt field, then closes the armature circuit through resistances which are cut out in steps, it then weakens the field to increase speed, it then doubles the field strength and puts the armatures in parallel, a small series winding being in series with each armature and resistance in the main circuit. This resistance is then cut out in steps and the field again weakened to attain the top speed.

The series winding has nearly as many turns as the ordinary series winding of a series motor, but the current is shunted so that the series winding carries only a fraction of the current passing through its own armature.

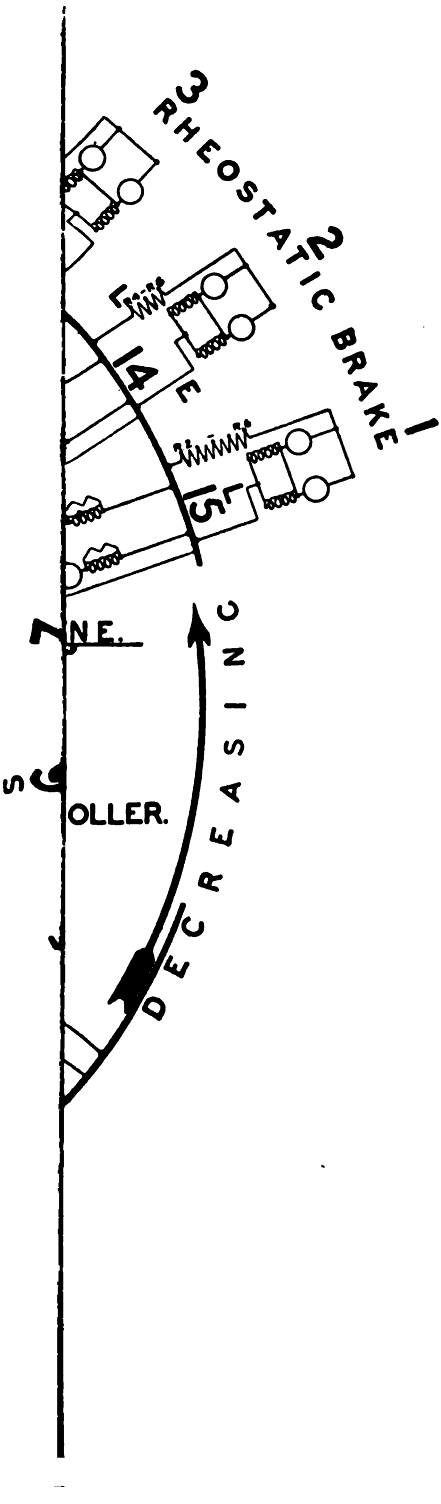
The series of operations is shown in Fig. 1, which is a diagram showing the connections on each notch of a series parallel controller arranged for working two motors. Referring to this diagram, it will be seen that the shunt field circuit is first closed, then on the first notch the armatures are connected in series with resistance. The resistance is then cut out in steps, and on notch No. 4 the armatures are in series across the full voltage, the shunt fields being fully excited. This notch gives a speed of from 3½ to 6 miles an hour, according to the type of motor used. But it is an advantage to have the minimum reactive speed as low as possible, both for the sake of economy in current due to the reduction of the rheostatic period and because it is the minimum speed at which the motors will return current to the line.

On notches 5 and 6 resistance is inserted in the shunt field circuit with the object of increasing the speed. The speed on the sixth notch being 3 or 4 miles an hour faster than the minimum reactive speed.

On notches 7, 8, and 9, when the controller handle is being turned in a clockwise direction, the connections are exactly the same as on notch 6. Thus, when the circuit is opened preparatory to going into parallel, there is no resistance in the armature circuit.

On notch 10 all the resistance in series with the shunt fields is cut out, thus giving the maximum field strength, the armatures are connected in parallel, each in series with its series field winding, and resistance is inserted in the main circuit. A resistance is put in parallel with the series windings in order to shunt a portion of the current, therefore only so much current as will balance the load between the two armatures is allowed to pass through the series winding.

On notches 11 and 12 the resistance in the main circuit is cut out in steps, and on notch 13 the armatures are in parallel across the full voltage, the field strength being at the maximum.



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DECREASING

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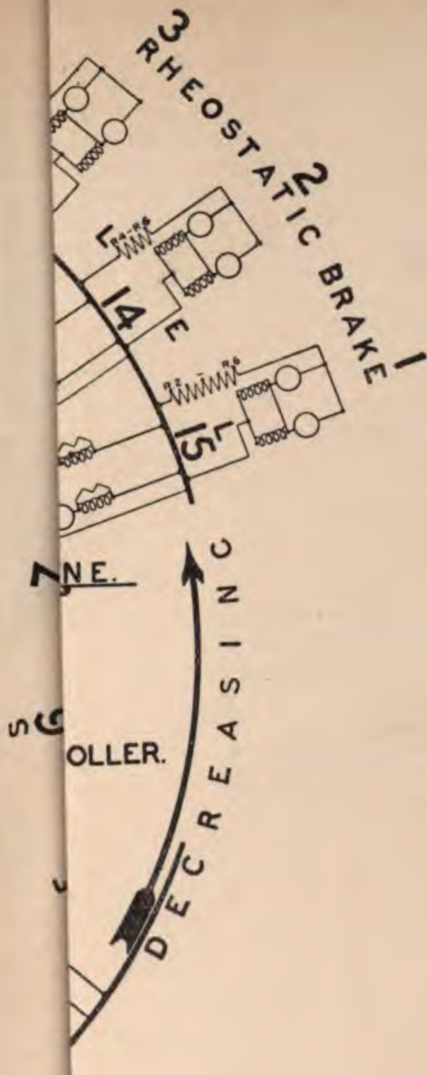

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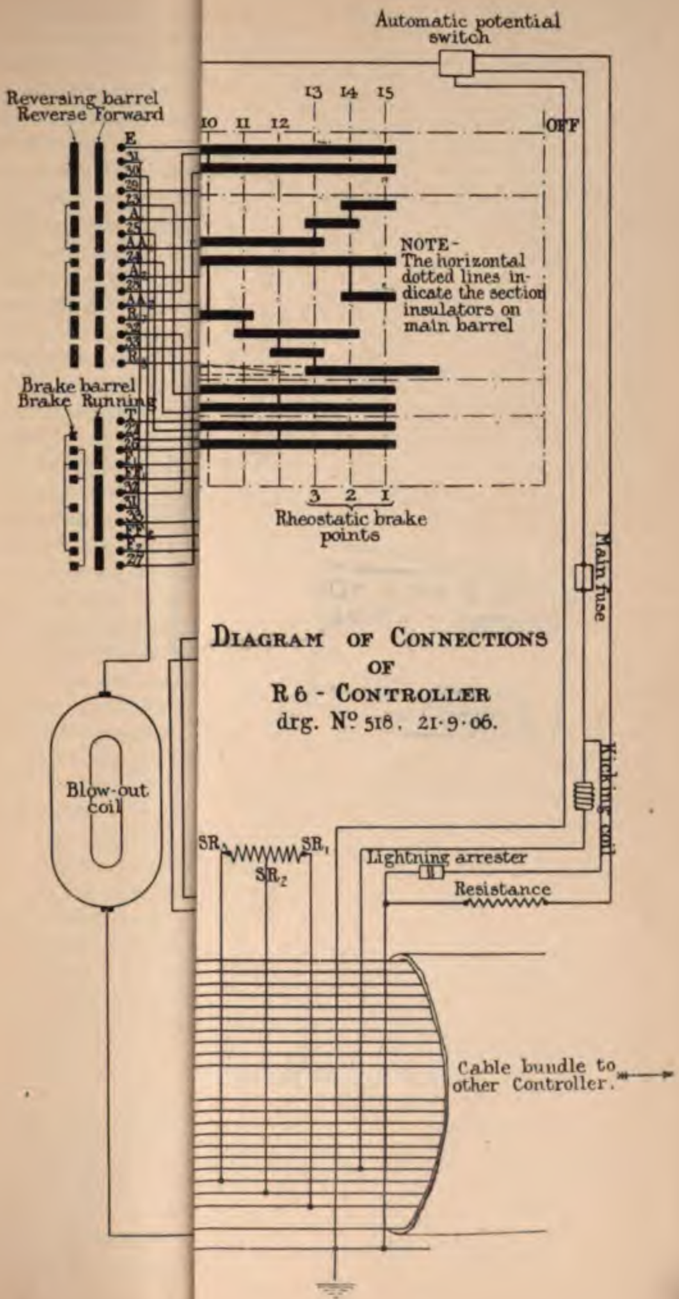

DIAGRAM OF CONNECTIONS OF R 6 - CONTROLLER drg. No. 518. 21·9·06.

On notches 14 and 15 resistance is inserted in steps in the shunt field circuit to attain the top speeds.

Now when the controller handle is turned in a counter-clockwise direction, the same combinations as when accelerating are made as far as notch 13, and the speed reduced from the maximum down to a speed which is only slightly more than double the minimum regenerating speed in series.

But on notches 12, 11, and 10 the connections are exactly the same as on notch 13, and when the circuit is opened preparatory to going into series, there is thus no resistance in series with the armatures.

On notch 9 resistance is connected in series with the shunt field windings and the armatures are connected in series, in series with resistance.

On notches 8 and 7 this resistance is cut out in steps until on notch 6 the armatures in series are connected across the full voltage.

On notch 5 the resistance in series with the shunt fields is reduced, and on notch 4 it is cut out altogether.

At this point the speed of the car has been reduced to the minimum regenerating speed. The handle is then shut off in the usual manner and the hand-brake applied. But when moving the handle from notch 4 to the off-position, the resistance which was inserted on notches 1, 2, and 3, when moving in a clockwise direction, is kept short-circuited, thus again providing for the opening of the circuit with the armatures connected across the full voltage.

The rheostatic brake notches 1, 2, and 3 make connections similar to those used for the same purpose in ordinary controllers, but these notches in the controller coincide with the power notches 15, 14, and 13. How this is effected I will now explain :—

There are also two other points which require explanation :—

(1.) Notches 1, 2, and 3, 10, 11, and 12 make connections as shown on the diagram when the controller handle is moving in a clockwise direction, but when the handle is turning in the opposite direction the resistance in series with the armatures is kept short-circuited.

(2.) Notches 7, 8, and 9 give connections as shown in the diagram when the handle is moving in a counter-clockwise direction, but the resistance in series with the armatures is kept short-circuited when the handle is moving in a clockwise direction.

Referring now to Fig. 2, which is a development of the controller laid out in the usual way: Finger *22* is connected direct to the trolley and the six fingers immediately below it are connected to the resistance used in the armature circuit. When the controller cylinder connects the trolley finger *22* to one of these resistance fingers, the current flows through the resistance connected between that finger and the bottom one, R. 6, and then through the armatures. For instance, on notch 1

finger 22 is connected to finger R. 2, the resistance in circuit is therefore R. 2 to R. 6. On notch 5 the finger 22 is connected to finger R. 6, the resistance is therefore short-circuited.

Now contact ring X. is loose on the main cylinder, and is driven by a pin working in a slot, which enables the loose ring to lag behind the main barrel by the space covered by three notches. When the controller is turned in a clockwise direction the position of this slip ring is as shown on the diagram, but if the controller is moved, say, to notch 6, and then back to the off-position, the slip contact hangs back three notches behind the rest of the main cylinder and takes up the position shown in dotted lines.

It will thus be seen that the resistance in circuit on notches 1, 2, 3, 10, 11, and 12 is short-circuited when the controller handle is being turned in a counter-clockwise direction. In the same manner the resistance in circuit on notches 7, 8, and 9 is short-circuited only when the handle is turned in a clockwise direction.

Now notches 13, 14, and 15 are rheostatic brake notches when the barrel is turned past the off-position. The finger 22 is disconnected from the trolley and connected to earth, and the series fields are reversed by the small brake cylinder on the left of the diagram. The resistance in circuit on the first notch is R. 2, R. 6; on the second notch R. 4, R. 6; and on the third notch all resistance is short-circuited by contact X., which will be in the position indicated by dotted lines.

The present form of controller has only been in existence twelve months, but there are now about one hundred of them working successfully.

As previously mentioned, when the first experiments were made in 1903, the motors were connected permanently in series, the speed being regulated by the shunt field. This worked well on the level, but great difficulty was experienced in preventing drivers from climbing steep gradients with a weak field, which caused heavy flashing at the commutators. It was then recognised that the introduction of the series parallel system of control was necessary to make the regenerative system suitable to all conditions.

The first scheme was to weaken the field until the armatures in series had attained a speed of double the minimum reactive speed, then to open the armature circuit, double the shunt field strength, close the armature circuit again with the armatures in parallel, and then again weaken the shunt field to attain the necessary speed. For these experiments a box full of open switches was used.

It was found that the armatures could be put into parallel only by making a considerable pause to allow the field to increase, and then only on the level. To cure this the controller was arranged to insert a small resistance in the main circuit, in order to give the shunt fields more time to increase. This arrangement worked well, and the motors could be switched into parallel on an 8 per cent. gradient.

It was then found that getting the armatures back into series was even more difficult than getting them into parallel, it being necessary

to use more resistance in the armature circuit. This difficulty was, however, overcome in the first series parallel controller that was made on the circular plan.

Many motors, however, will not work sparklessly in series with the field weakened sufficiently to produce double the minimum speed, and the controller has therefore been re-designed with three resistance notches for making the series parallel changes. Twelve months after the commencement of the experiments with series parallel working, there were fourteen cars running with series parallel control on the Yorkshire Woollen District Tramways, and they have never given any trouble.

The following motors have been converted successfully: G.E., 67A, G.E. 58, G.E. 800, G.E. 52, G.E. 1,000A, G.E. 55A, Westinghouse 49B and 200, Brush 1,202, 1,002B, 1,002A, 800B, 800C. It can be taken as a fact that any motor which is good when series wound is good also as a regenerative motor.

Safety of the Regenerative Control System.

From the foregoing it will be understood that each notch on the controller corresponds to a definite speed, which speed varies only within small limits. So long as the controller handle is left on any one notch, the car will run at the speed corresponding with that notch, practically irrespective of gradients. It will be seen, therefore, that if a driver be running on the sixth notch at, say, 6 miles an hour, and then brings his controller handle to notch 4, which we will say corresponds with 4 miles an hour, the speed of the car will at once be reduced accordingly. If the car at the time be ascending a gradient, the armature current will be reduced; if it be descending a gradient, or running on the level, the back E.M.F. of the motors will exceed the line pressure, and the motors will return current to the line.

The driver has, therefore, always under his control a most powerful braking effect, which is produced without any physical exertion on his part. A braking effect, equal to the propelling effect, and in the absence of which the car cannot be moved. A brake also which cannot under any circumstances whatever lock the car wheels.

Most tramway accidents are caused by a car approaching a falling gradient at a speed greater than that at which it is desired to descend. The wheels are locked by the hand brake, or the electric brake may not build up owing to a dirty commutator, or a dirty controller. The result is only what one would expect—the driver loses his head and an accident follows. A regenerative car may drift on to a down grade in the same way, but with this difference, that the speed is constant and can be reduced by the ordinary movement of the controller handle, no part of the equipment and no single piece of cable being used which is not also used to propel the car. The car cannot accelerate and therefore cannot get out of hand.

Until lately, however, there was one objection to the regenerative

method of braking. If a car were descending a grade, and from any cause were cut off from the source of supply, the braking effect would be immediately lost, also the driver in shutting off his controller might produce momentarily the combination of a strong field and a high speed, thus allowing the motors to generate a voltage which has destroyed a considerable number of lamps and not a few station voltmeters.

Both these troubles have, however, been cured, or rather prevented, by the device shown diagrammatically in Fig. 3. Suppose a car to be descending a gradient at, say, 6 miles an hour, and suppose also that while so doing the trolley comes off the wire, there is a tendency for the voltage across the motors to increase; directly this happens, the current in the coil A increases and trips the switch X, so establishing a circuit from contact E to F, and then through the resistance C to earth. In actual practice it is found that should the supply be interrupted as explained above, a car can be brought to a speed as low as 2 miles an hour by the ordinary movement of the controller handle, the regenerated current passing through the resistance C instead of along the trolley wire. Thus the regenerative brake is not dependent for its action upon the continuity of the supply circuit.

There are, therefore, four most valuable claims for the regenerative brake :—

1. It cannot, under any circumstances whatever, or with any condition of the rails, lock the car wheels.
2. If it is out of order the car cannot be moved.
3. It is not dependent on the continuity of the supply circuit.
4. The more it is used the less it costs; in fact, it pays to use it.

Economy.

The saving in current varies according to the contour of the route, conditions of traffic, etc. On level lines it is small, but on hilly lines may be as much as 30 per cent.

Results of comparative current consumption tests on cars on the lines of the Bristol Tramways and Carriage Company, the South Metropolitan Tramways Company, and the Devonport and District Tramways Company are given below. In these three cases savings of 24 per cent., 26·7 per cent., and 28·7 per cent. respectively are obtained. At Bristol and on the South Metropolitan Company's lines at Penge, the tests were made with special cars unloaded, but at Devonport the meter was fixed on cars running in service.

A special point of interest in the Penge test is that the total current taken by the regenerative car, before deducting the regeneration, is 6·5 per cent. less than the current taken by the series motor car.

A number of temperature readings which have been taken on regenerative motors after service runs on several roads are also given.

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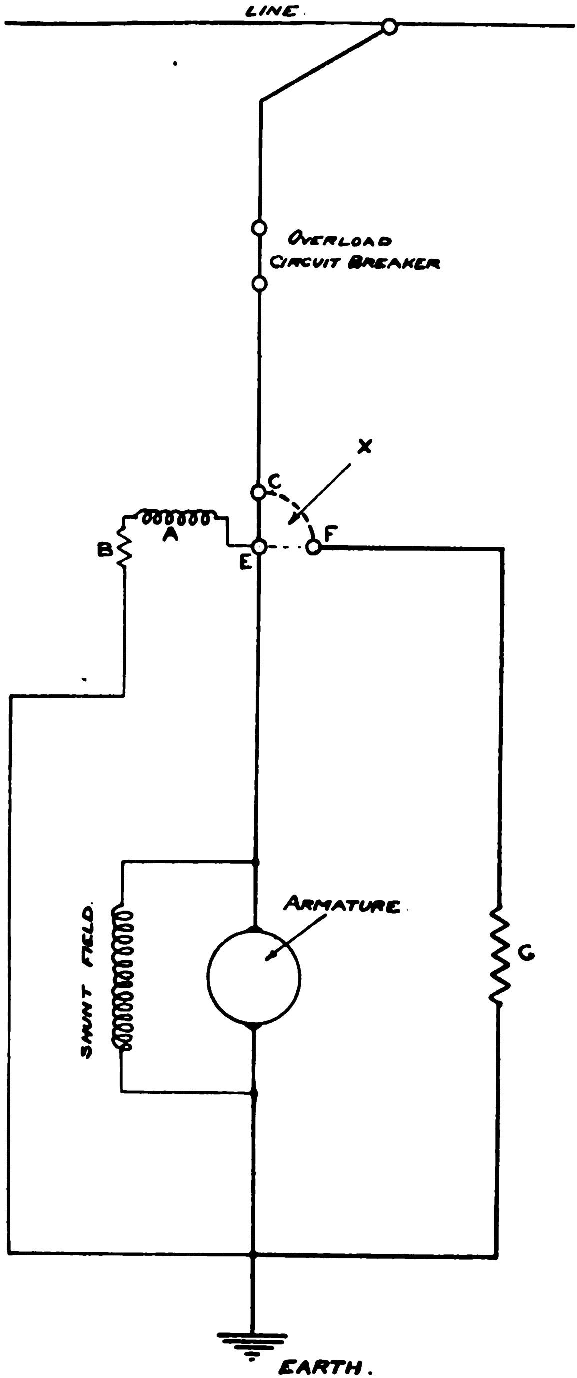

FIG. 3.

TABLE I.

Result of Official Tests taken at Bristol, March 14 and 15, 1906, on the Bristol Tramways and Carriage Company's System, with a 75-ampere Wattmeter.

CAR 105.—Equipped with Raworth's R. 3 type series parallel controller and two G.E. 58 regenerative motors.

CAR 181.—Equipped with B.T.H. B. 18 type series parallel controller and two G.E. 58 series motors.

Condition of Rail—Wet.

ROUTE.	MILES.		B.O.T. UNITS.	
	Car 105. Regenerative.	Car 181. Series Motors.	Car 105. Regenerative.	Car 181. Series Motors.
Tramway Centre—				
to Brislington	2·85	2·85	2·95	3·5
„ Centre	2·85	2·85	1·90	2·4
„ Hotwells...	1·69	1·69	1·37	1·5
„ Centre	1·69	1·69	1·83	1·5
„ Downs (1)	1·88	1·88	4·15	4·8
„ Centre	1·88	1·88	— 0·20	0·7
„ Downs (2)	2·66	2·66	4·90	5·4
„ Centre	2·66	2·66	— 0·20	1·7

[redacted]

TABLE II.

South Metropolitan Tramways Company, Ltd.

Official tests taken with two 75-ampere B.T.H. car wattmeters mounted on suitable springs, etc. Test taken on July 4, 1906, on two cars similar in every respect except the equipments, No. 4 car being equipped with 1,202 B. regenerative motors and R. 3 controllers; No. 10 car with 1,002 B. Brush motors and 3 A. controllers.

Route.	No. 4 Regenerative Car.				No. 10 Series Parallel Car.				Miles.
	Meters.		Time.	Stops	Meters.		Time.	Stops	
	Regenerating.	Taking Current.	Mins.		(Average Reading taken.)		Mins.		
Selby Road	223·6	42·6	—	—	51·0	225·4	—	—	
to Crystal Palace	223·8	46·2	10	6	55·1	229·3	9¾	6	1·609
to Selby Road	224·8	47·6	11¼	12	56·5	230·5	10¼	12	1·609
	1·2	5·0	21¼	18	5·5	5·1	20	18	3·218
	Net Units. 3·8		Units per Mile. 1·18		Net Units. 5·3		Units per Mile. 1·646		
Selby Road	224·8	47·6	—	—	56·5	230·5	—	—	
to Penge	225·0	49·3	9	6	58·5	232·5	10¼	6	1·54
to Selby Road	225·4	51·0	10	6	60·2	234·2	9¼	5	1·54
	0·6	3·4	19	12	3·7	3·7	19½	11	3·08
	Net Units. 2·8		Units per Mile. 0·909		Net Units. 3·7		Units per Mile. 1·2		
TOTALS ...	Units. 6·6		Minutes. 40¼	Stops 30	Units. 9·0	Mins. 39½	Stops. 29		6·298
	Average Units per Mile ... 1·04 Speed, m.p.h. ... ... 9·4				Average Units per Mile ... ... 1·43 Speed, m.p.h. ... 9·5				

Saving by Regenerative Car, Crystal Palace Route 28·25 per cent.
" " " Penge Route ... 24·30 "
Average saving by Regenerative Car ... ... 26·70 "

TABLE III.

Comparative Current Consumption Tests taken at Devonport, March 14, 15, 19, 1906, *on the Devonport and District Tramway Company's System, with a* 100-*ampere Watt-hour Meter.*

CAR No. 21.—Equipped with Raworth's R. 3 type series parallel regenerative controllers and two 1,002 B. regenerative motors. Mounted on Brush truck.

CAR No. 15.—Equipped with B.T.H. K. 10 controllers and two G.E. 58 (6-turn) motors. Mounted on Brill 21 E. truck.

Note.—Although the equipment and trucks of these cars are different the weights of the cars are approximately the same, No. 21 being a trifle heavier.

Route.	Total Miles Run.		Units Consumed.		Units per Car-mile.	
	Car 15.	Car 21.	Car 15.	Car 21.	Car 15.	Car 21.
Fore Street to Tor Lane and back	76½	54	98·8	49·6	1·29	0·92

Saving in current consumption by No. 21 Car = 28·7 per cent.

During tests both cars were running in service, and conditions of rail, traffic, etc., were exactly similar.

The saving in current as measured on the car is by no means the whole of the purely electrical savings effected by regenerative control.

The diminished output from the generating station implies diminished losses throughout the whole feeder system, and as the losses vary as the square of the current, the fact of reducing the latter by 20 per cent. is to reduce the line losses by 36 per cent. An original loss of 10 per cent. would therefore become 6·4 per cent., and this in the case of a system with an output of seven million units yearly at 1½d. would mean a saving of £1,575.

Again, the maximum demand on the power station will be very much reduced, and the variations of load will be less than with series motor cars. The actual mean load is at the same time diminished, with the result that probably one-fourth of the generating plant could be shut down, the car service remaining as before. At the same time the remaining generating plant would be more uniformly loaded, and therefore more economical.

Now a considerable saving may also be obtained by the reduction in the maintenance charges on trucks, wheels, and brakes. This is due to the fact that the regenerative brake is always made use of in regular running, thus to a great extent relieving from duty the hand and other mechanical brakes.

Not only is the continual setting, repairing, and replacing of brake gear done away with, but brake blocks, which ordinarily wear only a

TABLE IV.

Temperatures of Regenerative Motors.

Tramway System.	Controllers.	Motors.	Temperature in Deg. Fahr.					Time in Service and Section.
			Atmosphere.	Series Field Winding.	Shunt Field Winding.	Armature Winding.	Commutators.	
South Metropolitan Tramways.	R. 3	1,202 B.	63	142	164	159	155	14¾ hours on Selby Road and Penge.
South Metropolitan Tramways.	R. 3	1,202 B.	60	138	158	154	152	12 hours on Selby Road and Crystal Palace Hill.
Devonport and District Tramways Co.	R. 3	1,002 B.	57	106	106	112	128	10 hours on Tor Lane and Fore Street.
Devonport and District Tramways Co.	R. 3	1,002 B.	56	126	126	142	144	11 hours on Pennycomequick and St. Budeaux.
Birmingham and Midland Tramways.	R. 3	1,002 B.	46	153	168	150	154	11 hours on Station Road and Yardley.
Yorkshire Woollen District.	R. 3	1,202 B.	68	125	126	145	175	14 hours on Dewsbury and Cleckheaton.
Bristol Tramways and Carriage Co.	R. 3	G.E. 58 6 T.	53	139	168	147	151	14 hours on Knowle and Bristol Bridge.

few weeks, will last several months, records being available which show a life of from 20,000 to 30,000 miles. As the grinding away of the wheels, due to the application of the brakes, is also done away with, the life of the wheels is considerably increased. Even in hilly districts the figures which we have obtained from users of the system indicate a life of about 60,000 miles for chilled wheels with regenerative control, which is about double the life under ordinary conditions. The automatic action of the regenerative control brake for regular running also does away with the chief causes of flatted wheels.

Now, to take an example of what these savings would mean to a tramway undertaking running, say, 250 cars, and using, say, 10,781,227 B.O.T. units yearly.

The cost of current at 0·8d. per unit would be £44,920.

Assuming now that this sum would be reduced 25 per cent., an annual saving of £11,230 would be effected.

The saving in costs for repairs and maintenance, we will assume, is £15 per car per annum for trucks, wheels, and brakes—a fair estimate, I think, but possibly a little low.

The total yearly saving, therefore, in current, and in repairs to trucks, wheels, and brake gear, would be £11,230

3,750

£14,980

or enough to pay for the conversion of all the cars in two and a half years.

O

Discussion.

Professor Crapper.

Professor E. H. Crapper: In thinking about the subject of the paper, I recall a statement that was made some ten years ago when reference was being made to the principle of the emergency brake at the time when electric traction was new. Professor Ayrton made a remark that he would not like to recommend the free use of the emergency brake, because the motor ought to have a certain period for cooling, and that would not be possible if they were going to make use of the motor as a generator. I wonder whether this is the reason why so long a period has elapsed from 1885, when Mr. Sprague expressed himself as to this principle, to the time that Mr. Raworth took up the question. At the same time we must all congratulate Messrs. Raworth for the ingenious solution of the problem, and for having overcome a tremendous number of difficulties. I should like to know whether they have considered the question of the rating of motors. Can they say that a motor of given horse-power will do the work and not exceed a certain working temperature fixed as the standard for the rating of motors, *i.e.*, can they expect that it will keep its temperature less than 100° F. above the surrounding temperature? This matter seems to be of vital importance, and the capacity of the motors used should comply with the conditions with regard to rating, and the temperature, of course, is the important item. I am quite prepared to admit that if cost was left out of the question it can easily be done, and I think that Messrs. Raworth have solved the problem in a very satisfactory degree. Another point of vital importance is the life of the motor. Whether Messrs. Raworth have had sufficient experience is doubtful, and perhaps they will not be able to make any statement on this point. Until a moment ago I was under the impression that the motor was a shunt motor, and it is interesting to note the valuable part played by the additional series winding in this method of regenerative control.

Mr. A. R. FEARNLEY: It must be very gratifying to Messrs. Raworth to find regenerative control giving satisfaction at the present time, for they have encountered considerable difficulties and opposition in their efforts in endeavouring to solve this problem. I do not agree with the author in every respect, but no doubt he will be able to enlighten me on the points at issue. Regarding the temperatures, these are put down at least 25 per cent. higher than those we get with series motors on the Sheffield tramways. If we are to have a saving of 10 per cent. in current, and in turn an increase in renewals and maintenance, there will be little or no gain in adopting the regenerative system. It has been remarked in a polite manner in the advertisements of the supporters of this apparatus that there is a lack of appreciation on the part of tramway managers generally in not taking up this question in a sufficiently eager manner. It is not altogether a question of the power bill, but of the general financial result. If we are to gain in the power bill at the expense of another equally important department, I do not see the justification for the change. With reference to the author's statement that it is possible to obtain 60,000 miles from chilled wheels with regenerative control, I should be very much interested to hear where they are getting these results with chilled wheels of any description, as it is a considerable improvement on my experience in this direction. I had the pleasure of an invitation from Mr. Raworth to visit certain towns, including Birmingham, Spen Valley, Scarborough, and Penge, to inspect the apparatus under trial, and I am pleased to say that on each visit I found some improvement on the previous place visited, and at Penge, the last place visited, considerable improvement had been made, yet even there I did not see anything which would justify me in changing over from the present equipment to regenerative control. I do not agree with Mr. Raworth respecting the safety of cars equipped with regenerative equipment, as in my opinion there is exactly the same danger with this type as in coasting with a magnetic track brake, and for a town of considerable gradients which gives the advantages to the regenerative principle it would also, in my opinion, be attended with considerable risk if the regenerative control was used as the brake. Mr. Fearnley.

Mr. R. L. ACLAND: As regards the acceleration of these cars I should like to ask the author how it compares with the ordinary series parallel control, as this point would be of great importance on a large system. We are all aware that with the series parallel control a very high acceleration can be obtained. I should also like to ask how the Board of Trade views the system, and whether they permit the use of only the hand-brake with the regenerative control, or whether they also insist on a third brake for emergency. I should like to know what would be the effect on the generator if several of these cars in parallel were running down hill, and tending to run the generator as a motor, as might be the case on a small system, as at Chesterfield, where there are only six or seven cars on the line and the load ammeter often drops to zero. It would be interesting to know how this difficulty is over- Mr. Acland.

Mr. Acland come. I have had considerable experience with the magnetic track brake, which, although I consider it to be one of the best from the point of view of safety, undoubtedly puts much more work on the armature, and thus causes higher maintenance costs. It appears to me that if motors are to be used almost continuously either as motors or generators the ordinary rating must be decreased and larger motors used, or the upkeep will be very much higher, the same remarks applying to both the "regenerative control" and to the use of the magnetic as a "service" brake. When an armature burns out on the road, according to the diagram submitted, I understand that the motor is cut out in the same way as on the series parallel controller. Regarding the automatic cut-out carried on the cars, I understand that if a car is coming down a gradient and the trolley comes off, this arrangement is brought into action when the voltage on the motors (running as generators) rises to 600, putting the other pole to "earth" through a resistance and using the motors in the ordinary way as a rheostatic brake, but if the commutators are not clean there seems to be a danger, with the increased voltage, of flashing over before the coil on the automatic switch has time to operate.

Mr. Yerbury. Mr. H. E. YERBURY (*communicated*): It appears to me that the change from ordinary series control to series parallel has been the salvation of the regenerative system. I admit that I was very much disappointed at the result of my visit to Devonport in 1904, when I was instructed by the Tramways Committee to take tests and report generally on the system. No useful purpose would be served if I gave the figures then obtained respecting current consumption, temperature of motors, or described the brilliancy of the pyrotechnic display which I witnessed on lifting up the motor covers when the car was mounting a steep incline, with the shunt field weakened to the extent necessary to give a reasonable speed. I am glad to learn these troubles are now overcome, and I have no doubt that my faith in regenerative control will be considerably strengthened, if I do not become an actual convert, when I have another opportunity of inspecting and testing the system. From a theoretical standpoint the system may be called ideal, for we are all anxious, not only for the safety of passengers, but that the motormen should be relieved as far as possible from the manual labour which is now entailed in the application of the ordinary hand-brakes, and I look upon the saving in current consumption of secondary importance when compared with the advantages of an easy, safe, and economical method of control. Respecting current consumption and the saving effect of regeneration, it appears to me to be reduced to a somewhat fine limit, as I understand that a car must be travelling over 4 miles per hour before any useful regenerative effect is attained, and when the voltage from the motors increases up to, say, 600 the useful effect of regeneration is cut off by the automatic switch, which I am told now effectively prevents the original troubles, such as burnt-out lamps, opening of circuit-breaker at power station, and occasional reversals of generator polarities, and other irregularities of a disturbing

character. I am somewhat surprised to note that on one test at Penge it was found that 6·5 per cent. less current was taken by the regenerative car (ignoring the regenerative effect) than by the series motor car. As I have yet to learn that a shunt-wound motor is more efficient than a series motor, I would like to ask Mr. Raworth how he accounts for this reduction and whether it is brought about by the more rapid rate of acceleration. I am inclined to think that the controller handle on a regenerative car needs more careful manipulation than is sometimes seen in the "slashing round" on the ordinary series parallel equipment, and I would like to ask Mr. Raworth the estimated life of a motor with a commutator and coils working at a temperature of over 100° F. above the atmosphere. I am of opinion that the best results (including reasonable life) will be attained by having larger motors and, if possible, ventilated carcasses, although I note that the author of the paper boldly states that it can be taken as a fact that any motor which is good when series wound is good also as a regenerative motor. Mr. Yerbury.

Mr. E. J. MARSH: I should like to ask the author if the arrangement which he describes for automatically cutting out when the voltage reaches 600 replaces itself, because if so it seems likely to cause considerable trouble if the controller happens to be half open; or perhaps the motormen are instructed to put it back? If it needs to be replaced, I imagine that a good many of the drivers, when they find that they cannot get power to the motors, will be found sitting in the car waiting for the power to come on. I should also like to know what would be the effect on the motors and equipment with this system in the event of the controller being turned from the first to, say, the fifteenth notch without a pause. This is frequently done with the series parallel equipment without serious damage. Regarding the tests at the end of the paper, I should like to ask whether these tests were taken with an ammeter in front of the man who was driving the car, and whether he was an ordinary motorman, whose chief thought was how to get from one end of the city to the other. I am of opinion that if the ammeter was in full view it would be possible to get much better results by keeping the car at certain speeds than by the ordinary method of using the controller. Mr. Marsh.

Mr. E. A. PARIS: The company I represent has fourteen cars equipped with the first-evolved series parallel controllers. I did not notice in Mr. Raworth's paper the interesting and historical fact of the birthplace of this controller, and in justice to the company I represent this ought to be mentioned. The unfortunate conditions under which the system has had to be worked up to the present prevent us from fully profiting by all its advantages. Regarding the supply of current, power is taken from four different authorities, who have different units of plant. In one authority's boundary equipped with accumulators we have no trouble, but in all the others there is trouble owing to the return current, so that we have to coast down the hills to prevent the back current-breakers from coming out. A good many lamps have Mr. Paris

Mr. Paris. been broken in consequence of the high E.M.F. generated as soon as the station breaker comes out, but all these troubles are to disappear as soon as the apparatus shown in Fig. 3 of the paper is installed. What we can say is that we have very little repairs and adjustments to make, and that when we are in full swing and free from the station troubles, a saving of current as well as a considerable saving in wear and tear of brakes and in labour will no doubt result. As only a fifth of our cars are fitted, the saving in current over the whole system does not amount to more than about 5 per cent., if as much; taking current at 1·5d. per unit, they should save 75,000 units, or, roughly, £464. The saving, however, in other directions should make this figure quite acceptable as a set-off against the extra outlay. One important point about cars equipped with this system of control is that they are far less likely to collide. The car is kept better in hand than with the ordinary series parallel controller, in conjunction with which the hand-brake is required. I do not think that we have had any collisions with cars fitted with regenerative control.

Mr. Baylor. Mr. A. K. BAYLOR: I should like to mention a few practical points that have been referred to by one or two speakers, showing that in the minds of the speakers—and it seems to be very general among tramway engineers—anxiety exists as to working tramway motors as generators. It seems to be the general opinion that the operation of the regenerative system introduces certain disadvantages in the practical working of the car that series parallel control does not possess. As a matter of fact, ever since traction motors have been in general use every engineer has had his eye on regeneration. He has known of its possibilities, and it has only been a matter of practicability. The early motors could not be handled in this way because of excessive heating and sparking. Although the motors must heat more than they would with ordinary series parallel control, yet this heating does not extend into a dangerous zone. In fact, the heating and sparking difficulties have practically disappeared, and it is possible to take advantage of the great saving in current and also in wheel and brake wear and tear without attendant disadvantages. Mr. Raworth has, during the last few years, worked out and perfected the controller and the various accessories, such as the high-voltage cut-out switch, etc., which have been found necessary in actual practice. The equipment, as it exists to-day on many cars throughout England, is in every way practicable, and in accordance with tramway engineers' ideas of standard practice.

As regards the working of the system, I would mention the Crystal Palace line. This place is particularly adapted for demonstrating the principle of regenerative control. It is very steep, and at the foot of the hill there is a series of undulations, which give an opportunity of showing the regenerative effect particularly well. On a hilly line like this the additional safety due to regenerative control is especially emphasised. As to the handling of the controller, one speaker asked what would happen if the man brought his controller handle back too rapidly. Supposing a man with an ordinary series parallel

controller brought his handle rapidly from the first notch to the last, he would bring out the circuit-breaker or blow a fuse, or something else undesirable would happen. The men do not do it, because they are told not to do so. They become accustomed to the equipment, and they do not do it. They would quite as readily become accustomed to the regenerative control, and would not bring their handles back too quickly. If a man should do so he would probably be thrown over the dasher into the street, and would not want reminding again. If we take the evidence of the men themselves, we are told that they much prefer the regenerative control. I have handled tramcars on various gradients, and I am sure that in hilly districts it is no joke to bring a heavy car down on its brakes, but when a man feels that he has got the whole of the load under his control without any muscular effort, it adds greatly to the safety of running, because it relieves him of the nervousness which often prevails. Mr. Baylor

The accidents which have occurred at Highgate and elsewhere, and on which the Board of Trade has been reporting recently, emphasise the safety of regenerative control, and I venture to say that the Highgate car was in perfectly good braking condition when it went over the brow of the hill, but for some reason or other the man allowed the car to accelerate, and, after it got beyond a safe point, he was either unable to pull it up or lost his head. After that the accident was inevitable. With the regenerative system the car can be taken over the brow of the hill with the brakes thrown entirely off and the controller handle set on one of the early running notches, say 4 or 5 miles per hour, and the car will glide down the hill at that speed. It cannot go any faster or any slower. It cannot get out of hand. This point appeals to everybody using cars in hilly districts. I am perfectly satisfied that in a very few years regenerative control in some form will be absolutely universal on tramcars.

Mr. H. C. JENKINS: There is one point I should like to mention. The author states, regarding tramway accidents generally, that the electric brake may not build up owing to a dirty commutator or a dirty controller. I imagine that the same thing would happen with the regenerative control, as there would be a break in the circuit as with the electric brake. Mr. Jenkins.

Mr. R. C. GOLDSTON: Having personally operated both kinds of cars, I am perhaps in a better position than most people to speak as to their performance. First of all, with regard to the difficulties in operating, no matter what the rails or gradients may be like, practically the same results can be obtained with a regenerative car as with a car fitted with ordinary series parallel control. There is a little difficulty in starting. On a steep gradient, with unfavourable conditions of rail, there may be a little more difficulty in starting with the regenerative control, because, as the diagram shows, the resistance notches to start with are rather less in number than those on ordinary controllers. There are more notches on the ordinary controller than on the one before us; therefore the notches must be Mr. Goldston.

Mr. Goldston.

further apart and the jumps accentuated. This is the only difficulty. With regard to running down hills, the effect of the regenerative car is, of course, to give the motorman every confidence, as on descending a hill the controller is not switched off, but is still kept on one of the running notches according to the speed required. The car will then travel downhill at the same speed as it would on the level or uphill, the difference being, of course, that the motors are giving back current, taking a small amount, or taking a large amount, according to the gradient. The fact of the car not accelerating when it gets over the brow of a hill is a great point in the matter of safety. There is one point regarding the descending of very steep hills, and that is, if you have track brakes on the car it is just as well to apply them, because if the gradient is very steep, say 12 or 13 per cent., the wheels are liable to slip (not skid, for you cannot lock the wheels), but the momentum of the car is liable to overrun the speed of the wheels, and the car may be slipping down. If there is a track brake fitted to the car, by applying it as before stated any tendency to slip is eliminated. We have mechanical track brakes fixed to our cars, and it is invariably our custom to use them. On the ordinary hills this effect is not so serious. Regarding the life of the motors, the heating is slightly higher than those of the ordinary series parallel type, but with a Brush 45-H.P. motor rated at 125° F. temperature rise after 1 hour run at full load, the temperature rise above zero, in a number of experiments, was as follows: The temperature of the shunt winding rose to 150° F. The temperature of the armature rose to 160° F. With the ordinary series parallel controller fitted on an exactly similar car and running over the same road, the temperature of the armature winding was 120° F. and the temperature of the field winding 100° F. The number of hours that the car ran was 15. It will therefore be seen that the difference in temperature is very little, and is well within what the makers of the motors guarantee as their rated temperature.

The difficulties with the armatures with regenerative control are of little moment, and I can say that we have never had an armature burnt out with this system. As a matter of fact, also, we have never had an armature burnt out, through any electrical fault, on a car fitted with Westinghouse brakes.

With regard to maintenance, a very considerable saving has been effected, such as the upkeep of brakes and other parts which have to be seen to every day. With the ordinary series cars running on some of the worst routes the brake blocks last from five to six weeks (the cars do not run every day, but every second day, as there is practically a double shift), but with the regenerative cars I find that brake blocks last from five to six months, which is a very big saving both in material and labour. The adjustment of the brakes of an ordinary car must be carried out on an average about every day, but the adjustment of the brakes on cars fitted with regenerative control about once a week. They are, of course, examined every day, but the adjustments are not necessary. I also find that the adoption of this system does not

mean an increase in staff for the 10 extra cars which we have, and that exactly the same staff deals with the work. The only other item of importance is the power supply. We have four power stations to deal with and four different systems of supply. With one of them which consists of shunt machines with a battery and booster we have no trouble, as the regenerative cars when returning current to station simply charge the battery. The other systems have compound machines without batteries, and one of them has the reverse current-breakers set to a very sensitive degree, so as to come out at 15 or 20 amperes of reverse current. In consequence of this regenerative cars are not run on this section. In the other two the reverse breakers are not nearly so sensitive, and therefore not very liable to open. Although the current is sometimes cut off, we have never experienced any ill effects as regards safety. The motormen were aware of what had happened, and applied the ordinary brakes and pulled up. In addition to the ordinary brakes (hand and track) there is the rheostatic brake similar to that fitted to the ordinary series parallel cars, which can be used when the current goes off. Another point with regard to current going off or the trolley being de-wired: even if this happens the shunt motors will retain their fields for a certain time so that the car can drift down the hill without accelerating, and eventually come to a standstill without danger of running away. Mr. Goldston.

Mr. H. O. Wraith: The principal objection to this system of regenerative control appears to be the temperature rise in the motor and sparking. To get over the heating is surely only a matter of increasing the quantity of material in the motor. This, of course, adds to the capital cost of the equipment, but if a saving could be effected, as in the cost of upkeep of brakes, it appears worthy of adoption. I am not a traction engineer, but I have had a good deal of experience with both shunt and compound wound motors where the speed has to be varied under more severe conditions than in a tramcar, and it is wonderful what a properly designed motor can stand. The case in my mind is an idea used for reciprocating machinery where the motor is reversed and has to run at different speeds when running forward and when reversed, and we have to make use of regenerative and other effects for reversing the motor. If Mr. Raworth has trouble with sparking and flashing on the motors, I think the electrical design of the motors is at fault, and this should be easily remedied. Regarding the handling of the controller, although, of course, it should not happen, we cannot avoid getting a certain amount of mishandling at the hands of the average tram-driver, and I would like to know what really has been the experience of the result of rough usage of the controller with this system. It would also be very interesting to hear how the capital cost of the regenerative control equipment compares with that of the ordinary series parallel system. Mr. Wraith.

Mr. W. N. Y. King: I should like to ask Mr. Raworth how the conditions of service affect the amount of energy returned to the line. For instance, with a service where the stopping-places are only a short dis- Mr. King.

Mr. King. tance apart, and consequently the cars pull up every minute, the amount of energy returned to the line would be less than in a country district where the stopping-places are a considerable distance apart. In Sheffield the gradients are very steep and of considerable length, some of them 2 miles, but still the cars have to stop very frequently, and under these conditions there would not be so much energy returned to the line as may be expected from the gradients.

Mr. Baxter. Mr. WM. BAXTER (*communicated*): In this paper Mr. Raworth raises the old question when he says that "It can be taken as a fact that any motor which is good when series wound is good also as a regenerative motor." I presume that the full field speed of the re-wound motors is kept down to the same figure as when series wound. This being the case, it is very difficult to see how a greater temperature rise can be avoided. For instance, take the case of the motors running on the route from Selby Road to Crystal Palace and back. So far as the motors are concerned, the regenerated output has just the same heating effect (per unit, in a given time) as the input from the line. For the regenerative car the sum of these units amounts to 6·2, against 5·3 (average) for the series parallel car. The average load on the regenerative motors is thus 17 per cent. greater than that on the series parallel ones. In this connection the temperatures given in Table IV. are of little or no value without corresponding figures for series parallel motors running on the same routes. Again, unless the re-wound motors are fitted with commutators having an increased number of segments, the sparking constants, when running on notches 14 and 15, will not be as good as they were before re-winding. Raising the normal speed of the motors would, of course, help to get over the sparking difficulty, but higher gear ratios would be necessary. As these usually run about 5 : 1, very little can be done in this direction. Full data for a re-wound motor would have made Mr. Raworth's paper still more interesting, at least from a designer's point of view. On studying Table II., the first thing that strikes one is that one of the meters, while on the series parallel car, ran at least 8 per cent. faster than the other, for part of the time. As this meter had previously been used for measuring the intake on the regenerative car, it is quite possible that the net units (3·8) are overstated, and that consequently the saving by the regenerative system is understated. In this connection it would be interesting to know if there was any reason for stopping the car twelve times going *down* to Selby Road and only six times going up the hill to the Crystal Palace.

Mr. Raworth. Mr. J. S. RAWORTH: There is one element in the theory of regeneration which is apt to be lost sight of, namely, that for any given field strength there exists a definite speed of armature varying only to a very slight degree as the machine changes from motor to generator, and secondly, that with any given armature speed the field strength is the same whether it be produced by shunt winding or by series winding. As regards heating, it is obvious that the regenerative motor must run at a higher temperature than the same size of series motor. The

practical question to be decided is, does the higher temperature exceed the safe limit, that is to say, the temperature at which deterioration of the insulating material commences? Up to a certain temperature, which is certainly not less than 212° F., there is no deterioration, but if the limit be passed the destruction of the cotton is completed in a very short time. This is common knowledge. One of the great advantages of the system described in the paper is that it can be applied to existing motors, but we never undertake to convert a motor which is already overworked, and if we supply new motors we select some with a good margin of power. Mr. Raworth.

The tests of temperature quoted in the paper and by Mr. Goldston show that the heating is not excessive. I had some time ago to consult no less an authority than Mr. Parshall on this question of heating. I mention this because there seems to be an idea in the minds of some of the speakers that if the temperature of a motor be limited to 120° F. the insulation will last longer than that of a motor which attains a temperature of 140° F. My opinion is that it does not. I put the question to Mr. Parshall, and he said he did not know exactly where the danger limit was, but he knew that 160° was well inside the limit, as all the motors on the London Electric Railways are running at that temperature and give no trouble. Provided, therefore, that we do not go beyond the limit of safety, we are not more likely to burn out at 160° than at 120°. As regards sparking at the commutator, this has entirely ceased now that we have perfected the paralleling controller. With the old series controller, careless driving produced sparking. The points raised in Mr. Baxter's communication are nearly the same as those referred to by other speakers. The normal specified rise of temperature on one hour's test at rated load is 75° C., or 135° F. The quoted tests show that this limit is never reached. There is no necessity to redesign the commutators or to increase the armature speed to cure sparking, because there is no sparking. The reason why the car stops more frequently in descending the Crystal Palace Hill than in ascending is because the Board of Trade has fixed a number of compulsory stops on the down grade. With regard to the safety in descending hills, there seems to be a general impression that in the event of the trolley leaving the wire we lose control of the car. I think this is put perfectly clear by the diagram submitted, that when the trolley comes off the motors are immediately put to earth; but in addition to that, if anything happens within the controller or within the motor itself, when the handle is brought round on to the braking position we get on to a new set of contacts exactly similar to those in an ordinary series parallel controller, for the static braking.

There is just another point I may mention, namely, that if the cars proposed to be converted are already fitted with magnetic track brakes they will act a good deal more effectively with this controller and these motors than with the ordinary series motor, because they are not dependent on building up after the handle is moved on to the braking notches.

Mr. Raworth.

Mr. ALFRED RAWORTH (*in reply*): I am exceedingly obliged for the kind way in which the paper has been received. Mr. Fearnley says that the temperature readings which I have given are at least 25 per cent. higher than those which he has obtained in Sheffield. This may be so, but they were taken on different types and sizes of motors and on various roads, and I think every one will agree that they are not excessive. Mr. Goldston has handed to me the following table of some temperature readings which he has taken on both series and regenerative motors on the Yorkshire (Woollen District) Tramways, which afford a very interesting comparison. The weight of the cars in all cases is 9 tons, and they were in service for sixteen hours. The type of motor is 1202 B. Brush, the regenerative motors being series motors re-wound.

TEMPERATURES.

	Series Motors.	Regenerative Motors.
Armature Winding...	120° F.; 110° F.	132° F.; 148° F.; 135° F.
Commutator ...	132° F.	170° F.
Field Windings ...	105° F.; 100° F.	120° F.; 102° F.; 130° F.
Atmosphere	55° F.; 60° F.	55° F.; 60° F.

Mr. Fearnley said that in his opinion a car working on the regenerative motor brake is subjected to exactly the same danger as a car coasting on a magnetic track brake, but this is a mistake. Two of the disadvantages of the magnetic track brake are that it often locks the car wheels and that it is put entirely out of action if the wheels are locked by the hand-brake. But the regenerative brake cannot under any circumstances whatever lock the car wheels, and it is entirely independent of and unaffected by any other brake. Replying to Mr. Acland, the acceleration obtained is the same as with series motors on the low speeds, but it is much better on the higher speeds. The Board of Trade has so far expressed no opinion officially with regard to the safety factor, but I believe that the regenerative system is very favourably viewed by their advisers. Mr. Acland also asks what would be the effect of a current returning to the station. This contingency is provided for in the following manner: Should a reverse current reach the station, a relay actuates an electromagnetically operated switch, which closes a circuit between the busbars through a resistance. Immediately the current assumes the normal direction the switch resets itself. This apparatus effectually prevents any damage being done to power station gear. The controller shown diagrammatically in Figs. 1 and 2 is provided with the usual motor cut-outs. Mr.

Acland is correct in saying that the braking power of regenerative control should, especially at this time, be very carefully considered by tramway managers. Mr. Baylor has most ably described the chief points of the braking function, so I will not go over it again; but the qualities that more particularly deserve attention are the impossibility of locking the wheels and the fact that the car is always under control and therefore cannot accelerate to dangerous speeds, which, once attained, are so difficult to check. Mr. Yerbury, when he inspected regenerative cars at Devonport in 1903, saw only the very earliest development, when the motors were permanently connected in series; this arrangement has since been abandoned, and all the defects which Mr. Yerbury noticed on that occasion have now been eliminated. I am glad to hear that Mr. Yerbury admits the perfection of the regenerative system from the theoretical standpoint, but he is wrong, however, in thinking that the regenerated energy is decreased by the automatic switch diagrammatically shown in Fig. 3. This switch only operates when the voltage across the motors rises above 600, and this only happens when an open circuit occurs, either through the trolley leaving the wire or a circuit-breaker opening. All the troubles which he mentions are prevented by this switch, and it also has the advantage of making the braking of the car independent of the continuity of the supply circuit. Should a trolley leave the wire, the switch operates *immediately*, and the motorman can reduce the speed of the car to 2 miles an hour by the *ordinary* movement of the controller handle. Mr. Raworth.

Mr. Jenkins's conclusion that a dirty commutator or controller might influence the braking effect of regeneration is wrong. The circuits used for regenerating being exactly the same as those for propelling, and the voltage slightly greater, it follows, therefore, that if the motors will not regenerate they will not pull, and consequently the car cannot be taken out. Replying to Mr. Marsh's question, the automatic switch which operates when the trolley leaves the wire does not reset itself, but I fail to see that a motorman would be more likely to leave it open than he would the ordinary overload circuit-breaker.

As regards the tests at the end of the paper, those at Bristol and Penge were made on special cars, and the motorman knew that a test was being made; but this is true of the series motor car also. The instruments were in both cases inside the car, and therefore out of sight of the motorman. In the case of the Devonport cars the tests were made while the cars were actually carrying passengers in service. Mr. Wraith asks the extra cost of a regenerative equipment over a standard type. This is approximately £40 per car. I would also remind him that we have not so far designed a motor for regenerative control. All the motors which are running regeneratively are "series motors" with the series coils changed for shunt coils. Mr. King asked how the conditions of service affect the amount of energy returned to the line. In the case of going up a gradient a car may have a straight run up. In another case the car may have to proceed up the hill behind a coal cart, and consequently the driver may have to run on the

Mr. Raworth.

resistance notches, and therefore much less economically. I think that I have now replied fully to every one's questions and criticisms, but before sitting down I should like to pay a tribute of appreciation and thanks to Mr. Paris and Mr. Goldston for the very valuable assistance which they rendered during the experiments with, and the development of, the series parallel type of controller.

On the conclusion of the paper a cordial vote of thanks was passed to the author by the meeting.

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BIRMINGHAM LOCAL SECTION.

THE HEATING COEFFICIENT OF MAGNET COILS.

By G. A. LISTER, B.Sc., Associate Member.

(*Paper read December* 12, 1906.)

SYNOPSIS.—Introduction—The Heating Coefficient: the mean final temperature rise, the maximum temperature, the surface per watt—Variation of the Heating Coefficient—Actual Values of the Heating Coefficient: (1) Coil supported in air; (2) coil in position on machine, machine stationary; (3) machine running light at normal speed; (4) machine running fully loaded—Conclusion.

Introduction.—When discussing R. Goldschmidt's paper * on "Temperature Curves and the Rating of Electrical Machinery," Professor Silvanus Thompson called attention to the discrepancies which exist between the figures for the heating coefficient of field coils as quoted by various authorities. By the heating coefficient is meant the mean final rise of temperature above that of the surrounding air which would occur could the coil absorb energy at the rate of one watt per unit of radiating surface. Taking the temperature rise in degrees Centigrade, and the square centimetre as the unit of surface, the figures range from Esson's 355 to Goldschmidt's 1,500.

The great range of these figures is partly due to the entire lack of agreement as to what portion of the surface of the coil should be looked upon as radiating surface. Some experimenters have considered the outside cylindrical surface only, others the outside surface plus the ends, and others again have taken the whole surface. But even if the figures be reduced to a common basis in this respect it will still be found that they disagree to a great extent. This may be accounted for by the fact that the experiments were carried out under different conditions on different types of coils and machines, and that a standard method of ascertaining the temperature rise was not adopted.

Within the last few years several papers and articles on the heating of magnet coils have been published,† but the work done has been

* *Journal Institution of Electrical Engineers*, vol. 34, 1905, p. 660.

† E. Brown, "Rise of Temperature in Field Coils of Dynamos," *Journal Institution of Electrical Engineers*, vol. 30, 1901, p. 1159; Neu, Levine, and Havill, "Heating of Magnet Coils," *Electrical World and Engineer*, vol. 38, 1901, p. 56; E. H. Rayner,

mainly in the direction of determining the temperature gradients throughout the coil, rather than the mean temperature rise, upon which the heating coefficient depends.

There can be no doubt as to the value of reliable figures of the heating coefficient to designers and manufacturers of electrical machinery. Upon the value used in the design depends the temperature rise of the machine, and consequently the rating. It has become more important since the recent revival of commutating poles, the use of which has done so much to increase the sparking limit of machines. It was felt, therefore, that further experiments on the temperature rise of magnet coils of various types and under various conditions should prove useful.

By the kind permission of Professor Kapp I was able to carry out a number of such experiments in the Electrical Laboratory of the University of Birmingham. The results of these experiments will be found in the following curves, and I have also availed myself of the data published in the papers to which reference has already been made.

The Heating Coefficient.—The heating coefficient as applied by the designer of electrical machinery is expressed by—

$$C_h = S_w . t.$$

where—

t = Mean final temperature rise in degrees Centigrade.
S_w = Coil surface in square centimetres per watt dissipated.
C_h = Heating coefficient.

The mean final temperature rise is given by the difference between the mean temperature of the coil when it has reached a steady value, and the temperature of the surrounding air. The method of measuring the temperature of the coil by a thermometer is unsatisfactory. This is due to the difficulty of obtaining consistent readings, and also to the fact that the temperature of a coil varies considerably at different portions of its surface, as has been pointed out by Messrs. Brown and Rayner.

The safer way is to determine the temperature by calculation based on the observed increase of resistance. This gives the mean temperature throughout the coil, and is the method which has been used in the tests here recorded. The actual resistances are conveniently determined by means of a voltmeter and ammeter. Care, however, must be exercised, as an error in estimating the resistance is considerably magnified in the final result. Again, the current taken by the voltmeter, if allowed to pass through the ammeter, may easily introduce an error of as much as 5° of temperature. It is also preferable that the resistance of the coil when cold be measured by the same instruments as those which will be used during the test, the measurement

"Report on Temperature Experiments carried out at the National Physical Laboratory" *Journal Institution of Electrical Engineers*, vol. 34, 1905, p. 613.

being taken as soon as the current has reached a steady value after switching on.

The Maximum Temperature.—The ratio of the maximum to the mean temperature of a coil varies with its dimensions and design, and according to the conditions under which it is worked. The following average values of this ratio are taken mainly from Mr. Rayner's report to the Engineering Standards Committee. Mr. Rayner's tests were made on machines ranging from 25 to 500 k.w.

Conditions.	Ratio. $\frac{\text{Max. temperature}}{\text{Mean temperature.}}$
Stationary coil without core	1·12
Coil in place on machine	1·17
" " machine running light ...	1·15
" " machine fully loaded ...	1·175

Very few values are over 1·2, and this is consequently a safe figure to employ when estimating the maximum temperature of the field coils of fully loaded machines.

The Surface per Watt.—As previously stated, there is a diversity of opinion as to what portion of the coil surface should be taken in expressing the surface per watt, or the surface to be provided per watt dissipated in the coil.

Messrs. Neu, Levine, and Havill, in tests on small Crocker-Wheeler motors, found that the temperature rise for a given amount of energy dissipated was considerably greater when the coil was supported in air than when placed in position on the core of the machine. From this they concluded that conduction of heat to the carcase of the machine was more effective than direct radiation, and they proposed a conduction constant for that portion of a coil in contact with the core, and a radiation constant for the free portion. They admit, however, that "when the machine is running, the iron does not cool the coils so much as when not running."

Mr. Rayner stated in reply to the discussion on his paper that he had observed no extra cooling due to the iron core. In fact, with a coil fitting tight on a pole, very little heat abstraction was found to take place on the core side.

My own tests show a slight increase in the temperature rise when the coil is placed on the core, this small effect occurring both with taped coils and with coils wound on a metal former. In view of the fact that the temperature rise differs so very little in the two cases, it seems preferable to consider the total surface of the coil when dealing with the surface per watt, and to use a "heating coefficient" which will combine the effects of radiation, conduction, and convection.

Variation of the Heating Coefficient.—The heating coefficient, although usually quoted as a single figure and called the heating constant, is certainly not constant even for the same machine. It varies with the speed and with the load. In different machines it will also vary with

the nature of the surface of the coil, the method of securing the coil to the machine, the ventilating properties of the rotating armature, and generally with the design of the machine. If all these conditions remain constant it will still be found that the coefficient varies with the final temperature attained by the coil.

Since the greater part of the cooling effect is due to radiation, it is not surprising that field coils should follow to some extent the law of radiation. In 1879, Stefan* showed that radiation was proportional to the fourth power of the absolute temperature, and this has since been verified, and is recognised as Stefan's law. If radiation only had to be considered we should have—

$$k = S_w (\theta_2^4 - \theta_1^4),$$

where k is a constant known as the "constant of radiation" and θ_1 and θ_2 are absolute temperatures.

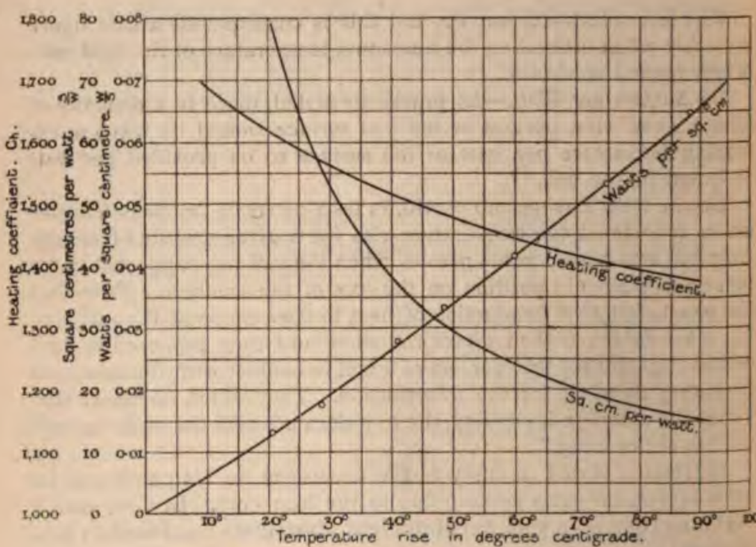


FIG. 1.—Coil No. 1 Supported in Air.

Small Coil covered with two layers of tape and varnished. Total Surface 960 sq. cm. For details of Coil see Table I.

The experimental heating curves of field coils show characteristics of this law (see Fig. 1), but unfortunately the disturbing and uncertain effects of conduction and convection render it impossible to state a general equation, such as the above.

Actual Values of the Heating Coefficient.—The following curves, Figs.

* *Kaiserliche Akademie d. Wiss, Wien, Sitzungsberichte* (Section 2) vol. 79, 1879, 391.

to 7, show the variation of the temperature rise with the watts ab-
orbed for various types of coils and under varying conditions. In
ases where it should prove useful, the heating coefficient has also been
lotted.

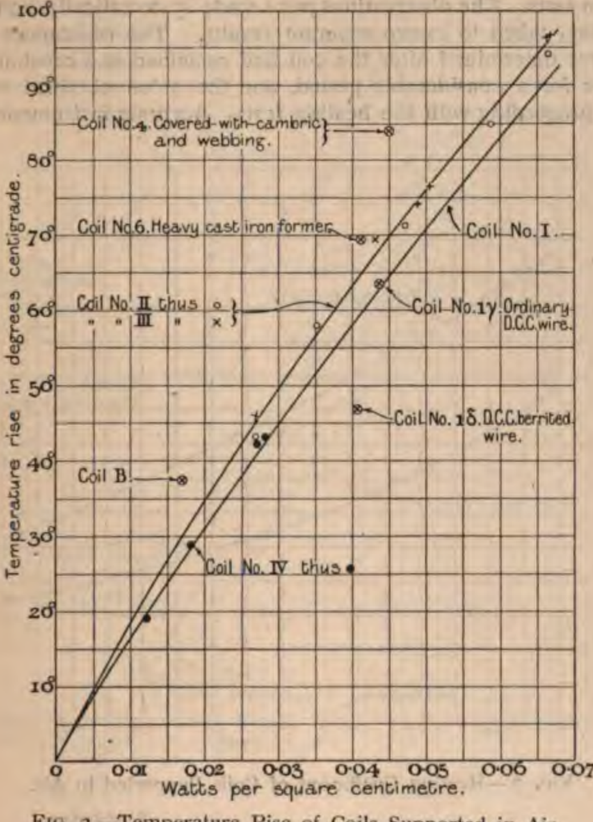


FIG. 2.—Temperature Rise of Coils Supported in Air.

	Total Surface.
Coil No. I. Covered with two layers of tape ...	960 sq. cm.
Coil No. II. Covered with Mica and tape	1.075 sq. cm.
Coil No. III. Heavily Insulated with Mica and taped	2,530 sq. cm.
Coil No. IV. Wound on Sheet Iron former	2,100 sq. cm.

For details of these Coils see Table I.

Four conditions have been considered:—

1. The coil supported in air.
2. The coil in place on the machine with the machine stationary.
3. Machine running light at normal speed.
4. Machine running fully loaded at normal speed.

Some of the results recorded have been taken from the papers
reviously mentioned. In each such case the points are plotted in a dis-

tinctive manner, thus ⊕. Particulars of these coils, so far as they are available, will be found in Table II., and also full references to the original papers.

A few additional words of explanation are necessary in reference to my own tests. The observations were made systematically, and every precaution taken to insure accurate results. The resistances when cold were determined after the coil had remained at a constant temperature for a considerable period, and the value obtained verified before proceeding with the heating tests. Accurate instruments were

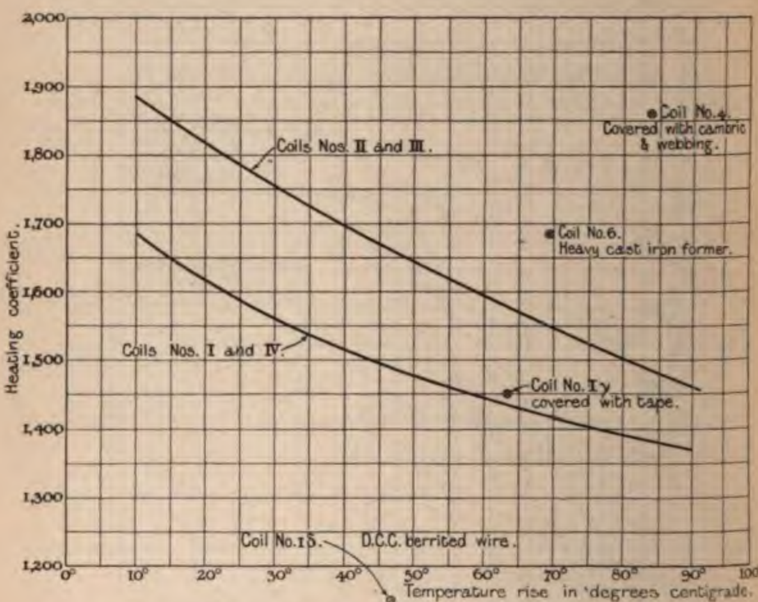


FIG. 3.—Heating Coefficient of Coils Supported in Air.

	Total Surface.
Coil No. I. Covered with two layers of tape ...	960 sq. cm.
Coil No. II. Covered with Mica and tape	1,075 sq. cm.
Coil No. III. Heavily Insulated with Mica and taped	2,530 sq. cm.
Coil No. IV. Wound on Sheet Iron former	2,100 sq. cm.

used, and for each coil the same instruments were employed for all the measurements upon that coil. The resistance of the coil at 0° Centigrade was calculated by using the standard temperature coefficient of 0·428 per cent. increase of resistance per degree Centigrade rise. All temperature rises were then calculated from this basis. Each measurement of final temperature was made after the coil had been carrying current for a period averaging about ten hours, thus insuring that perfectly steady temperature conditions had been reached. Full particulars of the six coils tested will be found in Table I., and

photographs in Figs. 8 to 11. The coils have been numbered I. to VI., using Roman figures. In every instance the figures for the watts per sq. cm. are based upon the total surface of the coil.

1. *Coil supported in air.*—In my tests the coils were supported on

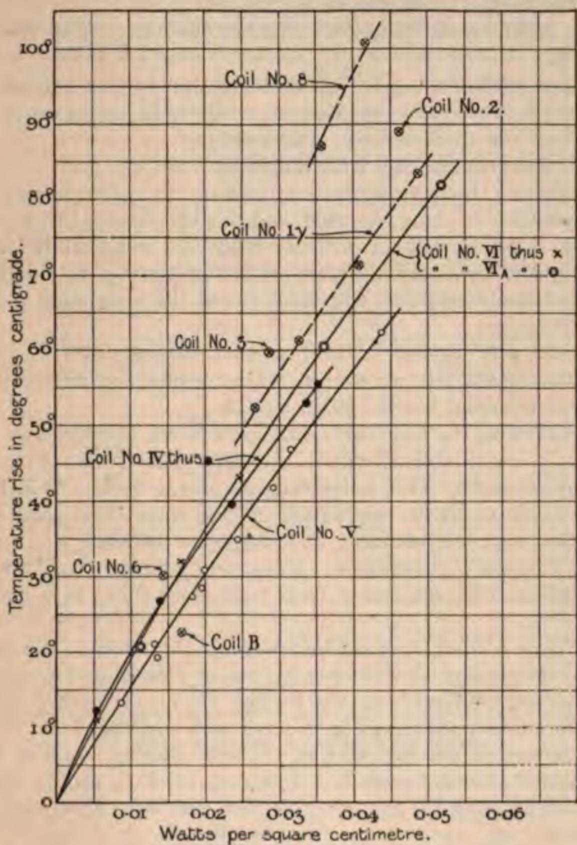


FIG. 4.—Temperature Rise of Coils in Position, Machine Stationary.

	Total Surface.
Coil No. IV. Wound on Sheet Iron former	2,100 sq. cm.
Coil No. V. Wound on Zinc former ...	1,815 sq. cm.
Coil No. VI. No former, very little covering	3,400 sq. cm.
Coil No. VI*a*. No former, very little covering	3,400 sq. cm.

For details of these Coils see Table I.

three points, at a height of from 4 ins. to 6 ins. above the surface of a table. Four coils were tested in this manner, and the results will be found plotted in Figs. 1 to 3. The actual observations are shown. Points obtained from published data are also plotted.

The results obtained on Coil I. are given separately in Fig. 1, the watts dissipated per sq. cm., the sq. cms. per watt, and the heating coefficient being all plotted against the temperature rise. The whole of the observations are plotted in Figs. 2 and 3; the temperature rise against the watts per sq. cm. in Fig. 2, and the heating coefficient against the temperature rise in Fig. 3.

These results show clearly the effect of the covering of the coil on its heating.

Coil 4 is that of a 100-B.H.P. motor tested by Mr. Rayner. This was covered in an unusual manner with three layers of varnished cambric and four thicknesses of boot webbing.

Coil I. was covered only with varnished tape.

Coils II. and III. gave almost identical results. In each case a considerable amount of mica was used, and this was covered with tape and varnished. Coil II. was an ordinary field coil, and Coil III. was one belonging to a stationary armature of a high-tension alternator. The latter was heavily insulated with mica round the embedded portion of the coil.

Coil IV. was wound in a sheet-iron former with the outside cylindrical surface left uncovered. The results are nearly identical with those obtained for the plain Coil I.

An interesting fact is the remarkably small temperature rise of Coil I.δ. It was identical with I.γ, except that it was wound with d.c.c. berrited wire. This point was referred to by Mr. A. F. Berry in the discussion on Mr. Rayner's paper.* He stated that such enamels would stand high temperatures, and suggested their use as a means of reducing the cost of production. Engineers generally would welcome further information respecting such coils from those in a position to give it.

2. *Coils in position on machine—machine stationary.*—The variation of temperature rise with watts per sq. cm. is plotted in Fig. 4, and two typical heating coefficient curves in Fig. 5. Fig. 4 gives results on a greater variety of coils than Fig. 2, more data and tests being available. A comparison of the two sets of curves, however, such as may be obtained at a glance by placing a tracing of Fig. 2 over Fig. 4, is instructive, as showing the effect of the iron core upon the heating of the coil.

Coil I of Fig. 2 and Coil VI. of Fig. 4 are comparable, being constructed in much the same manner. At a temperature rise of 50° C., Coil VI. shows 10° greater heating than Coil I. This must be partly due to the fact that Coil VI. is considerably larger than Coil I., and also to the general tendency of coils which are made to fit loosely over the core in the usual manner to show a slightly greater temperature rise when placed in position.

Also, tests on Coil IV. are plotted in both figures, and the curves show that at 50° C. the coil heats about 3° more on the core than when supported in air. This coil is wound on a special former, the

* *Journal Institution of Electrical Engineers*, vol. 34, 1905, p. 704.

end plates of which project inwards about $\frac{1}{4}$ in., thus leaving a clear but practically unventilated space between the main part of the former and the core.

Coils I.γ and 6 also show slightly greater heating when placed in position on the core. In all cases, however, this increase of temperature rise is small, and the results justify the system of basing the heating coefficient on the total surface of coil and former.

Coil V. shows the least heating. It was the smallest coil tested,

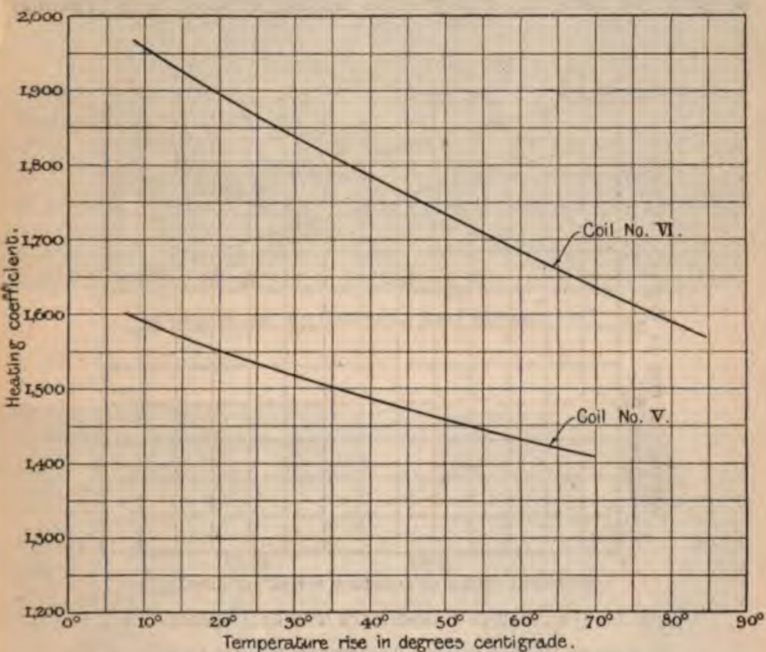


FIG. 5.—Heating Coefficient of Coils in Position, Machine Stationary.

	Total Surface.
Coil No V. Wound on Zinc former ...	1,815 sq. cm.
Coil No. VI. No former, very little covering	3,400 sq. cm.

and was wound on a zinc former fitting very closely to the core. I think it likely that the method of making the former of metal and fitting it very closely over the iron core may cause the coil to run a little cooler when placed in position than when freely supported in air. Unfortunately I was not able to test this coil off the machine. Coil VI. (without former) was practically only in contact with the core at the corners. As will be seen from Table I., the motor Coil VI. was wound with larger wire than the generator Coil VI.*a*, and the results obtained, taken partly on one machine and partly on the other,

show experimentally by their close agreement that the heating coefficient does not depend on the size of the wire. The curves also show the tendency of large coils to heat more than small ones. Experiments to determine the variation of heating due to this cause, which could be carried out on a standard line of machines, would be extremely useful.

A comparison between the figures which I have derived from Mr. Rayner's tests on Coil I.γ and my own tests on Coil VI. is interesting. The coils are of comparable size, and neither of them is wound on formers. The 5° extra heating of Coil I.γ is probably

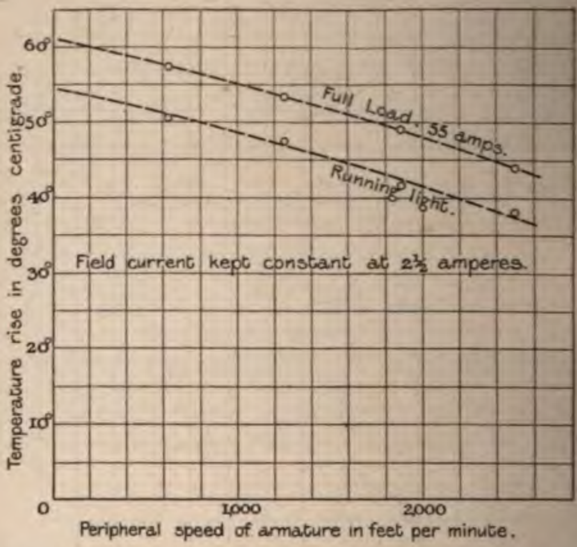


FIG. 6.—Variation of Temperature Rise with Speed, Coil A.

Two-pole machine with armature above magnets, tested by Mr. E. Brown. See Table II.

due to the extra covering and to the fact that the motor which Mr. Rayner tested was possibly of a much more enclosed design than the machine illustrated in Fig. 11, to which Coil VI. belonged.

3. *Machine running light at normal speed.*—The ventilation caused by the rotating armature must vary considerably under different conditions. It certainly varies with the general design of the machine and with the peripheral speed of the armature. It probably varies also to an extent which is quite appreciable with the air-gap, the ratio of pole surface to pole pitch, and the design of the armature end connections. In reference to the latter, the direction of rotation of the armature may also affect the ventilation.

It seems, therefore, that the temperature rise of field coils when the

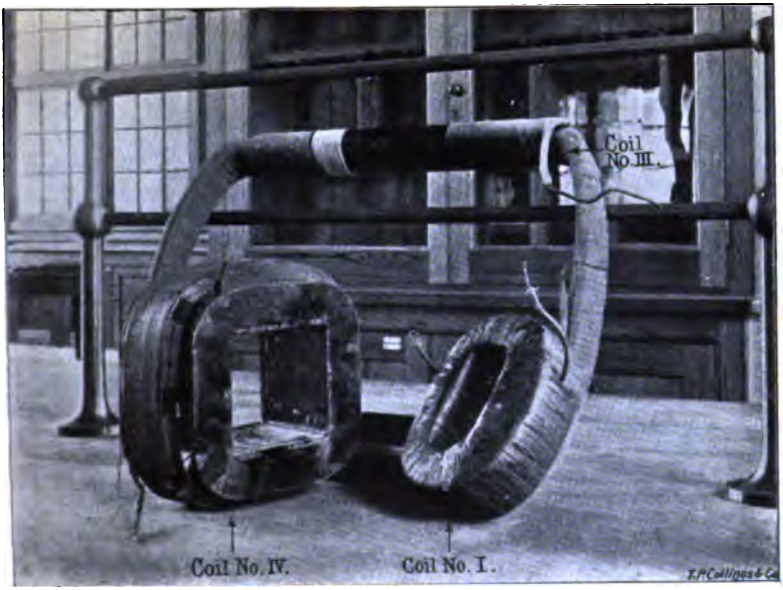


FIG. 8.

FIG. 9.—Two 10 B.H.P. 220-Volt Semi-enclosed Motors, Coil No. IV.

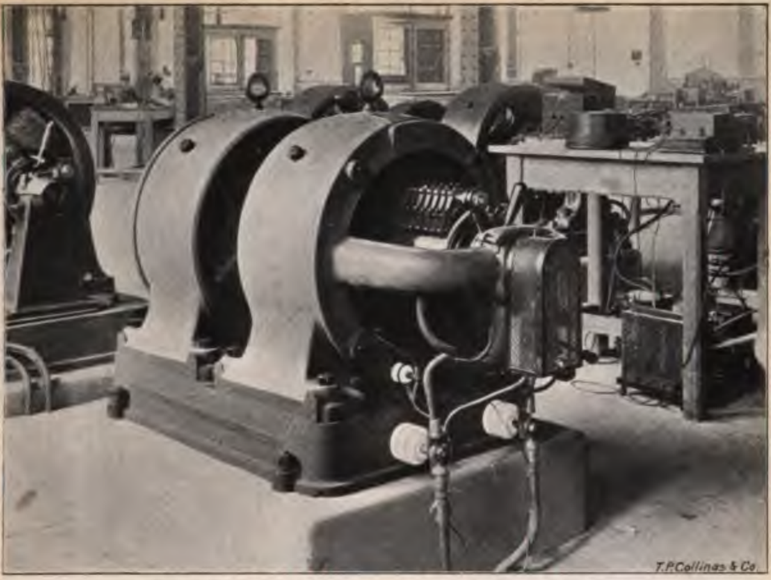


FIG. 10.—$7\frac{1}{2}$-k.w. Motor-Driven Booster, Coil No. V.

FIG. 11.—24-k.w. Motor-Generator Set, Coil No. VI.

machine is running cannot readily be predetermined, unless based on experiments made on machines of similar design.

Mr. Rayner made a number of tests on machines running light, the coils of which he had previously tested with the machine stationary. These comparative results would be of value, but unfortunately we have no details of the construction of the machines. To show the variations which do exist, I have estimated from his figures the ratio of the temperature rise of the coils when the machine is running to that which occurs when it is stationary. I have taken as a common

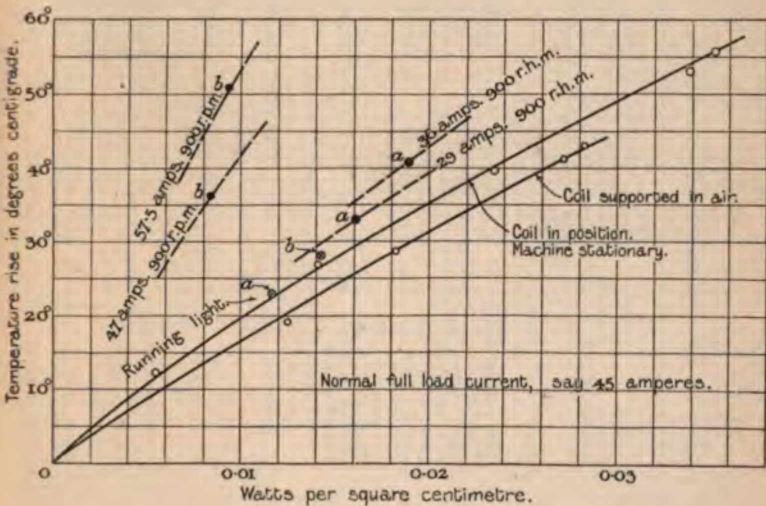


FIG. 7.—Variation of Temperature Rise of Coil IV.

Two 10-B.H.P 220-Volt semi-enclosed motors, 1,000 r.p.m., fitted with commutating poles.
For details of Coil see Table I.
Motor A was run in the direction giving the better fanning action.
Motor B was run in the direction giving the worse fanning action.

basis in each case the watts per sq. cm. which give a rise of 50° C. when the machine is running.

Coil 1.	26½-k.w.	shunt motor,	850 r.p.m.	=	0·80
„ 2.	75-k.w.	„ „	305/550 „	=	0·55
„ 6.	200-k.w.	„ dynamo,	350 „	=	0·75

Mr. E. Brown's tests on a small 2-pole horseshoe-magnet type machine (a few particulars of which are given in Table II.), show to some extent the variation of coil heating with speed. From his temperature-grade curves I have estimated the mean temperature rise of the coil, and have plotted these figures against the peripheral speed of the armature, as shown in Fig. 6. These curves show the kind of variation which may be expected in similar machines.

[redacted]

TABLE I.

TESTED AT THE BIRMINGHAM UNIVERSITY.

Covering of Coil.	Former.	Total Surface of Coil and Former in Sq. Cm.	No. of Turns per Coil.	Size of Wire.	Insulation of Wire.	Resistance per Coil at 0° C.	Remarks.
Thin presspahn adjacent to coil. Taped and varnished	Nil	960	2,270	21 S.W.G.	S.C.C.	30·050	
A covering of mica. Then taped and varnished	Nil	1,075	—	19 S.W.G.	D.C.C.	40·000	
Thick mica insulation round embedded portion. Taped	Nil	2,530	—	11 S.W.G.	D.C.C.	0·227	Stationary armature coil of alternator
Varnished	Sheet iron	2,100	3,010	1·065 mm. diam.	D.C.C. 10 mils	33·310 33·420	Two similar machines, each of which was tested
Varnished	Sheet zinc	1,815	—	—	—	29·850	Generator of a motor-driven booster set
Partly bound with tape and string	Nil	3,400	655 445	No. 12 S.W.G. " 13 S.W.G.	S.C.C. 8 mils	3·000	Combined motor-generator set
Ditto	"	"	600 900	" 13 S.W.G. " 14 S.W.G.	S.C.C. 8 mils	5·825	

and of Machines IV., V., and VI., see Figs. 8, 9, 10 and 11 respectively.

TABLE II.

Details of the Coils Used by Mr. E. H. Rayner, Mr. E. Brown, and Messrs. Neu, Levine, and Havill.

No. of Coil.	Reference.	Type of Machine.	Capacity of Machine.	Normal Speed in Revs. per Min.	No. of Poles.	Excitation Voltage.	Covering of Coil.	Former.	Estimated Total Surface of Coil and Former in Sq. Cm.	Remarks.
1a	Mr. E. H. Rayner *Journal Inst. Elec. Eng.*, 1905, vol. 34, p. 613	Shunt Motor	26·5 k.w.	850	4	440	Empire cloth and tape ...	Nil	2,620	
1γ		"	26·5 k.w.	850	4	440	" " ...	"	"	
1δ		"	26·5 k.w.	850	4	440	Nil	"	"	Berrited d.c.c. wire
2		"	75·0 k.w.	305/550	6	550	Two layers tape	"	5,270	Coil impregnated
3		"	75·0 k.w.	600	6	500	Varnished only	Sheet iron	4,020	
4		Compound Motor	82·5 k.w.	265/360	6	500	3 layers cambric ... 4 layers boot webbing	Nil	3.320	Series winding all at one end of shunt coil
5		Shunt Generator	147·0 k.w.	180	4	210	Varnished only	Brass	13,000	
6		"	200·0 k.w.	350	6	220	Varnished only	Cast iron	11,320	
8		"	500·0 k.w.	320	8	480/600	Canvas, paper, leatheroid, and string, totalling ⅛ in. ...	Nil	11,700	
9		"	500·0 k.w.	200	10	200	Canvas, fuller board, millboard, and string	Brass and sheet iron	12,400	
A	Mr. E. Brown, *Journal I.E.E.*, 1901, p. 1159	Shunt Motor	7·5 k.w.	1,250	2	135	Nil	Part wood		
B	Messrs. Neu, Levine, and Havill, *Elec. World and Engineer*, 1901, p. 56	Shunt Motors	1 B.H.P. 5 B.H.P.	—	2	—				

The total surface of the coils used by Mr. Rayner was estimated from the drawings given in his paper.

The two semi-enclosed 10-H.P. motors to which Coil IV. belongs show very little advantage due to the revolving armature (see Fig. 7). This is probably due to the enclosed design, and to the proximity of the commutating poles with which these machines are provided. In connection with the tests on these two machines it was noticed that, when running, the coils on one machine heated appreciably more than those on the other. The machines are of exactly similar design, and are mounted in line on a common foundation (see Fig. 9). A clutch is provided for coupling them together, half of which is keyed to each motor spindle. The commutator end is thus in each case outside. The machines are wired so that they run in the same direction. The armatures consequently revolve in opposite directions with respect to the slope of the free portions of the armature winding, and it is noticeable that the machine (motor *a*) running against this slope works appreciably cooler than the other (motor *b*). The pressure of the air entering and leaving the machine was measured by means of a sensitive pressure gauge, and was found to be appreciably greater in the case of machine *a* than with machine *b*.

4. *Machine running fully loaded.*—The heating of field coils when the machine is fully loaded depends to a large extent on the temperature rise of the armature, and also upon the efficiency of the ventilation under full-load conditions. The figures for the heating coefficient consequently vary considerably.

TABLE III.

HEATING COEFFICIENTS OF MAGNET COILS OF FULLY LOADED MACHINES FOR A FINAL TEMPERATURE RISE OF 50° C.

Type of Machine.	Output.	Voltage.	Speed.	Covering of Coil.	Former.	Reference to Tables I. & II.	C_h.	Sq. Cms. per Watt.	Watts per Sq. Cm.
Two-pole shunt motor, armature above magnets	7½ k.w.	135	1,250	Nil	Part wood	A	1,025	20·5	0·049
Four-pole semi-enclosed shunt motor	10 H.P.	220	1,000	Nil	Iron	IV.	3,300	66·0	0·015
Six-pole shunt motor	75 k.w.	550	325	Two layers tape	Nil	2	1,190	23·8	0·042
Ditto	75 k.w.	500	600	Varnished	Sheet iron	3	1,220	24·4	0·041
Six-pole compound motor	82·5 k.w.	220	350	Cambric and boot webbing	Nil	4	2,380	47·6	0·021
				Above removed			1,870	37·4	0·027
Eight-pole shunt generator	500 k.w.	480/600	320	Canvas, paper, leatheroid, and string	Nil	8	1,565	31·3	0·032

Fig. 6 shows the increase of heating due to full load in the case of the machine tested by Mr. Brown to be about 15 per cent. at normal speed.

Fig. 7 shows the heating with various armature currents of the field coils of the 10-B.H.P. semi-enclosed motors illustrated in Fig. 9. The rapid increase with overload is probably due in a large measure to the close proximity of the series-wound commutating poles.

In Table III. will be found the values of the heating coefficient for fully loaded machines of which data are available. In each instance the total surface is taken into account, and the figure is quoted for a final temperature rise of 50° C. The figures for the sq. cms. per watt and the watts per sq. cm. are also given.

The variation in these figures, and the consideration of the numerous and uncertain conditions introduced by the rotating armature, show that it is not possible at present to fix any very definite figures for the heating coefficient of fully loaded machines.

Conclusion.—The tests and figures here recorded confirm the following points which have already been pointed out in connection with the heating of magnet coils :—

(*a*) The maximum temperature rise of a magnet coil seldom exceeds 1·2 times its mean temperature.

(*b*) The effect of the covering of the coil on its temperature rise is very marked. Such coverings should be reduced to a minimum, as also should the insulation thickness to core or former.

(*c*) A large coil heats slightly more than a smaller one for the same watts per sq. cm.

(*d*) Berrited wire produces a considerable reduction in the temperature rise.

I have also come to the following additional conclusions :—

(*e*) The heat removing property of the iron core differs very little from that of the surface of the coil exposed to air, and consequently it is best to consider the whole surface of the coil and former when dealing with the heating coefficient.

(*f*) The heating coefficient drops considerably with increase of the final temperature rise.

(*g*) From the point of view of cooling, a slight advantage is obtained with a coil wound on a metal former.

(*h*) A great deal may be done to secure cool running by cultivating a draught by means of the armature end connections, and by running the machine in the direction giving the maximum fan action.

Taking into account the whole of the data here given, I would suggest the following approximate values of the *heating coefficient* for a 50° C. rise, based upon the whole surface of the coil :—

TABLE IV.

	C_h	Sq. cms. per Watts	Watts per Sq. Cm.
1. *Supported in Air.*			
Medium size plain coils taped	1,500	30·0	0·033
" " " " mica and tape	1,650	33·0	3·030
" " " " heavily covered	1,750 to 2,250	35·0 to 45·0	0·029 to 0·022
" " coil, wound in metal former uncovered	1,500	30·0	0·033
" " berrited d.c.c. wire...	1,150	23·0	0·043
2. *In Place on Machine.*			
Medium size plain coil	1,750	35·0	0·029
" " wound in metal formers which slip loosely over core	1,550	31·0	0·032
3. *Machine Fully Loaded.*			
Small 2-pole machines, armature above magnets	1,000	20·0	0·050
Moderate size protected motors	2,000	40·0	0·025
" " semi-enclosed motors	2,500	50·0	0·020
" " " " with commutating poles	3,000	60·0	0·017
Open type machines, 50 k.w.	1,250	25·0	0·04
" " 500 k.w.	1,500	30·0	0·033

The heating coefficient is undoubtedly a very important figure, and I trust the few points brought forward, more particularly the variation of the coefficient with different types of coils, and with the actual temperature rise, will prove useful.

Discussion.

Mr. Lea. Mr. Henry Lea: I think that Mr. Lister's method of ascertaining the mean temperature of the coil by means of its resistance is a satisfactory way, but in order to compare the mean with the maximum the latter temperature must also be obtained. In carrying out such measurements a potentiometric method of determining the voltage would obviate the liability to error referred to. I should like to give a few figures relating to the radiating properties of iron pipes, a subject closely connected with that of the paper. I found that a square foot of hot iron will dissipate 2·14 thermal units per hour per 1° F. difference of temperature. Taking 70° F. (39° C.) as the temperature rise, which is the figure I always specify, it can be shown that 20·9 sq. cms. of such surface will be required to dissipate 1 watt per minute, and that the corresponding heating coefficient is $20{\cdot}9 \times 39 = 815$. The difference between this coefficient and those given in the paper would be accounted for by the

iron pipe being bare and the wire coil covered with non-conductor. I should like to understand the difference between the values of the heating coefficient given for the small 2-pole shunt motor in Table III. (1,025) and that in Table IV. for medium coils wound on metal formers and in place on machine (1,550). Also I do not know why the author has adopted so high a temperature rise as 50° C. (90° F.). Machines should not get so hot as that at full load. It leaves no margin for overload. Mr. Lea.

Mr. A. M. TAYLOR: I gather that the author suggests that the measurement of the temperature rise of field coils by means of increase of resistance should be considered the standard method. He has referred to the fact that it is now the recognised method in Germany. This being the case, I think it probable that it will soon be adopted here. The manner in which the curves in Fig. 3 fall as the temperature increases is interesting. This may be due to conduction effects, which would tend to increase with the increase of temperature. The author has considered the question of temperature rise rather from the point of view of the field coil of a commercial motor, but it might also be considered with regard to the stationary coils of large alternators. I have been much interested in this question lately particularly as to how the temperature rises when the peak load comes on, the generator having been running under normal full load conditions for some hours previously. Designers may, not improbably, soon have to meet a demand for a generator to give 100 per cent. overload during the short period of peak load. Mr. Taylor.

Mr. R. ORSETTICH: I regret that the author has found it impossible to obtain more exact information as to the behaviour of coils when the machines are running either light or fully loaded, as these are the important conditions from the point of view of the manufacturer. I believe this to be the first paper in which prominence has been given to the important question of the effect of ventilation. A proper consideration of this detail should enable the designer to save a considerable amount of copper in the case of high-speed machines. Machines by different makers of course give different results. At the same time the subject is well worth investigation. I do not use the heating coefficient in the way shown by the author, but work with the outside surface only. By obtaining the coefficient for each type of machine and plotting against speed, the ventilation of the machine is taken into account. Mr. Orsettich.

Mr. L. MURPHY: In the early part of his paper the author states that he only finds a slight difference between the temperature rise of a coil when suspended in air and when placed on the iron core. In some experiments which I carried out on this point I found that the effect of the core on the final temperature was, as he states, slight; in fact, it was found that the coil might heat more or less when on the core than off, according to the shape of coil used. Mr. Murphy.

If, however, the temperature rise of the coil be considered with regard to time, it will be found that there is a marked difference, and

Mr. Murphy.

that on the core the coil will heat more slowly than when suspended, owing undoubtedly to the heat being absorbed into the mass of the pole core.

In the case of heavily loaded coils a difference of 30 per cent. in the temperature rise may be obtained, due to this cause, during, say, a twenty minutes' run. The point is of importance in connection with series motors working under intermittent loads, such as tramcar and crane motors. In such machines it is desired that the final temperature shall not exceed a specified amount under varying conditions of load. In order to attain this end, it is advisable to wind the field coils as close as possible to the iron core, and to avoid the use of any isolit, wood, or other formers which will act as heat insulators between the coils and the core.

The author does not state the colour of the coils used in his experiments; I think he will find a considerable difference between a black coil and a varnished one.

The number of variables which he has to consider in order to obtain a heating coefficient for any type of machine is enormous, and in practice I find that for totally enclosed machines some approximate figure may be found experimentally for each size of machine which represents its maximum allowable watt loss for any condition of winding and speed, that for open-type generators Esson's constant usually gives correct results, and for semi-enclosed machines an intermediate factor must be chosen, which depends on the degree of ventilation afforded, and can be generally predetermined from experience with fair accuracy.

Mr. Smith.

Mr. C. F. SMITH: I think that the author of this paper has treated his subject from a point of view which is much to be commended, inasmuch as his purpose has not been to make a series of experiments of his own, and to found a practical rule on these without reference to the work of other experimenters. The constant comparison of his results with those of Messrs. E. Brown, E. H. Rayner, and others, and the plotting of their results side by side with his own, is a specially valuable feature of the paper.

With regard to the cooling effect of the surface of the coil which is next to the iron, it would seem that a good deal of uncertainty still exists as to the relation between the effectiveness of the conducting power of the iron and the radiating power of the surfaces exposed to air. Probably it would be found that the general dimensions of the magnetic circuit would have considerable influence on the heat-conducting power of the pole, since they would determine the rate at which the pole could get rid of heat given to it by the winding, as well as by the armature, etc.

One would naturally expect that the effect of a metal former would be materially to aid the conduction of heat from the magnet-winding to the cooler pole on which the former is mounted. From actual measurements it would appear, however, as if the metal former was far more effective in promoting the cooling of the coil when so arranged as

to leave an air-space between itself and the pole. The ventilation thereby produced appears to be far more effective as a cooling agent than the increased heat-conductivity due to metal-to-metal contact between the former and the pole. Mr. Smith.

One difficulty in deciding absolutely on a heating coefficient which can be applied to coils of all forms is the variation in the ratio maximum temperature : mean temperature. In Mr. Rayner's values there is an extreme variation of about 10 per cent. in this ratio. As pointed out by Mr. Goldschmidt in the discussion following his paper there is a practically constant relation between the differences (mean — external) temperature and (maximum — external) temperature.

Thus the difference between maximum and outside temperature is practically 50 per cent. higher than the difference between mean and outside temperature. By measurement of the mean and external temperatures it is thus possible to estimate closely what the maximum temperature inside the coil is. From this point of view it might be well if engineers would record both mean and external temperature rises when testing their machines.

Dr. R. T. GLAZEBROOK (*communicated*): The general result of the author's interesting paper seems to be that, as he points out on page 402, there is no such constant as the heating coefficient as he defines it, and it appears to me that this is what might be expected. Dr. Glazebrook.

The simplest assumption which can be made for the amount of heat lost by radiation from a surface element S which is at a temperature t° above its surroundings is that the loss per second is $h.t.$S., where h is the radiation constant, or exterior conductivity.

Experiment shows that this law of Newton's holds reasonably for moderate differences of temperature. The constant h depends on the nature of the surface, and there is no evidence to show that it should be the same both for the exposed portion of a coil and that in contact with the core; in fact, the reverse is probably the case. Assume, however, that h is a constant over the whole surface. Then the total loss per second $= \int h.t.ds = h\,S\,\bar{t}_s$ when $\bar{t}_s$ is the mean excess of temperature over the surroundings of the surface layers not of the whole coil.

If the temperature is steady, then the total loss must be equal to the watts expended; thus we have—

$$\text{Watts} = hS\bar{t}_s \text{ or } \frac{1}{h} = \frac{S}{\text{Watts}}\bar{t}_s = S_W \bar{t}_s,$$

using the author's notation.

Thus $1/h$ corresponds to his C_h, the heating constant, but the temperature in the equation is not the mean temperature of the coil, which is measured by the change in resistance, but the mean temperature of the surface of the coil, which is quite different. Of course, if there were a constant ratio between these two mean temperatures, then the law given in the paper would follow, but I think Mr. Rayner's curves show that there is no such constant ratio.

Again, I feel sure it is a mistake to assume that the coefficient is the

Dr. Glazebrook.

same for all the surfaces of the coil. Mr. Goldschmidt called attention to the fact that Mr. Rayner's curves of temperature distribution are parabolas. If the loss is the same outside and inside, then the vertices of these parabolas would be midway between the outer and inner surfaces, and this is not found to be the case.

In the discussion on Mr. Rayner's paper (*Journal*, vol. 34, p. 711), I examined his results and showed how values for both the interior conductivity k and the exterior conductivities, or radiation constants, h_1 h_2 of the coils, may be found—h_1 relates to the outer surface, h_2 to that in contact with the core. The two quantities differ notably, h_2 being of the order of $\frac{1}{2}$ h_1, so that it is impossible to determine the mean rise of temperature from a knowledge of the surface and the watts by means of a single coefficient.

Of course, this turns on Newton's law of cooling. If we had assumed Stefan's more complete law the results would be still more complex, but they would be of the same nature, and it appears to me that any generalisation as to a figure to be adopted for all coils of a certain size is hardly justified.

As I have already said, the $1/h$ of my formula is the S_r, or heating constant, defined, however, with reference to the mean rise of surface temperature; but in comparing the actual numbers it must be remembered that my h relates to a square inch, while Mr. Lister's S_r refers to a square centimetre. Hence, taking 6·45 sq. cm. to the square inch, we have $C = 6{\cdot}45/h$, and from this we get the following table, the values of h_1, h_2 being taken from my table on page 718, vol. 34 :—

Coil.	h_1.	h_2.	C_1.	C_2.
1	0·0110	0·0059	585	1,090
2	0·0138	0·0060	465	1,075
5	0·0138	0·0091	465	710
6	0·0350	0·0067	185	965
6*	0·0102	0·0063	630	1,020
8	0·0084	0·0031	770	2,080

* In this experiment the coil was stationary.

The variations in the value of C are very great. In the case of coil 6, the fanning action when the machine was running was extreme; thus h_1 is very large, C_1 very small. In any case the mean surface rise of temperature is to be found by dividing C by the surface through which the heat is being radiated, and multiplying the quotient by the watts transmitted through that surface.

The figures seem to me sufficient to show that no such thing as a "heating coefficient" can be found which is applicable to all coils of the same size. What can be done possibly is to obtain by careful experiment the values of k, h_1, and h_2 for coils similar in design and

material to those which are to be used, and to base calculations applicable only to such similar coils on these figures. Dr. Glazebrook.

Dr. ALFRED HAY (*communicated*): The author's interesting and valuable contribution to our knowledge of the temperature rise in magnet coils once more emphasises the extreme complexity of the problem and the insufficiency of the data on which to base a reliable predetermination of the probable temperature rise. Dr. Hay.

The relative importance of the parts played by radiation, convection, and conduction, still seems to be obscure, but from the author's experiments on the cooling of a coil when supported freely in air and when placed around its core, it would appear that conduction and convection are quite as effective in producing cooling as radiation.

Again, the marked influence of good ventilation points to the fact that convection is an extremely important factor. No experiments of a systematic kind appear to have been undertaken for the purpose of ascertaining the relative importance of radiation, conduction, and convection.

In the case of a coil freely exposed to the air over its entire surface (in which case the conduction effect is negligible), the radiation effect could to some extent be differentiated from the convection effect by experiments of the following nature: Let the temperature rise of the same coil be determined when its surface is coated with various materials—say lampblack, white paint, and a very thin and highly polished shield of sheet-metal. The variations in the temperature rise would then be accounted for by changes in the rate of radiation due to the varying nature of the radiating surface. In a similar manner, by using a coil of the same material in each case, but varying the actual cooling area by having the coil wound, first, evenly in layers, and then in sections of varying depth, so as to produce channels or corrugations in the external surface, the changes in the temperature rise corresponding to the various arrangements of the external surface of the coil would be due to changes in the *convection* factor since the effective *radiating* surface is not affected by the corrugations.

Mr. H. M. HOBART (*communicated*): I wish to call attention to the range of figures quoted on the first page of the paper. The range of 355 to 1,500 for the temperature rise in degrees Centigrade per watt per square centimetre is at first sight misleading, and there is really more uniformity in the value of the "heating coefficient" than appears at first sight. Mr. Hobart.

The figure 1,500 quoted by Goldschmidt is reckoned on the *whole* surface of the coil taken as cooling surface and on the rise as determined by resistance measurement. Reckoning this figure on the *external* cylindrical surface of the coil would reduce its value by about 60 per cent., which gives $1{,}500\,(1-0{\cdot}6)=600$.

The rise by resistance measurement may be taken as 1·65 times the thermometrically determined temperature rise at the outer surface of the coil, and thus the value 600 should be further reduced to

Mr. Hobart. $\frac{600}{1{\cdot}65} = 364°$ C. per watt per square centimetre of external cylindrical surface of the coil, thermometrically measured. This value is in good agreement with the value 355 given by Esson, and is also, with due regard to the numerous factors which influences the heating, in fair agreement with Neu Levine and Havill's * value of 720 by resistance measurement, which reduces to $\frac{720}{1{\cdot}65} = 435$.

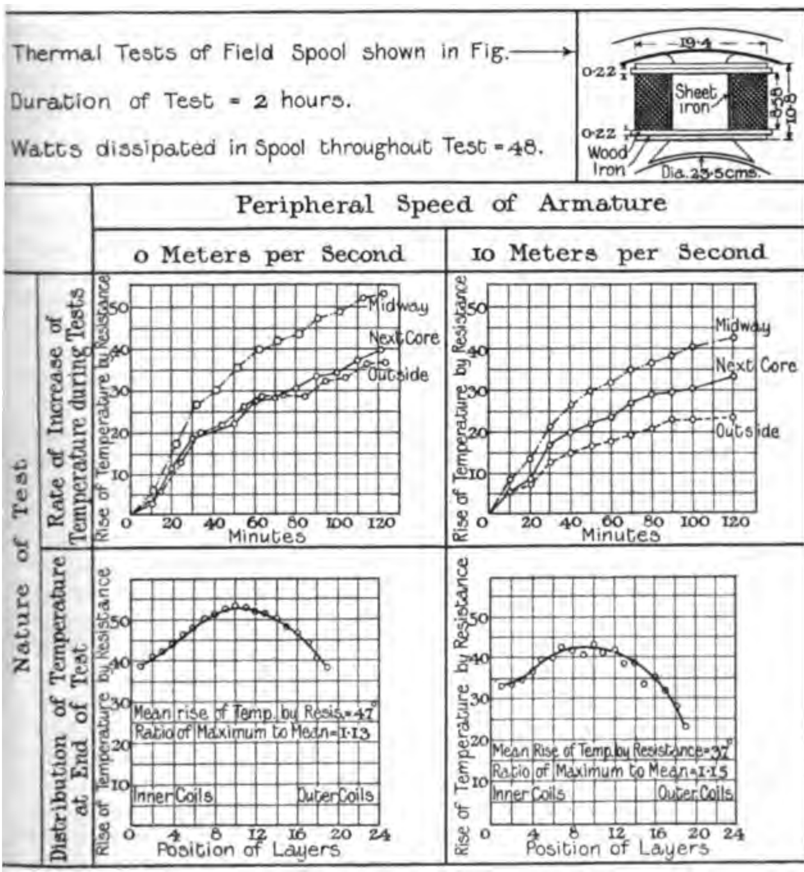
In the year 1898 Mr. R. C. Clinker, at my request, made quite a large number of tests concerning the thermal occurrences in field coils. Although these were quite comprehensive, and were published in *Electric Generators* and in *Electric Machine Design*, pp. 97–105, their existence does not seem to be known. I allude to these tests, as they throw interesting light on a number of questions discussed in Mr. Lister's paper.

In the accompanying figure I have arranged a few of the results of Mr. Clinker's tests in a form which brings out the bearing of the peripheral speed of the armature upon the temperature rise of the field coil, and also shows the way in which the temperature rise varied at different parts of the coil. We see from this figure that when the armature was at rest the layer next to the magnet core and the outside layer had practically the same temperature, but that when the armature was running at a peripheral speed of 10 metres per second the outer layer had a much lower temperature than the layer next the magnet core. It is very important to avoid taking the result of tests on a single coil as guidance for other cases. According as a coil is completely bound up in insulating materials on the one hand, or left exposed to the air on all sides on the other hand, the temperature rise and the temperature distribution become greatly modified. In a general way, however, it will be seen that Rayner's † later tests, in which he obtained a ratio of maximum to mean temperature as determined by resistance measurements of about 1·15, confirm fairly well the results of Clinker's 1898 tests.

Another very exhaustive series of tests to which but scant allusion has ever been made is that carried out by Dettmar ‡ in 1900 on the effect of temperature on the cotton coverings of copper wires. These tests showed that in the course of time a temperature of less than 100° C. caused decided deterioration of the cotton coverings. The more recent tests of the National Physical Laboratory on large numbers of insulating materials showed that in the case of all these materials deterioration set in at a temperature of not over 125° C., and in most of these materials the temperature at which deterioration occurred was considerably below 125° C. With a knowledge of these results it is certainly undesirable that the highest temperature existing in any part of a coil wound with cotton-covered wires should be sub-

* *Electrical World*, vol. 38, 1901, p. 56.
† *Journal*, vol. 34, pp 613–659.
‡ *Elektrotechn. Zeitschr.*, vol. 21, August, 1900, p. 727.

jected to temperatures of much over 100° C. Suppose we set this maximum temperature at 105° C. If the hottest part of the coil is at a temperature of 105° C., the mean temperature, as determined by resistance measurements, will be about 90° C. In a case where the temperature of the surrounding air is the very frequently occurring value of 30° C., it is evident that the temperature rise, as determined by Mr. Hobart.



resistance measurements, should not be more than 60° C. Mr. Clinker's tests, and also the subsequent tests made by the National Physical Laboratory, show that in many cases the temperature rise, as determined by resistance measurements, often exceeds by more than 60 per cent. the temperature rise as determined by thermometric measurements. Thus the thermometrically determined temperature rise should not be permitted to exceed 37½° C., and it is evidently not advisable to

Mr. Hobart. raise the present customary permissible temperature rise of 45° C., but rather to reduce this.

Allusion is made in the paper to a coil in which the cotton coverings of the wires were impregnated with a certain material which resulted in a decreased temperature rise. I am of opinion that the first earnest attempt to develop and properly apply such materials was made by Mr. H. W. Turner, about the year 1901. At any rate it was at about that time that I learned from him of the excellent results which he was obtaining in his work along these lines. It has been difficult to demonstrate the effectiveness of such a material as regards decreasing the temperature rise, owing to the fact that it tends to make all parts of the coil assume a more nearly uniform temperature, *i.e.*, when such a material is used, the mean temperature as determined by resistance measurement exceeds by a smaller amount the temperature as determined by thermometric measurements than would otherwise be the case. In coils in which such materials are employed, a thermometrically determined temperature rise of 40° C. would correspond to a lower internal temperature as determined by resistance measurements than in a coil in which such materials are not used, but the thermometrically determined temperature rise of the surface of the coil would be but slightly decreased.

For these and other reasons I am strongly in favour of the introduction of the general practice of determining the temperature rise by resistance measurements instead of by thermometer. Were this practice general, a temperature rise of 50° C. above surrounding air would be thoroughly conservative, but so long as the temperatures are determined by thermometric measurements, a "nominal" temperature rise of from 40° to 45° C. above surrounding air is already rather higher than it is desirable to permit. The trouble is that no individual engineer can introduce such a comparatively new practice as that of invariably expressing the temperature rise of field coils in terms of the results of resistance measurements. Should any designing engineer attempt to do this, he would bring upon himself the charge of employing higher temperatures than other designers who quoted their temperatures on the basis of a thermometrically determined temperature rise, whereas the former designer would actually be working to lower temperatures than the latter designer.

This is only one instance among many others which render it highly desirable that the issuing of standardisation rules for temperature rise should not continue to be delayed. Such rules should be promptly framed and in accordance with the best present practice; they may be readily revised from year to year in accordance with the constantly accumulating available experience.

It is very difficult to generalise as to values for the degrees Centigrade temperature rise per watt per square centimetre of radiating surface, even in cases where it is definitely stated that the temperature rise is determined by resistance measurements, and that the surface is the external cylindrical surface of the coil. There still remains a

number of considerations making it very difficult to frame any general conclusions. Thus, for example, the effectiveness of ventilating ducts in armatures has gradually come to be admitted on all hands. This has been followed by the gradual introduction of a ventilating space between the magnet core and the field coil. Still better results are obtained by additional ventilating ducts through the midst of the field coil. I myself prefer to tabulate values of the temperature rise per watt per square centimetre of external surface of the coil for each of these alternative arrangements and for various peripheral speeds of the armature for each arrangement. Even then, however, one must remember that the influence exerted by the rotating armature on the temperature of the field coil depends very greatly upon the means adopted to ventilate the armature, and upon the consequent amount and temperature of the air passing from the armature to the field. The use of interpoles is decidedly disadvantageous from the thermal standpoint, for they obstruct the emission of heat from the field coils, and introduce an additional considerable loss through the heat generated in their own windings. Thus, each of the constants indicated above must be modified according to whether the design does or does not employ interpoles, and also according to the size of these interpoles. In general, the cooling will be more effective the greater the polar pitch at the air-gap, but there are also, of course, a great many other factors influencing the result. In the design of field coils for rotors, the rise of temperature in degrees Centigrade per watt per square centimetre of coil surface may be much greater than for stationary coils. Here again, however, it is equally impossible to generalise. Mr. Hobart.

Mr. E. H. RAYNER (*communicated*): When attempting to find some basis which would indicate the efficiency with which the field coils of very different patterns would radiate heat, it seemed to me that the temperature which would be attained if unit quantity of energy were dissipated from unit area of the coil per second was the best indication of this useful quality. For various reasons also I took the external area plus the flanges as being the radiating area. However, I think, with the author, the total area, including the surface next to the core, might well be included. One reason that I neglected it was that, in certain cases, this surface was very inefficient, and it seemed less misleading to neglect it. Mr. Rayner.

With this limitation, and using inches instead of centimetres—a concession to the usual unit of the designer in these matters—the two columns 20–22, page 655, of my paper give the heating coefficients as defined by Mr. Lister, except that column 20 gives the results using the maximum internal temperature instead of the mean temperature, which is used in column 22.

This constant for shunt coils varies from 100 to 250, or in Mr. Lister's units multiplied by $(2\cdot54)^2$, 650 to 1,600, so that the figures in Table IV., referring to open-type machines to which my experiments referred, namely, 1,250 and 1,500 for 50 and 500 k.w., are on the safe side.

Mr. Rayner.

However, these factors must, I consider, be used very carefully in any given case, and, until information is obtained from any build of coil, it is not wise to trust such coefficients obtained from other and dissimilar coils except as very rough approximations. What I would especially draw attention to is the low value given above, 650 as compared with 1,600, the upper limit. It must be remembered that these values were obtained on running machines, and, as it happens, of a similar size—500 k.w.

Now, the efficiency of the use made of the copper is roughly in proportion to these figures. In other words, a proper consideration of the design of field coils, from a radiative point of view, as well as from a consideration of the ampere-turns required, may lead to a saving of several cwt. of copper in large machines. I have also been assured that in the case of the manufacture of small machines this aspect of the subject has enabled such economies to be made in field copper as to convert a commercial loss into a profit. This emphasises the importance of any paper on the subject, and I would advise designers to see that they are not using unnecessarily too much covering, of poor heat conductivity, on their coils.

I have just brought before the Institution in the discussion on Professor Epstein's paper,* the results of some further work on the subject. Some half-dozen field coils, each with single variation of finish, were experimented on. It is not necessary to repeat the figures, which will be published in due course, but I feel that a great deal of useful work remains to be done on these lines.

The many independent variables in a number of commercial coils render any numerical deduction almost impossible, and it is only by varying one at a time that any certainty as to its effect can be deduced; and if the author could add to his work by giving results obtained on a few machines which had special coils wound for them, and investigate the many different conditions of finish met with in practice, I am sure that designers would have still more data for further economies.

Mr. Russell.

Mr. ALEXANDER RUSSELL (*communicated*): The paper is very interesting, and the experimental results given will be of permanent value to the designer. Much attention has recently been paid to devising methods of getting rid of the heat generated in field coils, but, as the author points out, the published data are of the scantiest description.

I think the term "heating coefficient" is not a good one. The number of "coefficients" and "factors" in electrotechnics is now so large that a new one ought only to be accepted as a last resource. In this case I would suggest the word "emissivity," on the analogy of "surface emissivity" in heat.

What we want in practice is the watts, that is to say, the joules per second, radiated per square centimetre of the surface of the coil

* *Journal Institution of Electrical Engineers*, vol. 38, 1907, p. 70.

per degree difference between its mean temperature and that of the surrounding objects. Theoretically it would be better to take the temperature of the outside of the coil instead of its mean temperature, but the accurate measurement of the outside temperature is very difficult in most practical cases. Mr. Russell.

The question arises, Does this emissivity vary much with the temperature? In Fig. 1 the author gives a curve showing how the watts radiated per square centimetre vary with the temperature rise for a coil supported in air. If W denote the watts radiated, S the surface, and t the mean temperature rise, the equation to the curve in Fig. 1 marked "Watts per square centimetre" is—

$$W/S = (0{\cdot}6 + 0{\cdot}0016t)t/1000.$$

The maximum inaccuracy of the results calculated by this equation as compared with the given curve is about 1 per cent. We see, therefore, that the "emissivity" in this case is $(0{\cdot}6 + 0{\cdot}0016t)/1000$, and for small values of t it may be taken equal to 0·0006 watts per square centimetre per degree difference of temperature.

For $t = 50°$ C. the "emissivity" would be about 13 per cent. greater. It may be objected that 0·0006 is a small fraction, and that engineers prefer dealing with a larger integer like its reciprocal, 1,667, and the author has given a meaning to this number. As copper melts, however, at 1,085° C., we get into difficulties when we try to imagine what happens when it gets to 1,667° C. In conclusion, I should like to point out the difference in the temperature of coils measured in this country, in America, and on the continent. In this country we generally assume in commercial testing that $\alpha = 0{\cdot}00428$, but in America they take $\alpha = 0{\cdot}0042$. The calculated temperature of the coil is therefore about 2 per cent. hotter in America. On the continent, however, α is taken either as 0·004 or as 0·0038, and hence the temperatures calculated abroad are sometimes 10 per cent. higher. The best value to take for α in commercial testing is 0·004, as this enables us to use the simple formula—

$$\theta = (R_2/R_1 - 1)(t + 250),$$

where θ is the rise of temperature in degrees Centigrade, R_1 the initial measured resistance at the temperature t, and R_2 the final measured resistance at the temperature $(t + \theta)°$ C.

Mr. C. C. HAWKINS (*communicated*): In addition to the valuable data given by the author, something might be added in reference to similar data for coils divided into sections, with ventilating passages between the sections. Each coil is divided into, say, three sections, with intervening air-gaps of ⅛ in., and the whole coil is separated from the pole by distance-pieces, so that air can freely pass radially inwards and upwards. The cross-area of each section is such that it is not far from square, or if anything the radial depth is slightly greater than the axial height. The merits of the construction are that the inner end-connections can be so easily brought out, and a thin wrapping of tape Mr. Hawkins.

Mr. Hawkins. gives perfect insulation; but also incidentally the mean final rise of the coil is made to approach much more closely to the external surface rise, and the heating coefficient for the mean final rise on the basis of calculation suggested by the author can be reduced with open dynamo coils to 800, or even as low as 700. One of the sections is conveniently confined to the series winding when the machine is compound-wound. Of course such "sectionised" coils are slightly detrimental to the efficiency of the machine, since if the intervening gaps were filled up with copper less watts would be spent. But in a large machine this has so small an influence on the efficiency that its economy recommends it.

Since almost all insulating materials are good heat-insulators, thick insulating bobbins, such as those moulded from vulcanasbest, have the disadvantage of raising the heating coefficient considerably, although they are of great convenience to the manufacturer in the process of winding. When they are used it is advisable to take the exterior surface only as the basis for cooling calculations.

One hardly likes to add any figures dealing with the rise of the exterior surface for fear of meeting with the reproach, "How very unscientific!" It is quite true that the mean rise of temperature bears more immediately upon the life and regulation of the machine. But the fact is that up to the present in the majority of specifications it still remains the rise of temperature of the exterior surface as measured by a thermometer, for which a limit is specified. It must therefore possess the greater interest to the designer so long as the acceptance or rejection of the machine depends upon it. Further, there is something to be said for it, since the test is so easily applied, and agreement between purchaser and seller follows at once from inspection of the thermometer, while the measurement of the hot resistance has to be very carefully taken if accurate results are to be obtained. In reference to *surface* rise, the figure for well-ventilated "sectionised" coils is reduced to about 465 to 400.

Mr. Lister. Mr. LISTER (*in reply*): In view of several criticisms as to the reliability of the figures given in Table IV., I would like to emphasise the fact that they are put forward as a consistent range of approximate values only, and, as stated in the paper, accurate figures for working conditions can only be obtained by direct experiment on the particular type of coil and machine under consideration, and such figures only apply to this type.

In reply to Mr. Lea, potentiometric measurements are troublesome and difficult to carry out in the case of running machinery. Good moving coil instruments will give results sufficiently accurate, and such instruments will be found in every test room. Mr. Lea's reference to iron piping is interesting. Iron pipe is probably a better radiator than the surface of a field coil, and is usually so fixed that it works under better conditions. The figure, 1,025, for the heating coefficient which appears in Table III. was obtained on a type of machine the field coils of which were very favourably placed. The figure of 1,550 in Table IV. refers to the modern multipolar protected type. Mr. Lea raised the

question of the advisable temperature rise. This has been fully dealt with by Mr. Hobart in his contribution to the discussion. Mr. Lister.

I agree with Mr. Taylor that convection may be one of the causes of the falling of the curves in Fig. 3, but it is also largely accounted for by Stefan's laws of radiation. Unfortunately I have had no opportunity of testing embedded coils.

In reply to Mr. Murphy, all the coils tested were black except Coils III. and V., which were varnished with shellac.

I am very much interested in Dr. Glazebrook's treatment of the problem. But it appears to me that in addition to determining k for each type of coil, it will be necessary to determine the exterior conductivities h_1 and h_2 under all the various conditions of type, speed, and load. Three ranges of experimentally determined figures will have to be employed, and most designers would prefer to use one coefficient only, especially as it would probably give results equally useful.

Dr. Hay's suggested tests should give very valuable data. I understand that Mr. Rayner has recently been experimenting with various coverings. I am afraid that tests on corrugated coils will not give very practical results, and probably the best way to study convective effects will be by testing a coil in various regulated currents of air. Testing the coil in place on machine and varying the peripheral speed of the armature when running unloaded is of course the practical test for any given type of machine (see Fig. 6).

I have to thank Mr. Hobart for the two references to previous work which I had overlooked. I did not suggest that Esson's 365 should be compared with Goldschmidt's 1,600 on the same basis. In fact, I gave the same reasons for the disagreement as those numerically worked out by Mr. Hobart. Mr. Hobart shows a very close agreement, but it depends upon the figures taken for the two correcting factors used. Both of these will vary with the type of machine, and in neither case is this information available. Mr. Hobart has very clearly pointed out the difference between the temperature rise as measured by thermometer and by increase of resistance.

At the conclusion of the meeting a unanimous vote of thanks was passed to the author for his interesting paper.

MANCHESTER LOCAL SECTION.

ROTARY CONVERTERS *VERSUS* MOTOR-GENERATORS.

By MILES WALKER, Associate.

(*Paper read December* 4, 1906.)

The main systems in which rotary converters have been employed can be classified broadly as follows:—

First, there are railway and tramway systems, where 3-phase power is generated for distribution and converted into direct current at 500 or 600 volts. Here, as a rule, the frequency chosen is fairly low, and the rotary converter is indisputably the best apparatus for converting.

The second use of rotaries—which in the future may become very much greater than it is now—is for electrolytic work. Here the high efficiency of the rotary converter commends it for general use.

The third great use for rotaries is in the conversion for lighting and traction systems in towns where the frequency of supply is 40 or 50 $\sim$. For the last three or four years there has been a well-established opinion in the minds of central station engineers and consulting engineers that the 50-$\sim$ rotary converter was a delicate and unstable piece of apparatus. This opinion is beginning to waver in the face of facts, and there are now 50-$\sim$ rotary converters of capacities from 150 to 1,500 k.w. running in different parts of the country in a highly satisfactory manner.

In order to compare the advantages of the rotary converter over the motor-generator for this class of work, it is necessary to take into account a great number of features, and for this purpose I have made out a table in which the advantages and disadvantages are set out in columns.

The four types of apparatus which are in commercial use at the present time are the rotary converter, the motor converter, the synchronous motor-generator, and the asynchronous motor-generator. I have included in the table the permutator, the electrolytic rectifier, and the mercury vapour rectifier—not because they are at present serious competitors, but because they may at some future time become competitors. We may, however, mainly confine our attention to the first four.

Starting.—Beginning with the question of starting, the usual practice

	Starting.	Synchronised.	Parallel Operation. A.C. Side.	Parallel Operation. D.C. Side.	D.C. Voltage. Variation with A.C.	D.C. Voltage. Variation with Frequency	D.C. Voltage. Hand Adjustment.	Compounding.	Commutation.	Overload Capacity.	Risk of Break-down on H.T. Side.	Power Factor.	Efficiency with Transformers 1,000 k.w.	Attention.	Floor Space.
Rotary Converter	Starting Motor. Start D.C. Side Self-Starting	Yes. Automatic	Good	Good	Yes. Automatic Regulator	No	Yes	Yes	Excellent	Very Great	Small	Unity	$94\frac{1}{2}$%	1	1
Motor Converter (Peebles La Cour)	Self	Yes	Good	Good	Yes	No	Yes	Yes	Good	Fair	Greater	Variable	$92\frac{1}{2}$%	—	$1\frac{3}{4}$
Synchronous Motor-Generator	Starting Motor. Start D.C. Side Self-Starting	Yes	Fair	Good	No	Yes Bad	Yes	Yes	Fair	Fair	Greater	Unity	90%	2	$2\frac{1}{4}$
Asynchronous Motor-Generator	Self	No	—	Good	No	Yes Bad	Yes	Yes	Fair	Fair	Greater	Variable	89%	1	$2\frac{1}{4}$
Permutator	Starting Motor	Yes	—	—	Yes	No	—	—	?	—	Small	—	95%	—	$\frac{1}{2}$
Electrolytic Rectifier	—	No	—	—	Yes	No	—	—	—	—	Small	—	—	—	—
Vacuum Rectifier	—	No	—	—	Yes	No	—	—	—	—	—	—	High	—	—

with large rotary converters is to employ starting motors, because experience has shown that this method of starting is exceedingly simple and satisfactory. Where, however, there is any objection to its use, it is possible to start up either on the D.C. side or on the A.C. side. For low frequency, rotaries starting up on the A.C. side without starting motors and without synchronisers can be recommended. Where it is necessary to cut down the starting current to the lowest possible minimum, ball bearings may be used with safety up to the 500-k.w. size. Ball bearings have been installed at Leeds on 325-k.w. rotaries, and have proved satisfactory. One of these rotaries will easily start on one-fifth of the voltage of supply, and the maximum current on starting up does not exceed one-third of full-load current on the high tension side. For larger sizes, if it were necessary for any special reason to start up on the A.C. side, special provision can be made in the design of the rotary to limit the starting current to a very small amount. One reason which has been brought against the use of rotary converters in traction work is that, if, owing to failure in the generating station, there is a general shut down, it is much more difficult to start up again than it is to start up with induction motor-generators which can be started without synchronising. This objection can be met by using self-starting rotary converters, such as are installed at Leeds and at other places. A self-starting rotary may have attached to it a small exciter in order that its polarity may with certainty be brought up the right way in the first instance. The position of the motor converter in the matter of synchronising is the same as the rotary. There is supplied with the motor converter a little instrument which indicates when the current in the rotor is at the minimum : after the starting switch has been closed, the current in the rotor rises and falls, depending on the change of frequency. It is just as difficult to hit off the right time for throwing in the motor converter by observing this instrument as it is to hit off the right time for throwing in a rotary converter by means of a synchroscope, and the time taken is the same under the same conditions, and depends somewhat on the skill of the operator.

The synchronous motor-generator of course can be started either by means of a starting motor or on the D.C. side, or can be made self-starting on the A.C. side when suitably designed.

The induction motor-generator, of course, does not require synchronising at all.

Parallel Running.—We now come to the question of parallel running. There is a prevalent opinion that a 50-∿ rotary converter hunts; this is true of a badly-designed rotary converter, but is not true of a properly-designed machine when working under reasonable conditions, such as can ordinarily be found in the sub-stations of towns. I have seen a 250-k.w. 50-∿ rotary running in the Polygon sub-station, Manchester, in parallel with motor-generator sets in a highly satisfactory manner; there was not the slightest suspicion of hunting. The rotary took its load with the motor-generators, and was in every way perfectly stable. The parallel operation of a 50-∿ rotary

converter when properly designed is in every respect satisfactory, and the statement to the contrary is based only on experience with antiquated machines. The parallel running on the A.C. side of a rotary converter is really considerably better than the parallel running of a synchronous motor-generator set, because the rotating part being lighter, the speed can be more easily changed to meet any sudden changes of frequency. In case of a heavy load the rotary has an enormous advantage, because the heavy load makes very little difference to the mechanical turning moment called for, whereas with a motor-generator, the turning moment is proportional to the load. In the case of a rotary, the work is done by the transformers. It is hardly felt by the running machine, which merely acts as a conductor. The disadvantage of the synchronous motor-generator in this respect is to a certain extent shared by the Peebles-Lacour motor converter. It is not true that the motor converter has a greater synchronising power than the rotary. The synchronising current taken by a rotary is only limited by the resistance of the line, and the small resistance and self-induction of its armature, whereas the synchronising current of a motor converter is limited by the self-induction both of the stator and rotor of the induction motor end. With regard to the parallel running on the D.C. side, all types stand on the same footing.

Variation of Voltage.—There are really four different questions to consider in connection with the variation of voltage. There is the question how far the D.C. voltage varies with the A.C. voltage; there is the question how far the D.C. voltage varies when the frequency varies; also how far it is possible to obtain hand adjustment of the voltage through any required range; and there is the further question of automatic compounding when load comes on.

As to the variation of the D.C. voltage with the A.C. voltage, the table shows exactly what the position is. The D.C. voltage and the A.C. voltage of both the rotary converter and the motor converter vary together. The simple motor-generator has an advantage in this respect in that the D.C. generator is entirely independent, and so long as it is running at constant speed and load gives a constant voltage. In cases where the A.C. voltage is unsteady, and it is required to preserve a constant D.C. voltage, the motor-generator will be the simplest apparatus to employ, but the superior efficiency of the rotary converter will sometimes make it worth while to introduce automatic voltage regulators, so that a steady D.C. voltage can be maintained, notwithstanding variations in the A.C. supply. Automatic regulators have been designed which will hold the D.C. voltage constant within half of 1 per cent., notwithstanding the variation of the A.C. voltage through a range of 10 per cent. When it is considered how the D.C. voltage varies with the frequency, it will be seen that the positions of the rotary and the motor-generator are reversed. The D.C. voltage of a rotary is not dependent upon the frequency, whereas with the motor-generator a change in the frequency of 1 per cent. will often make a change in the D.C. voltage of 2 per cent.

Hand Regulation of Voltage.—It is possible to carry out hand adjustment of voltage through wide ranges on all four classes of apparatus. In the case of a rotary converter, there are four methods by which the adjustment of voltage can be obtained.

The simplest method is to build the transformers feeding the rotary with a high inductive drop. The arrangement is given diagrammatically in Fig. 1. The rotary then behaves very much as a shunt generator. By increasing the excitation the voltage is increased; by decreasing the excitation the voltage is decreased. The only drawback

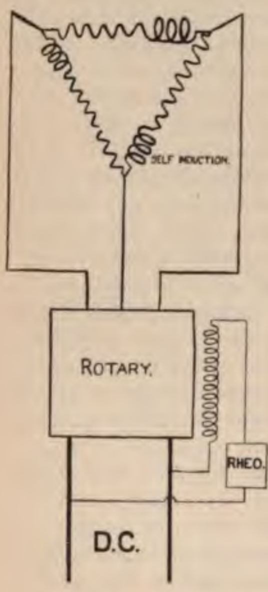


FIG. 1.

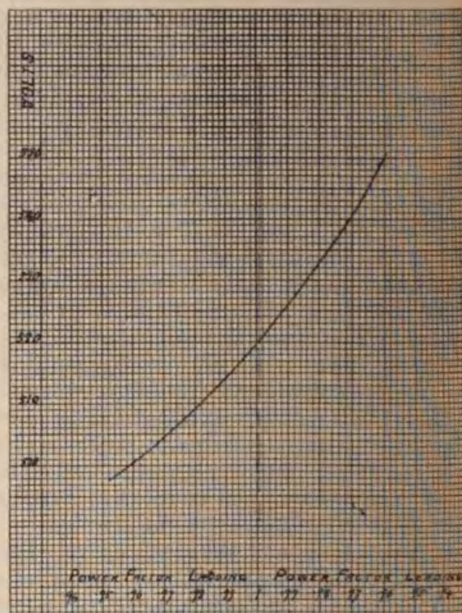


FIG. 2.

to this method is that the power factor varies as the voltage is varied. Curve Fig. 2 shows the limits within which the power factor on the high tension side varies at full load when the voltage is changed through a range of 10 per cent. It will be seen that we keep within 95 per cent. lagging and 96 per cent. leading. As it generally happens that it is at heavy loads that we require to raise the voltage, the leading current is rather a good point, as it helps out the lagging current taken by any induction motors on the system, and tends to raise the voltage of the generators. Moreover, it will be noticed that anywhere in the neighbourhood of normal voltage the power factor is near unity. It is only when the voltage is reduced below normal that the power factor comes over to the lagging side.

The second method for obtaining hand adjustment of the voltage is

to put an alternating-current booster on to the shaft of the rotary, and change the A.C. voltage supply of the rotary by varying the field of this booster. This method is shown diagrammatically in Fig. 3.

In addition, by means of this booster complete control can be preserved over the power factor, notwithstanding any change in the voltage. In all cases where a very wide range of voltage, say up to 25 per cent., is required, the booster method is to be preferred to the self-induction method. We may take 10 per cent. range in voltage as being the practical limit in this latter case, though by special arrangements it can be extended to 15 per cent.

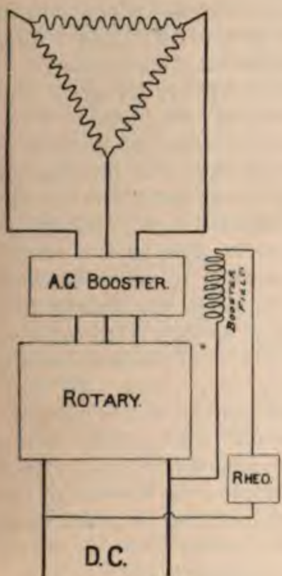


FIG. 3.

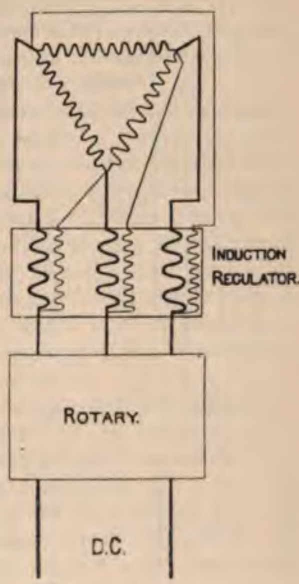


FIG. 4.

The third method is by connecting an induction regulator between the transformers and the rotary. This is shown in Fig. 4. The 1,500-k.w. 50-$\sim$ rotary converters operating at Brighton obtain their voltage regulation by means of induction regulators. One of the objections to the use of these regulators is that they require cooling by an air-blast; this, however, is not a serious objection where air-blast transformers are used. In cases where an air-blast is available, the induction regulator is to be preferred to the A.C. booster if the number of poles on the rotary is very great. It will be understood that the number of poles on the booster must be the same as on the rotary. Where the number of poles is as great as thirty—as at Brighton—the induction regulator, in which the number of poles can be made as few as we like, makes a cheaper and more compact machine.

The fourth method (shown in Fig. 5) of adjusting the D.C. voltage is to bring taps from the transformers either on the high tension side or on the low tension side, and change the working tappings by means of a dial. There will, of course, always be an objection to any system in which tappings from the transformer are brought to a dial, on account of the possibility of accidental short circuits.

Compounding.—All four classes of converting apparatus are the same in the matter of automatic compounding. It is just as easy to obtain a compounding effect of 10 per cent. between no load and full load on a rotary converter as it is on a motor-generator. This compounding can either be carried out by putting a series coil on the field of the rotary and self-induction in the transformers feeding it, or by adding a series coil to the A.C. booster fixed on the shaft.

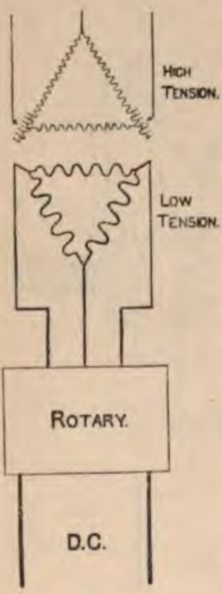


FIG. 5.

There are some cases where a reversed compound winding is useful; for instance, where it is intended to run rotary converters in parallel with shunt generators, the transformers should be designed with considerable self-induction, and a reversed series winding put on the rotary converter. This gives the rotary all the characteristics of a shunt machine. When the load comes on, the voltage falls, and by arranging a suitable German-silver shunt, the characteristics of the machine may be made to imitate very closely the shunt generators with which it is to run in parallel.

Commutation. — In all direct - current machines, good commutation is a most important feature. It is common knowledge that the problem of commutation on a rotary converter is very much easier than on direct-current machines, as there is very little field distortion by the armature current, so that excellent commutation can be obtained through very wide ranges of load. Even up to three and four times full load current can be collected from a rotary converter without excessive sparking or flashing over, and up to 100 per cent. overload can be commutated sparklessly.

The 40-∿ rotary converters supplied by the Westinghouse Company to the North-Eastern Railway Company yield a normal-rated current of 1,300 amperes. I have seen one of these machines delivering 2,500 amperes on ordinary railway service without the sign of a spark on the commutator.

In a recent test of a 50-∿ 250-k.w. rotary converter, in the presence of a well-known engineer who wished to see an overload test, the machine, when running at three-quarter load, was subjected to an additional load equal to 250 per cent. overload, thrown suddenly on

with a knife switch, without causing serious sparking. It is hard to say what grounds there can be for the contention that the 50-∿ rotary converter is a delicate and unstable piece of apparatus. The great overload capacity of the rotary converter is due not only to its good commutating qualities, but to the fact that the copper losses in the armature are small on overloads.

Risk of Breakdown.—The consideration which is most important to a station engineer is freedom from breakdown. For smallness of risk of breakdown, the rotary converter and its transformers are superior to any other converting machinery, because in the first place there is no high voltage on the running machine, and in the second place there is only one running machine to break down, instead of two. A well-insulated transformer is very much less likely to break down than any kind of high-voltage armature. Even if one of the transformers does break down, the load can be carried on the other two.

Power Factor.—As is well known, the power factor of a load of rotary converters can be adjusted to any desired figure by varying the excitation. In ordinary traction work, where the rotaries are compounded, the power factor varies with the load, and it is usually arranged that on full load there is a leading power factor on the high tension side between 99 per cent. and 96 per cent. This gives a slight leading component, which of course is desirable on full load. The synchronous motor-generator set is the same as the rotary converter in this respect, that the power factor is completely under control in all circumstances. The asynchronous motor-generator set has a lagging power factor less than unity.

Efficiency.—Here the rotary converter has a substantial advantage over any other type of converting apparatus. In comparing the efficiencies of different machines, it is very important to consider the basis of their ratings. When a sub-station is supplying a town load, it is always necessary to have enough machines running to avoid catastrophe in the case of sudden unexpected demands. The efficiency of the sub-station will greatly depend upon how many machines are running, and how near to full load they are working. If the station is provided with rotary converters, it is in most cases perfectly safe to have all the rotary converters fully loaded and allow them to be overloaded before a new machine is thrown in. This is because the rotary converters can take 50 per cent. overload for an hour or two in case the load should be unexpectedly increased. Moreover, they can take 100 per cent. overload without any commutation troubles for a time sufficient to synchronise and throw in additional machines. When dealing with motor converters or motor-generators, which are not able to take such excessive overloads without danger, the station engineer is not justified in normally running his machines up to such a high point. He must, except for the very steadiest loads, always have some capacity in hand to meet sudden demands and give him time to throw in other machines. In making a fair comparison of the year-round efficiency of different types of converters, it would not

be unfair to take the efficiency of the rotary at full load and the other types of apparatus at three-quarter load. In the table, however, efficiencies are given without regard to this additional advantage on the part of the rotary. The machines are compared designed with the same liberality and same factor of safety in insulation and mechanical strength.

Attention.—In the amount of attention required to keep a machine clean and in good running condition, the rotary converter is just twice as good as its competitors. There is only one machine to keep clean instead of two, and, moreover, the easy commutation enables the carbon brushes to run for years with only the slightest wear.

Floor Space.—In the matter of floor space, the rotary is again at the head, provided the transformers can be placed in a lower or upper storey, as is often the case. The floor space of the rotary is 77 sq. ft.; of the motor-generator 164 sq. ft.; and of the motor converter 117 sq. ft. In addition to this, the starting motor of the rotary (if required) takes up a few square feet, as does also the starting box of the motor converter. A comparison of the floor space of 500-k.w. sizes is still more in favour of the rotary.

Discussion.

The Chairman.

The CHAIRMAN (Mr. T. L. MILLER): Before opening the discussion, I should like to make one or two remarks on the paper. The author has presented the case between the converter and the motor generator very fairly. He states that such rotaries as he has met with which have given trouble are of antiquated design, but no doubt we shall hear a good deal of difference of opinion with regard to the operation of rotaries and motor generators. In the past there has undoubtedly been very considerable difficulty with running rotaries at 50 or 60 ∾. Of course the most important point for the central station engineer is continuous running. The matter of efficiency is a very trifling one compared with a shutdown, which would be likely to cause a greater financial loss than any slight gain in efficiency.

The author has drawn attention to the fact that the voltage varies on the D.C. side with that on the A.C. side with the rotary, and has pointed out how that could be got over, but all such additions cost money, and if there is an advantage in price, then every additional appliance put on means a reduction of that advantage, and I should like to have from Mr. Walker some figures with regard to the cost of the motor generator with the extra appliances. I think the majority of us are concerned with what we can buy the machinery at, and no doubt during the evening we shall have some figures with regard to the relative cost.

There is another point in regard to the operation of rotaries which I should like to refer to. In a paper read before the American I.E.E., reference is made to trouble experienced when rotaries are run in parallel from one bank of transformers. It appeared from the

discussion that that arrangement had in a number of cases given rise to a very great deal of trouble, but that these troubles were obviated by putting with each rotary its own bank of transformers. I should be glad to know if the author has experienced that trouble. The Chairman.

Mr. S. L. PEARCE: What few remarks I have to make I make from the point of view of the central station engineer, and more particularly from that of one who has had at some time or other most of the types of plant mentioned in the paper under his control. I do not propose to go into the technical points in connection with the design of motor generators and of rotary converters. There are, of course, a very large number of interesting points in connection with the design of rotaries, but more particularly of the 50-∿ rotaries, which of necessity are bound to have a very large number of poles and a large number of commutator bars, with comparatively small centres between the brush collectors. All those are points which I hope will be brought out later on, as bearing on the question whether the 50-∿ rotary is really a good piece of electrical apparatus or not. The author divides the use to which rotaries may be put under practically three headings. He instances first of all traction plants. He then goes on to mention in the second place the use of rotaries for electrolytic work, and instances the Carville sub-station of the Castner Kellner Company. I was in that particular station a few weeks ago, and was somewhat surprised to see alongside the rotary plant, three motor-generator sets, of 600 k.w. capacity. It would be of interest to know why it was found necessary to put motor generators alongside the rotaries in the Castner Kellner station. The third and most important point is the question of using rotaries at 50 ∿ for combined lighting and tramway work, and one cannot fail to be struck with the very slow advance made in the past few years with the installation of rotaries on 50-∿ circuits. It may be said that this is due to prejudice on the part of station engineers. I prefer to think it is not due altogether to prejudice, but to certain well-defined and clear opinions that English engineers have with regard to the comparative merits of the operation of rotaries and motor converters on high frequency circuits. I have only this evening returned from a most interesting visit to the Continent. During a course of inspection of some of the large plants operating there, I have come in contact with a good many engineers, and am much impressed with the fact that there is on the Continent, as in England, a very deep-rooted objection to the use of rotaries on 50-∿ circuits. With regard to the author's statement that opinion was beginning to waver in the face of facts, and that there were a large number of 50-∿ rotaries being put down in the country, it will be interesting if he can give us a list of this plant, and also the size of the units. I think the 1,500-k.w. sets referred to are at Brighton, and there the voltage on the D.C. side is low, being only 250, which makes a considerable difference in the design of the machine. I have been informed, I do not know Mr. Pearce.

Mr. Pearce. whether correctly or not, that at Brighton, where only one turbine is run at Southwark power station, and one rotary is run in the town, that at times of light-load oscillations are set up on the line, and they are obliged to put in a second turbine to stop that oscillation. The author brings out very clearly the good points of the rotary converter and the motor converter as against the remaining types enumerated, and the most unbiassed mind cannot fail to be struck with the fact that the first two machines are bound to be the machines to be utilised in the future.

Taking the first point in the paper—the question of starting—there is no question that for rotaries operating at 50 $\sim$ there are really only two practical methods for starting, viz., by means of a small induction motor at the end of a shaft, or starting from the D.C. side, and the former is undoubtedly preferable. At 25 $\sim$ it is possible to start up on the A.C. side, and it is done invariably in the case of the first machine on the Central London Railway system, but it is not easy, because there is often a difficulty in getting the field to build up to the right polarity, and a large amount of time is often wasted in so doing. With regard to starting up motor converters, they, of course, can be started up from the D.C. or the A.C. side, but the latter is preferable. The author has got a little bit adrift as to the method of starting up motor converters; it is only a small detail, but I will say this, that of all the types of sub-station machinery that I have had under my control, I consider there is nothing so good as the motor converter as regards the ease with which it can be got into synchronism. The author deals with the question of the tendency for the rotary converter to reverse its polarity. I do not know how far that is prevalent in up-to-date machines, but it certainly is a serious nuisance on many occasions on the Central London Railway system.

Now we come to the question of parallel running, and I am bound to state that the 250-k.w. set referred to at the Polygon sub-station has run satisfactorily in synchronism with Stuart Street and in parallel with existing sub-station motor-generator sets. The conditions, however, are very good. The variation in the frequency of the Stuart Street supply is under 1 per cent.; under the worst conditions the maximum variation was $1\frac{1}{2}$ per cent., and it frequently came down to half of 1 per cent. I do not think that is bad at all. As regards the voltage, the average is 1 per cent. either way from the mean. In all large central stations some engines may govern better than others, but the facts are as stated. In the case of the many converter sets installed on the Manchester system we have never experienced the slightest trouble as regards parallel running.

The author states that it is not true that the motor converter has a greater synchronising power than the rotary. I do not know whether that is so, but it appears to me that, as all motor converters of the latest type are wound for 12 phases—and I am not sure whether that is a standard practice—the synchronising power of the motor converter must obviously be greater than that of the rotary. The author's

Mr. Pearce.

statement is not correct with regard to the parallel running on the D.C. side, that all types stand on the same footing. Take the case of the rotary converter, every fluctuation on the A.C. side has a corresponding fluctuation on the D.C. side, but in the case of the motor converter half the D.C. energy is generated energy, and depends, of course, on the periodicity.

With regard to the question of the variation of voltage, I agree with the author that, in cases where the A.C. supply is exceedingly variable, it is preferable even under those conditions to employ converter plant, and to use a voltage regulator so as to keep the D.C. voltage constant.

The next point touched upon is the question of hand regulation of voltage, with which also is more or less bound up the question of variation in power factor. The author mentions four methods by which hand regulation can be accomplished, and the first is by means of putting a self-induction into the transformers. That does not appear to me to be a method that has a great deal to commend it. I think there will be some little difficulty in calculating the exact amount of self-induction that is required in the first place, and it seems to me that the preferable arrangement would be to add self-induction by means of a separate coil or separate chokers, which could be altered, if found necessary, later. On looking at the curve in Fig. 2 it will be seen that even with the method advocated by the author of putting self-induction into the transformers, the power factor varies from 8 per cent. to 9 per cent. for a 5 per cent. up and down, or a total of 10 per cent. voltage regulation. That appears to be too wide for practical working conditions. The second method advocated for hand regulation is by means of an A.C. booster, and that undoubtedly is a very pretty and beautiful arrangement, but it adds very considerably to the cost, a point which was brought out by the chairman in his remarks, and, in comparing cost between machine and machine, one must know what the accessories are going to cost. It is only right to state, of course, that with the A.C. booster one can get perfect control over the power factor. The third method mentioned was by means of the induction regulator, and that is, I think, open to the objection stated in the paper. The fourth method is rarely used, and very little information is forthcoming upon it. I have here some results of tests carried out at the Dickinson Street sub-station on 500-k.w. motor-converter plant operated at full load. At 420 volts on the D.C. side the power factor was unity; rising up to 450 we got 98 per cent., leaving, that is, practically 7 to 7½ per cent. alteration in the D.C. voltage with only 2 per cent. variation in power factor. That is exceedingly striking, and brings out the main points of difference on the question of hand regulation between the rotary converter and the motor converter. The author, I think, minimises the question of power factor, but it must be remembered that, taking the Manchester sub-stations, they are running practically all day at 420 volts, and it is only for a very short period of the day, during the peak of the load, that the voltage is raised to 450 volts; so that for the bulk of the twenty-four hours they are working on a poor

Mr. Pearce, power factor, which becomes unity or slightly leading for a very short period only of the day. Therefore it seems to me that the method of putting self-induction into the transformer is really not good enough for practical work.

With regard to the question of commutation, the test to which the author refers on the 50-∿ 250-k.w. rotary converter is extraordinarily good, and the machine proved to be most stable, the results being excellent, but I will go so far as to say that the motor converter, from the records available, is very little inferior to the rotary converter, even on that point. I have been informed quite recently by the resident engineer on the Great Western Railway that six 600-k.w. motor converter sets installed there had stood dead shorts on the line, and never showed the slightest tendency to flash over, and as regards overload, I can only say that those machines at the Great Western Railway have carried, I think, 1,000 k.w. for one hour with perfect commutation without undue heating. On the Manchester sets I have run the 500-k.w. motor converters arranged with commutating poles, with 800 k.w. for a solid hour, and the 250-k.w. sets with a load of 450 k.w., for the same period, without sparking and undue heating, and I think the results are quite good enough for practical working.

Regarding the risk of breakdown, it is a very novel argument to be advanced, and it would have been just as well if it had been omitted from the paper, as it did not bring out any very good points in favour of the rotary converter. Moreover, it seems to me that the paragraph is quite misleading, because the author states that there is no high voltage on the running machine. Where is the high voltage on any portion of the running machine? Is it not just as safe to have the high voltage on the stator as on the transformer? What is the voltage we get on the rotors of the converters? I have known transformers give quite as much, if not more trouble than the stators of any motor converters.

We now come to the question of efficiencies, and there the author is quite right in his statement that the rotary converter does show a superiority to any other type of converting apparatus. I do not think it is as much, however, as he makes out. We recently invited tenders in Manchester for 1,500 k.w. converting plant, and on the maker's own showing there was only a difference at full load of 1½ per cent. I do not doubt the figures put forward for rotaries are correct, but I am quite certain that the figures put forward for the motor converters are good, because I have proved up to the hilt that the figures obtained on smaller sizes have been surpassed. I am of opinion that for all day in and out running there is not much more than 1½ per cent. margin in favour of the rotary converter.

With regard to cost, that question is a little bit outside the scope of the paper, but I can only say that the logical deductions drawn as to the cost of rotary plants are not borne out in practice. A good many conditions govern the market price, but as far as the buyer is concerned, my experience is not exactly what has been forecasted.

With regard to the general conclusions on the subject, there is no doubt about it that if one is going in for rotary converter plant, the plant must be carefully designed, having in view the conditions under which it is going to operate, and most accurately adjusted. It is no good buying rotaries promiscuously, and putting them down on any system. The author has referred to rotaries running in the Polygon sub-station; no more ideal site could be found. The sub-station is situated in the centre of the Manchester system, is heavily loaded, and the rotary is set to run on a steady lighting load. I do not say all rotary converters are stable, or all rotary converters unstable, but I do say that in adopting rotaries at 50 ∿ we shall be cutting down to a pretty fine point the line between a stable and an unstable machine, and looked at from that point of view, it seems to me that we are running a risk of decreasing their factor of safety. Mr. Pearce.

Mr. LA COUR: I have been much interested in this paper, which deals with such an important subject. In a number of respects, however, my views differ from Mr. Miles Walker's, and I should particularly like to define what I claim to be the correct status of the motor converter. As the 50-∿ motor converter generally consists of an induction motor coupled in cascade with a 25-∿ 12-phase rotary converter, it is evident that it closely resembles an ordinary rotary converter worked in series with a stationary transformer. From this the advantages will readily be understood which a 50-∿ motor converter possesses over a 25-∿ rotary converter. The two types differ principally in three respects: first, as regards the revolving mass; secondly, as regards the reactance; and thirdly, that the motor converter is working half as a motor generator and half as a rotary converter. The author claims that the small mass and low reactance of the rotary converter is advantageous. I think the other way about. It has long been known that the 50-∿ synchronous motor generator which has large revolving masses and a fair amount of reactance is a much better machine than the rotary for stability; from this it is obvious that the 50-∿ motor converter must have an advantage over the 25-∿ rotary. Let us, for example, take the extreme, and consider the behaviour of a 25-∿ rotary with negligible mass and reactance—that is, with an immense synchronising force. Such a machine would follow the engines in all their irregularities in speed and the lights would flicker. In my opinion, therefore, the motor converter stands at good advantage in this respect, since it possesses a fair amount of flywheel effect and reactance; the first tends to steady the system, and the second prevents the motor converter taking up a large synchronising current which consists of higher harmonics when running light. The revolving field of the induction motor also has the same effect, so that the motor converter does not take any synchronising current at all. The ratio between the flywheel effect and synchronising force is naturally made so that no mechanical resonance can take place between the converter and any of the prime movers on the system and so disturb stability. From experience extending over many years it has been Mr. La Cour.

Mr. La Cour.

proved that machines with large synchronising forces and small flywheel effect are more liable to cause hunting than machines with a fair amount of reactance and flywheel effect.

The reactance voltage of a transformer designed on normal lines is about 3 to 4 per cent. of the working pressure, while the reactance of an ordinary induction motor varies between 15 per cent. and 20 per cent. It is, of course, possible to design a transformer with more reactance and an induction motor with less; but as the reactance of a transformer and machine of given proportions increases with the number of turns, an increase in reactance means more copper and less iron, and as it is well known that iron is cheaper than copper, it is quite clear that an increase in the reactance over a certain limit (which is fairly low in the case of a transformer) will increase the cost of the apparatus.

The commutation, of a 50-∼ motor converter, is theoretically considered, inferior to the 25-∼ rotary, but by means of commutating poles it is possible to obtain sparkless running with well over 100 per cent. overload, which meets most requirements. As the overload capacity of rotaries is generally limited by hunting and not by sparking, as the author appears to think, it is easily seen that the 50-∼ motor converter from the operation point of view is superior to the 25-∼ rotary and far superior to the 50-∼.

As regards efficiency, the rotary naturally has a slight advantage, which amounts to 1½ per cent. for a 1,000-k.w. set at full load. The 50- and 60-∼ rotaries have many poles, high peripheral speeds and small distances between the brush spindles, so that these machines are more liable to hunt and flash over than the motor converters and 25-∼ rotaries.

As regards the contention that the motor converter is a far more satisfactory machine on the higher frequency circuits than the rotary, we have but to refer to plants in operation in Great Britain and the United States. The Seattle Electric Company, which take a part of their power from the Snouqualmic Falls Power Company, claim to have the largest installation of rotaries working on 60-∼ circuits in the United States. During the years 1900–1904 much trouble was experienced, they frequently flashed over at the brushes, and hunting and flickering in the lights was quite common. Although the trouble could not entirely be attributed to the rotaries, they were undoubtedly responsible to a great extent, and in spite of the experience gained during the time they have been working, their performance still leaves much to be desired. The commutators still require the use of sandpaper at least once a week, and many attempts have been made to obtain a suitable brush. With the motor converter such troubles are practically unknown.

The machines of the motor-converter type recently supplied to the Great Western Railway are of the latest design, being provided with commutating poles and they can withstand 100 per cent. overload without signs of sparking. On one occasion an iron bar was acci-

dentally dropped across the connections of one of the machines leading to the busbars, causing a dead short; the machine vibrated violently but did not flash over. Mr. La Cour.

The statement which the author makes with regard to the Westinghouse Company supplying rotaries which will stand 50 per cent. overload continuously without heating, appears to indicate that there are certain features in the design of rotaries over which they have no control. It is obvious that a machine cannot do this without an abnormal amount of material, which the customer pays for. The question of initial outlay, however, the author wisely considers outside the sphere of the paper.

The remaining points which are raised against the motor converter I think quite unnecessary to consider, as the various statements which have from time to time appeared in favour of these machines have been amply borne out in practice.

In conclusion, I feel convinced that the motor-converter plant has distinct advantages over the other forms, especially on the higher frequency circuits, and that a 50-∿ motor converter can meet all ordinary requirements and at the same time prove a much better all-round article than any rotary or other machine on the market.

Mr. J. H. Bowden: In calling for tenders for motor generators at Poplar we asked the contractors to put forward alternative suggestions, and orders were placed with Messrs. Bruce Peebles for motor converters on account of the low cost and the great simplicity of their proposal. With regard to reversal of polarity, a small exciter is suggested in the paper; this is a complication. There is no tendency in a motor converter to reverse. I do not agree with the author that a rotary is easier to synchronise than a motor converter. The needle seems to stop longer in the position denoting synchronism starting from the D.C. side, and other complications are not advisable if avoidable. The induction regulator, for instance, requires an air-blast and other accessories. The author speaks of 50 per cent. overload for an hour or two. If this is the case and the efficiency does not drop with the overload, why do they not rate these machines higher? With regard to the question of attention, the Poplar motor converters are only shut down for three hours per week, and one can hardly demand less attention than that. Mr. Bowden.

Mr. J. S. Peck: I have been told frequently that there exists a strong prejudice in Great Britain against the rotary converter, but I have never fully appreciated it before. It has been stated by a preceding speaker that transformers cannot be made with very high reactance. This is not correct, as transformers can be built for any reactance desired. Ordinarily very considerable trouble is taken in order to keep the reactance low. If a transformer is desired with a high reactance it will, in general, be cheaper than one with a low reactance. The objection to high reactance in transformers or other apparatus is that it impairs the regulation. Transformers have been built with so high a reactance that they will stand a short circuit without taking Mr. Peck.

Mr. Peck. more than full load current. A motor converter may be short-circuited without damage provided there is sufficient reactance in series with it to keep down the current to a low value. The same can be said of any other type of alternating-current machinery.

A question has been asked regarding the possibility of operating rotaries in parallel from the same transformers. In general it is not advisable to operate rotaries in parallel from the same A.C. busbars. If, however, the transformers are provided with separate secondaries, one for each rotary, as many rotaries as desired may be operated from the same bank of transformers.

I think the whole question amounts to this: Can the 50-∿ rotary converter be made to operate with perfect satisfaction? The best answer to this question is that there are thousands of kilowatts of 50-, 60-, and even 66-∿ rotaries in highly successful operation in Canada, in Mexico, and in the United States, and during the past year a large number of 50-∿ rotaries have been installed in Great Britain. Troubles have, however, been experienced with rotary converters for both low and high frequency work. Some trouble was experienced with the rotaries on the Underground Railway in London, due to the fact that they flashed over when violent short circuits occurred on the D.C. side. It was found, however, that the rotaries would stand enormous overload without flashing, and that they flashed only when a short circuit occurred just outside the sub-station—a condition under which any well-designed machine would fail.

Recently a case has been brought to my attention where a rotary flashed over. On investigating the conditions I found that this occurred only when 150 per cent. to 175 per cent. overload was suddenly thrown off the machine. I asked what a D.C. generator would do under similar conditions. The reply was that a D.C. generator would probably flash over before they could get any such overload upon it, but that a rotary was expected to give very much heavier overloads than a D.C. generator.

A few days ago I visited a station where several motor converters were installed. Two of the machines were operating at half load without sparking. When the load on one machine was reduced from ½ to ¼ load, the brushes sparked violently and had to be shifted. The remainder of the load was thrown off, the brushes sparked again, and had to be readjusted. Thus, in going from half load to no load, two adjustments of the brushes were required, yet these machines had been pronounced eminently satisfactory. I think that if a rotary converter had been installed in which two adjustments of the brushes were required in going from half load to full load, it would deserve to be called a miserable failure, yet the motor converter is said to be entirely satisfactory. The motor converter was invented in 1899 by Maurice Leblanc. About two years later it was invented independently by B. G. Lamme.

Mr. Cramp. Mr. W. Cramp: Although I should like to give help to the side of the author and the rotary converter, I must confess myself unable to

do much in that direction. He admits that the real machine that central station engineers want is one which will transform high tension alternating current to low tension or 600-volt direct current, and allow of voltage regulation; and while starting from that point of view, which is perfectly fair, he proceeds to compare the simple rotary converter with the motor converter, the motor generator being admittedly out of the question at the present moment. He mentions the need of transformers under certain circumstances where the advantages of transformers come in, but when it comes to the question of floor space, and to one or two other items in the table given at the beginning of the paper, as, for instance, regulation, power factor, attention, cost, and so on, there the necessity for transformers apparently drops out. There is one question that has not been touched upon as yet, and that is the extra complication of the switchgear. It seems to me that if we compare the switchgear required for the motor converter as against that required for the rotary converter, there alone is a very great advantage on the side of the motor converter. The switchgear for the latter is much more simple, and as far as the risk of breakdown is concerned the risk is certainly greater in my estimation in the rotary-converter system with its switchgear than it would be in the motor-converter system, and certainly the connections are more complicated in the former case. But apart from those considerations, a case which has been forgotten is the central station in which voltage regulation is not of such very great importance, and particularly a low frequency central station. There it seems to me the rotary converter might well hold its own against the motor converter. One might say, indeed, that the true comparison really existed, not as the author had drawn it, between the rotary converter and the motor converter, but between the rotary converter and its accessories, and the motor converter and its accessories; so that where there is a traction system alone and voltage variation does not very much matter, there the rotary converter will be adopted; while for anything in the way of a lighting system, or combined lighting and traction system, the motor converter has the field, and will keep it. Mr. Cramp.

Mr. H. M. SOUTHGATE: There seems to be an idea that the author has touched lightly on question of cost because it is unfavourable to the rotary. Prices to-day are determined very largely upon competition rather than upon the actual cost of manufacture. Price in most cases is the determining factor in award of contracts. I do not think there is any doubt but that the same firm making both rotary converters and motor converters will find the actual cost to be lower for the rotary complete with stators, oil, starting motor, and even A.C. booster if necessary. Practically the same switchgear would be required in each case, as there is no need of apparatus in the static transformer secondaries with the exception possibly of isolating switches, which would cost but little. Mr. Southgate.

There are two schools in the United States regarding high periodicity rotaries. The lack of success of the General Electric

Mr. Southgate. Company with 60-∿ rotaries was such that they at an early date took the stand for motor generators for higher periodicity work. The Westinghouse was so satisfied with the high periodicity machines that in determining the periodicity for a 30-mile inter-urban system 60 ∿ was put in rather than wait a few weeks for 25-∿ apparatus. That was ten years ago.

Mr. Schoepf. Mr. T. H. SCHOEPF: In 1889 and 1900 I was responsible for four tramway sub-stations equipped with rotary converters operating on the supply system of 60 ∿, and the operation, in every particular, of these machines gave entire satisfaction for the two years I was with the tramway. One of the sub-stations was equipped with six 300-k.w. rotary converters with starting motor, the switchboard comprising six A.C. panels, six D.C. panels, and five feeder panels. One day, I undertook to start all six machines and switch on the five feeders within ten minutes from the time of entering the building. This was actually accomplished within 9½ minutes, and I do not think equally good results could be obtained with motor converters. I know of only one instance in two years in which a rotary had "flashed over" or "bucked," and that one was due to an inexperienced attendant leaving the brushes out of the neutral position after cleaning the machines.

Upon an inter-urban railway, the Union Traction Company of Indiana, a test was made to determine the length of time a rotary converter could be operated continuously without shutting down. After three months' satisfactory operation the machine was shut down to renew the carbons on the D.C. side.

Concerning the admitted difference of efficiency in favour of the rotary converter as compared with the motor converter, suppose we assume a 1,000-k.w. set with average all-day load of 600 k.w. and a difference of efficiency of 1 per cent., which, with power at ¼d. per unit, results in an annual saving of £109. This capitalised at 5 per cent. is £2,000, which may be expended in rotary converters to place them on the same basis with motor converters as regards first cost.

Mr. Whysall. Mr. F. H. WHYSALL: On the question of starting up, we have installed at Dickinson Street station four motor converters, and I find that the average time taken to start up is two minutes for everything for each motor converter. We can do it in less; the entire operation has been done in one minute twenty seconds. I think that is very good, and better than Mr. Schoepf's record. Personally, I should be very glad indeed to have some experience of the author's booster regulated sets. I really believe that they will work satisfactorily, and in comparison with other types that have been mentioned, they would stand a very good chance. I think the question of first outlay is a most important one in connection with this matter. The booster regulators and other accessories seem to cost money, and that is a point that influences engineers in purchasing converting machinery.

Mr. Field. Mr. M. B. FIELD (*communicated*): There are few men better qualified than the author of this paper to discuss the vital points

of the design of rotary converters and motor generators, and it was with a feeling of much regret that I found all "State secrets" zealously guarded and excluded. The author merely deals with the pros and cons of the subject from the user's point of view, with, I presume the twofold object of eliciting a beneficial discussion and dispelling the strong prejudice which he alleges exists against rotary converters. I quite agree with the author as to the existence of this prejudice, but I think the performance of the early rotaries, as supplied, is solely to blame therefor. The author appears to me to characterise all such rotaries of doubtful behaviour as antiquated, and if this contention be upheld, station engineers must bear in mind that rotaries have in the past become antiquated while yet in the prime of life, and a due allowance should be made in the assessment of their plant-value for such antiquation. The discussion of the relative merits of these two classes of converters will never, I suppose, cease, there will always be advocates for each system, and to my mind it amounts to six of one and half a dozen of the other. Mr. Field.

It appears to me that the only case the author has made out for the rotary is higher efficiency and greater momentary overload capacity. My experience of rotaries, supplied not many years ago, has been that the high efficiencies claimed by the makers are not realised in practice—but perhaps the machines are now antiquated. As regards overload capacity, it must be remembered that with the advance in the design of rotaries, corresponding advances have been made in other commutating machines, so much so that a good D.C. generator running at high speed and provided with inter-poles will, I am told, behave satisfactorily with momentary overloads up to twice full load. Similarly, the motor portion may be very considerably overloaded for short periods, so that although the rotary will certainly have some advantage, I do not think it is necessarily a great one.

With regard to floor space, I think it is not fair to reckon on putting transformers in a basement or top storey. If the transformers are oil cooled they should be grouped in brick cells with means for ventilation, which can be cut off in the case one takes fire. If air-blast transformers are used, the usual paraphernalia of air-flues, blower, dampers, etc., are again necessary. All this should be taken into account when considering cost and floor space. When we remember that the adjuncts of the rotary are such details as transformers with their low-tension cables, and sometimes low-tension switchgear, starting motor, A.C. booster or induction regulator, exciter, and so on, it does not appear that the adoption of rotaries conduces to simplicity.

One objection to rotaries that I have frequently met with is the tendency to reverse their polarity if the voltage of the generating system is momentarily lowered. I have known fairly frequent cases where, say, half of the total number of rotaries working on the same system have had their polarity reversed by some unusual disturbance at the generator end. This, of course, may be very disconcerting, to put

Mr. Field. it mildly, and I would like to ask the author if this again is confined to antiquated machines.

I trust I may be excused if I refer to another point to which I have already called attention in my recent paper on "Idle Currents." I refer to the special losses in the armature conductors, if solid, owing to the non-uniform current distribution. This must, I think, occur in rotary converters to a more pronounced extent than in any other type of machine, owing to the peculiar shape of the resultant current flowing in the armature bars, *i.e.*, the current which results from the A.C. input and the D.C. output. This is a very irregular current, consisting of corners and odd bits where the A.C. and D.C. currents do not quite fit into one another, with the result that the effective current consists of a number of high frequency terms which are particularly active in increasing the armature losses.

In the case of large systems, the variations of frequency in the generating station are not so likely to occur as variations in the A.C. voltage. With motor generators, the voltage on the D.C. side will be unaffected by variations of voltage on the A.C. side; whereas in the case of the rotary converter the reverse is the case. This point has certainly been mentioned by the author, but I do not think sufficient emphasis has been laid upon it, and in large systems supplying railways and other work liable to very heavy overloads and supplying a considerable amount of lighting at the same time, the matter is likely to be an important one.

In designing any commutating machine, it is always a matter of great inconvenience to be tied down to some particular number of poles. This is the case in the rotary converter where given speed limits cannot be exceeded. On the other hand, where floor space is very much restricted, motor generators may be built of the turbine type; for instance, an 850-k.w. set, 50 $\sim$, may run at 1,500 revolutions per minute, and a 1,500-k.w. set at 1,000 revolutions per minute. Such flexibility is, of course, quite out of the question when dealing with rotary converters.

Mr. Walker. Mr. M. WALKER (*in reply*): The opponents of the rotary converter have harped upon the complications of the booster, induction regulator, and the various other contrivances which *might* be added to a rotary. One would think after hearing the discussion that these things were all necessary, and constituted the main drawback. As a matter of fact, they are not necessary at all. The main method of obtaining the voltage adjustment advocated in the paper requires none of these things. We want only transformers with a high inductive drop—that is, cheap, well-insulated transformers and a rotary converter. This arrangement is giving complete satisfaction in hundreds of cases. The power factor is sufficiently near unity under all practical conditions. It is only when the operator wishes to have complete control over his power factor or wishes for an exceptionally great range of adjustment, that we advocate the use of the booster. The same thing would be necessary under the same conditions with a motor converter. Mr. Pearce told

us of a case in which the power factor was kept within 2 per cent. Mr. Walker. This can be done with a rotary converter by simply increasing the reactance in the transformer. The operation of the rotary and transformers is just the same in this respect as the operation of the motor converter. Both obtain voltage variation by drawing a leading current through a machine containing reactance; the higher the reactance the less the current leads when the voltage is raised. The curve in Fig. 2, which I consider a good one for all practical purposes, is obtained with only 18 per cent. reactance. If we care to increase the reactance, as is done in motor converters, and thereby decrease the synchronising power, we can obtain just as good a curve on a rotary as on any motor converter. The advantage of a rotary is that we can make the reactive drop as little as we like. This can only be done on the motor converter by enormously increasing its cost.

Another objection which has been raised against 50-∿ rotaries is the great number of brush arms. This is not an objection at all unless it can be shown that the distance between the brush arms is too short. There is no difficulty in making the distance 9½ ins., which is quite sufficient for all practical purposes. The question is: Will a station engineer prefer a machine with only 9½ ins. between the brushes—which works perfectly satisfactorily in every way and saves 2 per cent. of power, amounting to £350 per year on a 1,500-k.w. machine—or will he prefer a machine with 12 ins. between the brushes—which does not operate any better and wastes £350 per year? Is the additional 2½ ins. worth the expense? The number of brush arms on a rotary is, from one point of view, a good feature. It enables the current per brush arm to be kept within small limits. Any one having experience with commutating machines knows that it is not a good plan to have too great a current per brush arm. A 1,500-k.w. 500-volt motor converter with 8 poles, when working on 50 per cent. overload, has 1,125 amperes per brush arm. A 1,500-k.w. 500-volt rotary working under the same conditions has only 450 amperes per brush arm. It is well known that under practical working conditions the current is not always equally divided between the brush arms, and some of the brushes get into bad condition. Experience has shown that the only way to get good results is to keep the current per brush arm and the current density in the brushes fairly low, so that if a brush arm is called on to yield double its normal current, it can do so without undue heating. It is possible by using a special grade of brush to work at current densities as high as 100 amperes per square inch, but if I were a central station engineer with any commutating machines under my charge, I should prefer to work with 35 amperes per square inch normally.

Some speakers have thought that a motor converter has an advantage over a rotary in steadiness of D.C. voltage with varying A.C. voltage, owing to the fact that it is half a D.C. generator. Mr. La Cour, however, did not raise this point: he knows more about it.

Mr. Pearce considers that there is no more risk in having a high voltage on the armature of an induction motor than having high voltage

Mr. Walker. on the transformers. I should like to refer him to Mr. Bowden, of Poplar, who has had some rather unpleasant experiences with high voltage on motor converters. It is impossible to obtain the same factor of safety of insulation at a reasonable cost on an armature as on a transformer. Moreover, it should be remembered that if the armature burns out, the whole machine is shut down for a long time while it is being rewound, whereas with a transformer it is simply a matter of substituting a spare.

Mr. La Cour's argument in favour of a large mass in the rotating part will hardly bear investigation. He admits that a rotary with a small mass can more easily follow the fluctuations in frequency, and in that respect it is less likely to go out of synchronism. His contention is that the high moment of inertia in the rotating part of a motor converter prevents the D.C. voltage being affected by fluctuations in the frequency of the A.C. supply. Now it is clear that as the machine runs synchronously it is not possible to have a displacement of more than a fraction of a pole pitch from the true synchronous position. In one or two seconds after any change has occurred in the frequency, the speed of the machine must correspond with the new frequency, or the machine will have lost more than a pole pitch.

One hears many stories about commutating machines standing short circuits. My experience is that no good 500-volt commutating machine will stand a real short circuit. If a machine is designed to stand up to its voltage on heavy loads, the current rises to thousands of amperes at the moment of short circuit. This causes an explosion under the brushes which develops into an arc between the brush holders. If this arc does not immediately clear itself the circuit breakers come out and the machine shuts down. All commutating machines are the same in this respect. Of course it is possible by putting in a very high reactance on the A.C. side to limit the voltage on heavy loads so that this danger from flashing over is reduced. This can be done with the rotary converter as easily as with any other class of apparatus. It is only a question of policy, as to whether a machine is to be designed for the best running conditions, or designed to take short circuits. The instance cited by Mr. La Cour of the motor converter running from D.C. to A.C. and turning round the steam turbines is in no way remarkable, the same thing would have happened if rotary converters had been running instead of motor converters.

The question has been asked as to the efficiency on overload. I may say that all the 50-$\sim$ rotary converters with which I have had to deal had a steadily increasing efficiency up to 75 per cent. overload. It was asked further why these rotaries are not rated up. The reason is that the policy of the British Westinghouse Co. has always been to give a rotary converter of very ample design, so that notwithstanding arduous conditions of service, it will stand up and give satisfaction. One of their standard 300-k.w. rotary converters will yield 450 k.w. continuously with not more than 40° C. rise with an efficiency of 96 per cent. The only reason why this machine is not sold for a 450-k.w.

rotary is that the British Westinghouse Company prefer to give the user a bigger machine with more poles and less current per brush arm, because they know that in the long run he will be better satisfied with it when he puts on overloads beyond the 450 k.w. Mr. Walker.

It has been said that the switchgear for a rotary converter is more complicated and more expensive than for a motor-converter set. This is not so. All that is required for a rotary is high-tension switchgear and direct-current switchgear of exactly the same type as used for motor generators. It is true that in some cases users prefer to have switches between the transformers and the rotary. This is a matter of taste. Where it is required it is an additional advantage in favour of the rotary, because it would be impossible in a motor generator. With regard to the floor space, the figures given in the table are based on the floor space occupied by a 500-k.w. motor converter and a 500-k.w. rotary.

Mr. Field points out that it is always a matter of great inconvenience to be tied down to some particular number of poles. The rotary converter gives us a wider choice in the number of poles that may be employed than any other type of direct-current machine. For instance, a 1,500-k.w. 50-∾ rotary may economically have any even number of poles from 4 to 30. If the voltage is very low, a large number of poles can be chosen so as to keep down the amperes per brush arm; if, on the other hand, the voltage is high, a smaller number of poles is advantageous. I have before me a drawing of a 1,500-k.w. 50-∾ rotary converter with 4 poles; the floor space of this machine is less than half that of a motor converter, and the calculated efficiency is about 98 per cent.

To sum up, it has been admitted even by the greatest supporters of motor converters that the rotary is a pretty good machine, and I think that the evidence goes further than this, and we can say that the 50-∾ rotary converter is a very stable machine of great overload capacity, with small risk of breakdown; that it can do everything that a motor converter can do under the same conditions, and it is admittedly at least 1½ per cent. more efficient.

On the motion of the CHAIRMAN a unanimous vote of thanks was accorded to the author for his interesting paper.

NOTICE.

The following publications can be obtained from th Secretary, or from Messrs. E. and F. N. Spon, Ltd., 57 Haymarket, S.W.:—

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Index to Vols. 1–10 of Journal (1872–1881) ...	2	6
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Report of the Electrical Legislation Committee	2	6

[redacted]

JOURNAL

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Proceedings of the Four Hundred and Fifty-Second Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, February 21, 1907—Dr. R. T. GLAZEBROOK, F.R.S., President, in the chair.

The minutes of the Ordinary General Meeting held on February 7, 1907, were taken as read and confirmed.

Messrs. A. F. T. Atchison and R. E. Shawcross were appointed scrutineers of the ballot for the election of new members, and, at the end of the meeting, the following were declared to have been duly elected :—

ELECTIONS.

As Members.

Robert Beattie, D.Sc.
Arthur Warren K. Peirce.

As Associate Members.

James Butler.
Bertram S. Cohen.
Frank Forrest.
Charles Matthew Forster.
Francis John Gerald Holden.
John Conrad Bedford Ingleby.
Frederick Margrie.
Alfred John Moore.
William Bolton Shaw.
Arthur Taylor.
Henry Howard Vallat.

As Associate.

Ernest Alexander Nash.

As Students.

Andrew Barr.
Robert Struthers Begg.
Frank Birch.
James Archer Birch.
Edwin Guthrie Bowers.
James Bowman.
Samuel Anthony Brooks.
James Westhall Brown.
William Richard Bullimore.
Percival Drew Butcher.
Edward Stanley Byng.
Herbert Stirling Carnegie.
Arthur Bertram Cartland.
Stephen Hayter Chase.
Alfred Lewis Cooke.
Basil Norbert Dolan.
John Leonard Eve.
Richard Francies.
Arthur Carr Hall.
Charles W. Hirst.
Percival Holliday.
Harold Kingsbury.
Charles Lawson Laing.
Francis Arthur Lawson.
Guy Ynyr Llewelyn Lloyd.
Arthur Lockwood.
James McCluney.
Harry Camden MacEwan.
Charles Marshall.
Edward Montague Marvin.
Ernest Morgan.
Norman Herbert Morris.
Joseph T. G. Philips.
Myles Herbert Roffey.
Arthur Ross-Jones.
Reginald J. Spencer-Phillips.
Frederick Ernest Squire.
William R. Steele.
James Gibb Stewart.
Edwyn James Stiell.
Sidney Fremlyn Streatfeild
Alexander Thomson.
Leslie Bruce Wallace.
John William Weber.

Donations to the *Library* were announced as having been received since the last meeting from Messrs. L. Amaduzzi, The Colliery Guardian Co., Ltd., A. Righi, E. and F. N. Spon, Ltd.; to the *Building Fund* from H. E. Harrison; and to the *Benevolent Fund* from G. B. Byng, M. B. Byng, H. E. Harrison, H. Hirst, J. P. Lawrence, A. W. Manton, G. Marconi, T. H. Minshall, J. B. Smith, W. C. P. Tapper, to whom the thanks of the meeting were duly accorded.

The PRESIDENT: The next matter I have to bring to your notice is an announcement which it gives me great pleasure to make, and I have no doubt it will give equal pleasure to all the members of the Institution to hear: Lord Kelvin has accepted the invitation of the Council to be nominated as President of the Institution for the ensuing year. It will be the third time that he will have held that office.

I now have the privilege of introducing to you Professor J. J. Thomson, and asking him to deliver his address on "The Modern Theory of Electrical Conductivity of Metals." I feel sure that I can assure Professor J. J. Thomson in your name that, though it is the first time he has visited us, his name and his work are well known, thoroughly admired, and appreciated by all the members of our Institution.

THE MODERN THEORY OF ELECTRICAL CONDUCTIVITY OF METALS.

By Professor J. J. THOMSON, F.R.S. (Cambridge).

(*Lecture delivered February* 21, 1907.)

It is somewhat remarkable that, though the passage of electric currents through metals is by far the most familiar case of electric conduction, yet our knowledge of the mechanism by which conduction is effected is far more definite and far more developed in the case when electricity is transmitted through gases or through liquids than when it is transmitted through metals. This is no doubt partly due to the fact that, thanks to the kinetic theory of gases, we have a very much more definite conception of the structure of a gas than we at present possess of a metal. But it is also, I think, due in part to the fact that the laws and phenomena associated with the conduction of electricity through metals are far less varied and show far fewer peculiarities than the conduction of electricity through gases. For instance, the conduction of electricity through gases shows so many peculiarities that it gives a clue to the mechanism; it gives itself away, so to speak, which the more regular and conventional metal refuses to do. In spite of this, some progress has of late years been made in the theory of metallic conduction, and it is some of the results of this theory that I wish to bring before you this evening. All the theories that I know of metallic conduction ascribe the passage of electricity through the metal to the movement of electrified particles. In the theory as originally developed by Riecke, and very shortly afterwards by Drude, it was supposed that particles, both positively and negatively electrified particles, were present in the metal, and that these moved under the action of the electric force carrying a current along with them. In a form of the theory which I published some few years ago, I limited the movement of the particles to the case of negative particles. I supposed that the actual carriers of the electricity in the metal were those small negatively electrified systems which we call corpuscles, and that the heavier positive particles took little or no part in the conduction of the electricity. This view has the advantage that the passage of electricity through a metal is not accompanied by any transport of the metal, a result which has often been looked for, but never detected. In the usual form of the theory, which I shall show later on requires some modification, the structure of the metal is

somewhat as follows: By the action of one atom of the metal on another, corpuscles are split off from the atoms, and they remain diffused through the mass of metal; so that we may picture to ourselves a metal as somewhat like a porous body, the pores of which are occupied by a substance with the properties of a perfect gas. In the older theory it was supposed that these corpuscles remained free for a time sufficiently long to enable them to get in thermal equilibrium with the metal itself, so that, like all gases, the average kinetic energy of the corpuscle was a constant merely depending upon the temperature.

With all gases the average kinetic energy is the same if the temperature is the same. As these corpuscles are exceedingly small compared with the molecules of hydrogen, the mass of a corpuscle being only about $\frac{1}{3400}$th part of that of a molecule of hydrogen, it follows that if the kinetic energy of the corpuscle is to be the same as that of a molecule of hydrogen, the corpuscle must move with very much greater rapidity than the hydrogen molecule at the same temperature, in fact, that the square of the velocity of the corpuscle must be 3,400 times the square of the velocity of the hydrogen molecule, with the result that the velocity of a corpuscle at 0° C. would be about 10^7 centimetres per second—roughly speaking, about sixty miles per second. These rapidly moving corpuscles are supposed to be disseminated through the metal, and to be moving with this velocity in all directions. If there is no external force acting upon them, though there is this movement of electricity through the metal, yet there is no transport of electricity in one direction rather than in another. There are as many of these corpuscles moving in the one direction as in the opposite, so that there is no resultant flow of electricity. But if we apply to these corpuscles an external electric force, then the corpuscles drift under the action of the force, drifting, since they are negatively electrified, in the opposite direction to the force, so that there is now a finite flow of these corpuscles through the metal, accompanied by the flow of the proportionate amount of electricity, and it is this flow which, on this theory, constitutes the electric current. We can on this view very easily find an expression for the conductivity of the metal in terms of the number of these corpuscles per cubic centimetre, and of their mean free path. The average velocity which the electric force gives to these corpuscles is practically the velocity which it can give in the interval between one collision and the next. The effect of the electric force is, as it were, annihilated when a collision occurs, and has to begin its work over again, so that if t is the interval between two collisions, X the electric force, and e the charge on the corpuscle, and m its mass the acceleration of course is $\frac{Xe}{m}$. If t is the time that elapses between one collision and the next, the time the acceleration has to act is equal to t, so that the velocity at the end due to the force will be $\frac{Xe}{m}t$.

One-half of this will be the average velocity, so that the average velocity which these corpuscles will acquire owing to the electric force X will be $\frac{1}{2}\frac{Xe}{m}t$. Let us call this velocity v, the amount of electricity that will pass through unit area in unit time will be the number of corpuscles which pass through that area multiplied by e, that is, nve, or substituting for v, $\frac{1}{2}\frac{Xne^2}{m}t$. This is the current i, thus $i=\frac{1}{2}\frac{Xne^2}{m}t$, so that the specific conductivity, called c, will be given by the equation $c=\frac{1}{2}\frac{ne^2}{m}t$.

That will be the expression for the conductivity of the metal. Let us try to estimate this time, t: when we are dealing with electric forces of the order of those which occur in metallic conduction, it is very easily shown that the velocity that, in the interval between one collision and the next, will be imparted to a corpuscle, is extremely small compared to the velocity of sixty miles per second which the corpuscle possesses in virtue of its temperature. So that the speed, at which the corpuscle moves from one collision to the next, will practically be that velocity of sixty miles per second, and be independent of the forces acting upon it. If we, for the moment, call V, the velocity of the corpuscle (10^7 centimetres per second), and λ the length of the free path, then $t=\frac{\lambda}{V}$, and the expression for the conductivity of the metal is $\frac{1}{2}\frac{ne^2}{m}\frac{\lambda}{V}$.

We want to get some check upon this result; it does not mean much as it stands; and I now proceed to consider the very remarkable result which Drude established for the connection between the electrical conductivity and the thermal conductivity of a metal. It is evident that if we have a metal filled with these corpuscles which are in temperature equilibrium with the metal itself, then if different parts of the metal are at different temperatures, the temperature of the corpuscles will be different at different parts of the metal, so that the energy of these corpuscles will tend to flow from the hot parts to the cold. There will be, therefore, in consequence of the freedom with which these corpuscles move, a conduction of heat arising from the corpuscles themselves. Now let us suppose for a moment that all the heat that is conducted in a metal is conducted by means of these corpuscles, so that the conductivity of the metal will be the conductivity of this collection of corpuscles. The conductivity of a gas has been worked out on the kinetic theory of gases, and expressed in terms of the number of molecules of the gas per cubic centimetre, the mean free path and the average velocity. The value of k, the thermal conductivity of the gas, is on the kinetic theory given by the equation $k=\frac{1}{3}n\lambda V\alpha$.

The quantity α, which occurs in this expression, is defined in this

way, $a\,\theta$ is the kinetic energy of the molecule of any gas at the absolute temperature θ. From this definition of a, it can be shown that a is about $1{\cdot}5 \times 10^{-16}$. If you look at the expression for k and the expression for c, and divide one by the other, you will notice that everything that is peculiar to the metal—that is, the number of corpuscles per cubic centimetre—and the free path of these corpuscles, goes out, and that

$$\frac{k}{c} = \frac{2}{3}\,\frac{m\,V^2}{e^2}\,a.$$

Everything has gone out, therefore, which is peculiar to the metal. $\frac{1}{2}\,m\,V^2$ is the kinetic energy of the corpuscle which is supposed to be determined by the temperature; that is to say, in whatever metal the corpuscle may exist, $m\,V^2 = 2\,a\,\theta$, so that the expression for k/c is equal to

$$\frac{4\,a^2\,\theta}{3\,e^2}.$$

Now this equation indicates that the ratio of the thermal to the electrical conductivity is the same for all metals. This result is not exactly true, but it is true to a very considerable degree of approximation for a pure metal. But we can go further than this. We can calculate what the ratio ought to be on this theory, and we can calculate it practically free from any hypothesis, and even free from any determination of the value of e by anything except the ordinary methods of electrolysis. I will just indicate, because it is rather interesting, the way in which we can determine this constant; that is, this value of $\frac{a^2\,\theta}{e^2}$.

The pressure of a gas on the kinetic theory is

$$\tfrac{1}{3}\,n\,m\,V^2,$$

n being the number of molecules per cubic centimetre, m the mass, and V the mean velocity of the molecules; or since $\frac{1}{2}\,m\,V^2 = a\,\theta$,

$$p = \tfrac{2}{3}\,a\,\theta\,n,$$

and therefore
$$\frac{p}{n} = \tfrac{2}{3}\,a\,\theta,$$

or
$$\frac{p}{n\,e} = \frac{2}{3}\,\frac{a\,\theta}{e}.$$

Now $n\,e$ is the number of molecules of hydrogen in a cubic centimetre multiplied by the charge on an atom of hydrogen. Now we know that one unit of electricity decomposes about $1{\cdot}2$ cubic centimetres of hydrogen at normal pressure and temperature, and therefore $2{\cdot}4\,n\,e = 1$; and since $p = 10^6$, we get $\frac{a\,\theta}{e} = 3{\cdot}6 \times 10^6$. From this we find that k/c, which is equal to $\frac{4\,a^2\,\theta}{3\,e^2}$ is, at $0°$ C., $6{\cdot}1 \times 10^{10}$ in absolute units.

Now lately some extremely valuable experiments have been made

on the ratio of the thermal to the electrical conductivities of various metals by Jaeger and Diesselhorst, and the values that they found for various pure metals for this ratio were: For copper, $6{\cdot}7 \times 10^{10}$; silver, $6{\cdot}8 \times 10^{10}$; and gold, $7{\cdot}09 \times 10^{10}$. Those are, you see, in very fair agreement with this value $6{\cdot}1 \times 10^{10}$ which has been deduced from entirely different considerations. I ought to say that for other metals the agreement is not so exact. For aluminium it is $6{\cdot}36 \times 10^{10}$; for iron it is $8{\cdot}02 \times 10^{10}$; and when you get up to alloys like constantine you get up to $11{\cdot}0 \times 10^{10}$. But in the case of mixtures of metals, as Lord Rayleigh has shown, some considerations come in which produce effects like a resistance which are not included in this new theoretical view. It is, I think, a very remarkable confirmation, that this ratio, calculated in this independent way, comes so nearly to the number found actually by experiment. But there is another confirmation. You notice that $\frac{k}{c}$ is proportional to the absolute temperature, so that the temperature coefficient of $\frac{k}{c}$ ought to be just $\frac{1}{273}$, or $0{\cdot}3665$ per cent. This point has also been investigated by Jaeger and Diesselhorst, and they find that the temperature coefficient of this ratio for copper is $0{\cdot}39$, for silver $0{\cdot}37$, and for gold $0{\cdot}36$. The value indicated by this expression is $0{\cdot}366$, so that there again you get close approximation between the results of experiment and the results of theory.

Perhaps even more interesting is a very remarkable investigation that has been made by Lorentz, in which he connects the ordinary radiation from a metal, or any substance at any temperature, with the corpuscles which are supposed to carry the electric current. These corpuscles are, as I have said, diffused through the metal; they are moving about with great rapidity, and are continually knocking against the molecules, and when they come into collision, their velocities are suddenly changed. When we start or stop an electrified particle we give rise to an electro-magnetic disturbance. The collisions of the corpuscles start pulses of electro-magnetic forces flowing through the medium, and though these pulses are quite irregular, yet they can be represented by means of Fourier's theorem as due to a collection of harmonic waves. Lorentz has attacked this kind of chaos of the forces produced by the collisions of these corpuscles inside the metal, and has tried to express them as a Fourier's series. He has only been able to work with one end of the spectrum, the long waves—waves whose length is large compared to the mean free path of the corpuscles; but he has found an expression for the amount of energy in the waves whose frequencies are between certain limits, provided these waves are long waves—long compared with the mean free path. The result that he finds is, that considering the energy in the waves whose frequency is between q and $q + dq$, the amount of this long-wave radiation given out by a slab of unit thickness would be $\frac{q^2\,dq}{6\pi^2 V} 4\pi e^2 n \lambda v$, where V is the velocity of light. He found that this is the expression for the energy

in the waves whose frequency is between q and $q + d\,q$ produced by the collision of these n corpuscles per cubic centimetre, having a free path λ and an average velocity v. That is the amount given by a slab; it is not the amount radiated by the metal. This radiation of course is absorbed as it passes through the metal, and the actual amount of radiation passing through the slab will be such that the amount of energy which is absorbed passing through the slab is equal to the amount radiated by the slab. Therefore we can easily find (I will not trouble you with the calculations) in terms of the resistance (the absorption here depends upon the resistance) the amount of energy that would have to pass through this slab and be absorbed, so that the absorption by the slab is equal to this radiation, and this with amount of energy is the energy passing through the slab. It comes out, then, that the amount of energy passing through each unit area of the slab in the radiation of whose wave-length is between $l + l + dl$ is $\frac{16}{3} \; \frac{\pi a \theta d l}{l^4}$.

That is the expression which, as I say, Lorentz has found for this long wave-length radiation; and you see again that everything peculiar to the metal has disappeared. The number of corpuscles and the free path of the corpuscle have gone out, and we get an expression which is the same for all these substances. It contains this quantity $a\,\theta$ in addition to the wave-length, and as we know $a\,\theta$, this expression can be easily calculated. It has been shown that, in order to agree with thermo-dynamics, the function which represents the amount radiated between certain limits of wave-length must belong to a general class, and this does belong to that class; it does not violate the laws of thermo-dynamics. Lorentz has calculated in this way the amount of energy radiated according to this expression, and has compared it with the experiments made by Lummer, Rubens, and others on the amount they have found to be actually radiated by a metal, and the two are in very close agreement. The ratio is about 1·5 to 1·3 in the two cases, so that here again this theory is confirmed. A most interesting point about this radiation is that it is all, so to speak, Röntgen rays, and the thermal radiation of the metal is, according to this, just a case of Röntgen rays.

But this is not all; there must be something else besides these long waves. If we analyse this disturbance produced by the collision of the corpuscles, there must be something else besides long waves. Lorentz has only calculated the energy in the particular case when the wave-length was very long compared to the mean free path. If you could express the effects of the collisions of the corpuscles completely, you would find you would have to include in the solution waves of very short wave-lengths. There must be another end to this radiation; the long wave part is only one end. Now, where is that other end? The metals, if this result is right, must give out, in addition to the thermal radiation, some other kind of radiation analogous in properties to ordinary Röntgen rays. There does seem some indication that metals

and other bodies give out radiation of that type. We find that each metal produces a characteristic amount of ionisation when a gas is enclosed in a vessel made of that particular metal, and it may be that the radiation that shows itself in this way is the other end, so to speak, of the effect of these collisions, one end being the ordinary thermal radiation and the other end being probably, I think, the peculiar radiation that is emitted by metals. At any rate, there must be some radiation besides that calculated by Lorentz.

I may say that this theory affords an easy explanation of the Peltier effect and the Thomson effect of the electro-motive forces along unequally heated metals.

But I must pass on. So far I have been considering phenomena which afford strong corroboration of this theory. I must now indicate one that I think requires us to modify the theory. It is of this character. If there are all these corpuscles in temperature equilibrium with the metal, then if we raise the temperature of the metal we have to raise the temperature of the corpuscles, so that a certain amount of energy will be required to raise the temperature of the corpuscles. That is, there must be certain specific heat corresponding to the corpuscles themselves as apart from that which raises the temperature of the atoms of the metal. The point I am going to try to bring before you is that, in order to reconcile some experiments to which I shall allude with the theory, it is necessary to suppose so many corpuscles in the metal, that the amount of heat required to raise these corpuscles through any range of temperature is far greater than the actual amount of heat that is required to raise both the metal and the corpuscles. This is founded upon some experiments that have been recently made by Rubens and Hagen on the conductivity of metals under very rapidly alternating electric forces, those forces that occur, for example, in waves of light. They experimented with very long waves, chiefly two sets of waves, one whose length was 25 μ, and the other set having a length of 4 μ, μ being $\frac{1}{1000}$ mm. They found that the conductivity for the metal for the light waves, if you can call them light waves, whose length is 25 μ, was, within their limits of experiment, exactly the same as for a steady current, and that for the waves whose wave-length was 4 μ it only differed by about 20 per cent. Now, if you look back at the way in which we calculated the conductivity, you will see that the expression we obtained is only true when the electric force acts in one direction for a time that is long compared with the interval between two collisions. If the electric force is going backwards and forwards, alternating many times between one collision and the next, then the actual velocity communicated to the corpuscle is very much smaller and the conductivity is very much less. There is no difficulty in calculating the effect of this rapid reversal of the forces on the conductivity. I find that if the forces are reversed so quickly, that t, the interval between one collision and the next were as much as one-quarter of the time of vibration of the light, the effective conductivity would be reduced to one-half. Rubens has shown that the conductivity under waves whose

wave-length is $\cdot 4\ \mu$ is only about 20 per cent. less than the normal conductivity, and therefore the interval between two collisions cannot be as great as the time of vibration of light whose wave-length is $\frac{1}{4}$ of $4\ \mu$, *i.e.* μ. Since μ is 10^{-4} cm., the time, t, that elapses between one collision and the next must be less than $10^{-4}/3 \times 10^{10}$ or $3\cdot3 \times 10^{-15}$ sec., which is a very small time. Now, we see from the expression $C = \frac{1}{2}\frac{ne^2}{m}t$ that if you are going to reduce the time and yet get definite conductivity, you must increase the number of the corpuscles per unit volume. If you have a very short time, you must make up for it by a very large number of corpuscles. Take the case of silver and work out the number of corpuscles per cubic centimetre that would be required to give the observed conductivity. With silver the number is $1\cdot8 \times 10^{24}$ per cubic centimetre.

We require that number of corpuscles per cubic centimetre to give, in the time deduced from the experiments of Rubens and Hagen, the proper electrical conductivity of silver. Now, you have that number, each possessing at the absolute temperature θ an amount of kinetic energy $\alpha\ \theta$, and therefore to raise the temperature one degree you would have to give to each one of these an amount of energy α, or to the whole lot an amount of energy represented by $\alpha \times 1\cdot8 \times 10^{24}$. α is about $1\cdot5 \times 10^{-16}$, so that this energy is $1\cdot8 \times 1\cdot5 \times 10^{8}$ ergs, that is, somewhere about 7 calories, is required to raise 1 cubic cm. of silver 1° C. if you leave the atoms of silver alone and merely attend to the corpuscles. The actual amount required to raise the temperature of the silver is only about $0\cdot6$ calories, so that there is a serious discrepancy between the results of the theory in this form and the results of experiments on specific heat. I do not see how to reconcile the two. If you want to get the observed conductivity you must have a large number of corpuscles if you take Rubens's and Hagen's experiments as giving a limit to the time that elapses between two collisions, and if you get this number of corpuscles you get too big a specific heat. I think that is a difficulty with regard to this theory in the form in which it is usually given. I think the difficulty arises from supposing that the corpuscles exist so long in a free state in the metal that they have time to get into temperature equilibrium with the metal. I think the actual state of things is somewhat different—that the corpuscle, instead of wandering about, when it once gets dragged out of one atom by the action of a neighbouring one, jumps practically straight from one into the other. There is no doubt that it must be due to the action between the atoms of a metal that these corpuscles are produced, because if you take, for example, mercury in a state of vapour, the number of corpuscles per molecule of mercury in the state of vapour is infinitesimal compared with the number that would have to exist to explain the conductivity of mercury in its liquid condition. It is the effect of one atom on another that gives rise to these corpuscles.

The modification which I think is required in this theory is to suppose that the electric force, instead of acting on the corpuscles

after they have left their atoms, really acts upon the atoms before the corpuscles leave them. Imagine the atoms acting on each other, like a system of electric doublets, and the corpuscle flows from the negative end of one doublet into the positive end of the other.

If you have all the doublets arranged higgledy-piggledy in the metal, then there will be as many corpuscles going one way as there are going the other; there will be no transport through the metal. But supposing the action of the electric force is to polarise these doublets before they discharge, so as to drag them into line, like a Grotthus chain, on the old theory of electrolysis. Supposing, for example, you drew them all into line with the negative ends pointing in one way and the positive in another, you would get a transport. When the corpuscles jump they will jump the same way, and you would get a definite transport of electricity through the metal. I think the modification that is required is to suppose that, when the electric force acts upon the metal, what it does is to arrange to a certain extent these atoms or doublets which are acting upon each other into a line, so that when they discharge the corpuscles from one to another, these corpuscles go in a definite direction. I have worked out the result, taking this theory instead of the one that I have brought before you; but as it is getting so late I will only bring before you the general result. I will mention, however, the hypothesis I made. I assumed that these doublets would range themselves like doublets in a gas. We know, from the law of the kinetic theory of gases, how many of these doublets would be pointing in any direction under the action of an external field. We know the law of distribution, for systems having given amounts of potential energy in a gas. If d is the length of the doublet and e the charge, the moment of this doublet is $\frac{ed}{X}$, and if it makes an angle with the direction of the electric force X, then the potential energy is $Xed\cos\theta$, and applying the law of distribution we can very easily find the average value of $\cos\theta$—that is, the excess which points in one direction rather than in the opposite. Assuming that the same law holds for the metal as holds for the gas, then I found that the electrical conductivity, which I call c, is—

$$\frac{2}{9}\,\frac{p\,b\,d\,e^2 n}{a\,\theta}$$

p is the number of times that each one of these doublets discharges per second; d is the distance between the positive and the negative component; b is the distance between the centres of adjacent doublets; e is the charge; n is the number of these doublets per unit volume, and a, θ are the same as before. Then with regard to the thermal conductivity, I assumed that the corpuscle, when it starts from a hot part of the metal, has more kinetic energy than when it starts from a cold, so that the shooting out of the corpuscles from the hot to the cold doublets carries the heat with it, and we get thermal conductivity in that way. If we do that, assuming that the corpuscle

in a doublet at the temperature θ has got an amount of kinetic energy equal to $a\theta$ we have $k = \frac{1}{3} p\, b^2\, n\, a$; k being the thermal conductivity.

If we take the ratio of the one to the other $\frac{k}{c} = \frac{2}{3} \frac{e^2}{a^2 \theta} \frac{b}{d}$, on the other theory $\frac{k}{c} = \frac{4}{3} \frac{e^2}{a^2 \theta}$.

The ratio of the two ratios on the two theories is thus $\frac{7}{8}$ of $\frac{b}{d}$, where b is the distance between the centres and d is the length of one of these doublets; so that if the doublets are very closely packed, $\frac{b}{d}$ will not be a large quantity. $\frac{b}{d}$ will be very nearly equal to unity, and the factor is only $\frac{7}{8}$. So that there will only be a difference of about 12 per cent. in the value of the ratio of the thermal to the electrical conductivity on the two theories, and it cannot be said that the agreement between theory and experiment in either case is sufficiently close to say that one theory is a closer approximation to the facts than the other. I have worked out the radiation in the same way, and again it is just the same as before, with the exception that the factor $\frac{b}{d}$ comes in, and again I do not think the difference is great enough to be determined by any existing experiments. This view is not open to the difficulty about requiring a large specific heat, because, during the time when the things are free, they just jump from one to the other and do not get into thermal equilibrium. It is never necessary to consider the amount of energy required to raise the temperature of a very large collection of them.

In conclusion, there is just one point I would like to illustrate by means of this theory. The Hall effect is always a considerable difficulty with regard to the conduction of electricity through metals. In the first form of theory I gave we do get a Hall effect, but it is always in one direction. The electrified negative particles, as they are placed in a magnetic field, are acted upon by a force at right angles to the direction in which they are moving. They are pushed, if they are moving horizontally, up or down by the magnetic field at right angles to the current, and it is that transverse current at right angles to the initial current which corresponds to the Hall effect. I say there is some little difficulty that, with this theory, the Hall effect would always tend to be in one direction. The Hall, however, is a very complex phenomenon, because it changes sign not only in different metals, but in the same metals occasionally, if the magnitude of the magnetic forces is altered. There are certain metals in which the effect has one for some force and the opposite sine for other forces, so that the Hall effect is not a very simple phenomenon. It may be asked, How can the magnetic fie possibly produce any such effect as this on the view I have just b giving, if it only acts upon the molecules before the interchange corpuscles takes place and not after? I think it is rather an intere point to see how it would.

Suppose that we have a doublet with negative and positive ends, then if it is pulled round by an electric force the ends start moving, one in one direction and the other in the opposite, and if there is a magnetic force acting upon the thing at right angles to the plane of motion, then there will be forces acting upon these two ends. If the ends move with equal and opposite velocities, the forces on the two ends are equal; but if the velocities are different, then the forces on the ends are different and there is a couple produced—in fact, the doublet when placed in a magnetic field behaves exactly like a gyroscope. Suppose we have a pendulum with a gyroscope bob instead of the ordinary bob; suppose it were held away from the vertical, corresponding to one of our doublets before an electric force acts; supposing, now, it is let fall; gravity, instead of pulling it straight down to the vertical, will make it swing round like a gyroscopic pendulum one way or the other, according to the direction of the spin in the top in the bob. So that if this gyroscope property were attached to a doublet, then when there is an attempt to pull it along the lines of electric force it will come out a little at right angles, and there will be a current either that way or the other way according to the sign corresponding to the spin of the top of the gyroscope. So that, in addition to the movement along the direction of the line of force, the doublets would tilt up a little bit at right angles to it; there would be a polarisation in the direction of right angles to it, and that polarisation would produce a current in that direction, so that unless the negative and positive ends move with equal and opposite velocities—that is, unless the centre of gravity of the doublet is exactly midway between the negative and positive charge—then these doublets when they are placed in a magnetic field will be acting like gyroscopes, and if it is attempted to pull them by a force in one direction they tend to squirm off in a direction at right angles to it, and that, I think, accounts for the Hall effect in metals.

Discussion.

Lord Rayleigh, F.R.S.: Gentlemen, I think the Institution is very fortunate in hearing from the most qualified man in the country, perhaps in the world, his exposition of these matters. Certainly, if one could ever understand a difficult subject expounded in an hour's time, to-night was the opportunity, for Professor Thomson has stated his case with extraordinary lucidity. I was especially interested myself in a formula which represented the results of Lorentz's researches deducing the law of radiation for great wave-lengths from the phenomenon of the collision of corpuscles within the pores of a metal. It so happens that, some years ago, I put forward practically the same formula, but deduced only from very general considerations, and without any particular idea as to the precise machinery by which the result might be reached. Professor Thomson has explained to us the theories which originated mainly, I think, with Drude and Lorentz, and to a great extent explain many very remarkable facts, Lord Rayleigh.

Lord Rayleigh.

leading to remarkable numerical coincidences, and he has also pointed out to us some difficulties which arise when the application is pursued further. In the latter part of his lecture he has indicated to us the results of researches of his own, which give quite a new complexion to the matter, and point out a way in which these difficulties may be overcome. It is to be hoped that the theory will work out in all the details as well as it has done in the matters which Professor Thomson has laid before us.

The President.

The PRESIDENT: Professor Thomson has kindly intimated to me that he is ready to answer questions put to him or take part in any discussion. I will therefore call on Professor Silvanus Thompson to speak.

Professor Silvanus Thompson.

Professor SILVANUS P. THOMPSON: You have asked me, sir, to put questions to the one man in the universe who understands best this matter; and I am uncommonly glad of the opportunity, because, as a matter of fact, I have found it one of the difficulties of the study of this branch of the subject that the different authorities who write and talk about it do not always speak in the same language. We have heard a good deal to-night about corpuscles; I do not think we have heard one thing about electrons. I want to know whether in the lecturer's usage those two words mean the same thing; or, if I may put it in another way, do we understand, by that which we have been hearing of to-night under the name of "corpuscle," a minute portion of matter much smaller than an atom and electrified? Or, is there no matter at all in it? Is it simply a little bit of electricity? Is it a disembodied bit of electricity which acts as a corpuscle, or is it an electrified bit of matter? We desire something definite about the terms which are used, and precisely what they connote. Then, to come to the immediate subject of the intensely interesting new theory, which was given to us all too briefly at the end of the lecture, I wish to put this question. If I caught the words rightly, it was put to us that the corpuscle is dragged out of the atom by the action of a neighbouring atom, and jumps straight from one to the other. Can Professor Thomson give us any idea of what is the origin or cause of such an effect? What is there in one atom which should act on the corpuscles of another atom and make them jump from one to the other? It is a new point to me in physics, and I think it deserves a little further explanation, especially if it will make the difference between the old theory and the new theory more clear. I have put my questions, sir, at your suggestion; but I cannot sit down without joining my thanks to those of Lord Rayleigh, and I have no doubt of the whole Institution, to Professor Thomson for his extremely important exposition of these highly recondite matters.

Sir William Preece.

Sir WILLIAM H. PREECE, K.C.B.: I am delighted at the opportunity of proposing a vote of thanks to Professor Thomson. We are old friends, and I have learned very much from him. We have had a unique lecture to-night. I cannot recall, in the history of the Institution of Electrical Engineers, a learned professor, the head of his profession

Sir William Preece.

now, coming here and giving us a discourse like that which we have enjoyed. We have not only enjoyed it to-night, but there is not a man in this room who has not had something impressed on his brain which has set it in a state of energy, which will lead to thought and to study. It is a gratification to us English engineers to know that the great steps in advance—for they must be steps in advance—in the theory and practical knowledge of electricity come from Cambridge. Professor J. J. Thomson is carrying on the great work commenced by Lord Rayleigh at the Cavendish Laboratory, and it will be continued, I am sure, until the questions will be answered whether electricity is a form of energy, a form of matter, or something *sui generis*. Gentlemen, with very great pleasure I propose that a hearty vote of thanks be given to Professor Thomson for the delightful address he has given to us to-night.

The President.

The PRESIDENT: Gentlemen, you have already shown by your applause that you accept unanimously this vote of thanks to Professor Thomson, which I now convey to him in your name.

Professor Thomson.

Professor J. J. THOMSON, in reply: Perhaps I can best show my appreciation by trying to answer the questions which Professor Silvanus Thompson addressed to me. I think his first question was a question rather of notation, as to the difference between the electron and the corpuscle. I prefer the corpuscle for two reasons: first of all, it is my own child, and I have a kind of parental affection for it; and, secondly, I think it has one merit which the term electron has not. We talk about positive and negative electrons, and I think when you use the same term for the two the suggestion is that there is an equality, so to speak, in the properties. From my point of view the difference between the negative and the positive is essential, and much greater than I think would be suggested by the term positive electron and negative electron. Therefore I prefer to use a special term for the negative unit and call it a corpuscle. A corpuscle is just a negative electron.

Professor Silvanus Thompson.

Professor SILVANUS THOMPSON: What do you call a positive electron?

Professor Thomson.

Professor J. J. THOMSON: I think I should call it a positive electron. I like the term electron well enough if it is not liable to run the positive and negative into an equality. Then Professor Silvanus Thompson went into some questions which, if I could answer, I should be very near solving the problem of the universe—the relation between electricity and matter, and whether a corpuscle was a bit of electricity or a bit of matter with a charge on it. I do not know what electricity is, and I do not know what matter is. I have tried to state what I regard as energy. Taking the corpuscle, I think that all the energy possessed by a corpuscle is due to the magnetic field that it produces when in motion, or kinetic energy. I think I should like to ask those people who talk about electricity and matter to try to think for themselves what they mean by matter and what they mean by electricity. If they do so, they will not find it so easy to define the terms they mean. Then with regard to the

rofessor homson.

question of the action of one atom upon another, I myself regard an atom as a collection of positive and negative electricity, not exactly neutral, but somewhat analogous to a magnet with positive and negative poles. I do not see any difficulty at all in supposing that, when two such systems come near each other, they exert forces upon each other. Two magnets will certainly exert forces upon each other, and I do not see why positive and negative electricity should not have corresponding forces.

In conclusion, I have only to thank you for your kind vote of thanks. I owe to your President a debt of gratitude for the great assistance he gave me during the time that he was connected with the Cavendish Laboratory; and I felt that in acceding to his request to lecture here I was able in some measure to repay that great debt of gratitude.

The meeting adjourned at 9.30 p.m.

Proceedings of the Four Hundred and Fifty-Third Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, March 7, 1907—Dr. R. T. GLAZEBROOK, F.R.S., President, in the chair.

The minutes of the Ordinary General Meeting held on February 21, 1907, were taken as read and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

TRANSFERS.

From the class of Associates to that of Members :—

Percy B. Crowe. | Chas. W. G. Little.

From the class of Associates to that of Associate Members :—

Reginald Norman Torpy.

From the class of Students to that of Associate Members :—

Gilbert Moss.

Donations to the *Library* were announced as having been received ·ce the last meeting from C. G. Allingham, A. C. Cormack, Messrs. :hier-Villars, E. G. Hillier, The University of Manchester ; to the *ding Fund* from R. H. Burnham, W. W. Cook, J. F. Henderson, G. Shee, J. C. Smail, J. M. Smyth ; and to the *Benevolent Fund* from . Gibbs, C. J. Wawn, to whom the thanks of the meeting were duly ›rded.

The PRESIDENT: All members of the Institution will have read, I am sure, with the deepest grief and sorrow of the death of M. Henri Moissan. Though he was not actually a member of our Institution, his work for electrical science has been so great and of such vast importance that you will, I trust, think the Council are doing right in sending to his representatives a statement of their sense of the loss electrical science has sustained by his death, and that I am right in bringing this matter to your notice and asking for your approval of the action.

The following paper was read and discussed, and the meeting adjourned at 9.40 p.m. :—

THE TRANSMISSION OF ELECTRICAL ENERGY BY DIRECT CURRENT ON THE SERIES SYSTEM.

By J. S. HIGHFIELD, Member of Council.

(Paper read March 7, 1907.)

It is long since series systems of transmission were used for arc lighting with direct and rectified currents of constant value, and small power transmissions have been put down consisting of two series machines, the one running as generator and the other as motor, but with the exception of a number of installations erected in Switzerland and elsewhere on the Continent, most of them of comparatively small size, the parallel alternate-current system has been used for all transmission work.

The ease with which the pressure of an alternate current can be altered, and the simplicity of the apparatus required for the purpose, the solidity of the generator construction, and the convenience of parallel working, will probably cause this system to hold its present field. When, however, very long transmission systems are necessary, involving the use of very high pressures, many difficulties, chiefly due to impedance and capacity of the line, are encountered. These difficulties have been recognised and pointed out by various authorities, but by the exercise of great skill they have been largely overcome. They can, however, be eliminated only by the use of direct current, and in view of the increasing lengths of transmission lines and of the amount of power transmitted it may be useful to examine what has been done in this direction to appreciate its advantages, to realise its limitations, and particularly to compare the relative value of alternate and direct currents for long-distance power transmissions.

I have for a long time been impressed with the advantages of direct current for long-distance transmissions, but during the last two years the problem was directly forced on my attention owing to the fact that the company to which I am engineer has obtained certain powers of supply in a very large area, aggregating 300 square miles, with a circumference of, roughly, 80 miles, which it is desirable to supply through underground mains from a station situated on the circumference. In this case it at once became apparent that the cost of the cable system was the dominating factor in the problem, and therefore I found it necessary carefully to consider any system which offered a saving in this respect. Consequently I have very closely investigated

the possibilities of the direct-current series system for supplying such an area. In studying the system for this special case it was necessary to extend the investigations into its possibilities in other fields, and it is owing to these circumstances that this paper comes to be written.

It is impossible to go further without making reference to M. Thury, who has for years steadily worked out the details essential to the direct-current series system, and has since 1889, when he put down his first system, designed and carried out schemes of gradually increasing size and importance, culminating with the Moutier-Lyon system, which was put to work last year. He has continually increased the working pressures and simplified the apparatus, till for careful attention to detail and general excellence of design it compares favourably with alternate-current apparatus, on which so many minds have for years been at work.

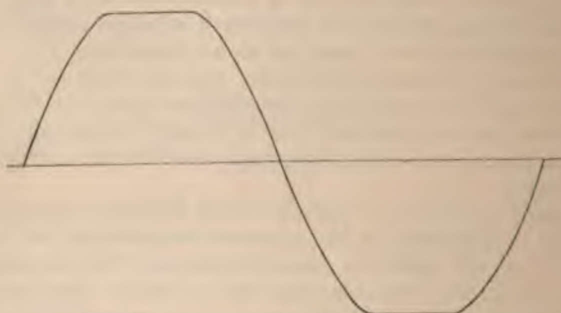


Fig. 1.—E.M.F. Wave Form of Alternator used in the "Comparative" Experiments.

Considerations Affecting Working Pressure.

In considering the relative advantages of alternate and direct currents for long-distance work, the first important matter to consider is which of the two systems offers the greater possibility for the use of high pressures. It is quite easy to produce alternate currents at exceedingly high pressures by means of static transformers. In order to obtain the same result from direct current it is necessary to put the generators in series. In this way there are no insurmountable difficulties in obtaining any direct-current pressure at which the line could be made to work.

Until recently but little was known of the effect of direct current at very high pressures on insulating materials, and since there was much information with regard to the effects of alternate current, it became important to establish their relative breaking-down effects. To this end M. Thury built five machines, three giving 20,000 volts direct current each, the other two 25,000 volts each. The machines were built with fixed commutators and armatures and revolving brushes and fields, and, although the volts between the sections of the commu-

tator were about 500, the machines worked most successfully. The current was about 1 ampere. In order to compare the difference between alternate and direct currents a 75-k.w., 50-∿ machine was used. Fig. 1 shows the E.M.F. wave-form. It will be noted that the curve is rather flat as compared with that given by most machines, and this, together with the fact that the number of commutator segments in the direct-current machines was comparatively small, giving a slight waviness to the direct current, caused the results somewhat to favour the alternate current.

It is not necessary to give the results of these tests in detail, but shortly it may be said that experiments were made by applying pressure to many types of porcelain insulators and to sheets and blocks of common insulating materials.

The first noticeable difference is that insulators which heat up on the application of alternate current do not heat with the application of direct current. Further, no crackling or brush discharge takes place in the neighbourhood of breakdown in the case of direct current; 60,000 volts direct current will not break down ordinary telegraph insulators provided they are well vitrified and generally of good quality.

Tests were made on sheets of presspahn; the samples used were 5 mm. thick, and the tables hereunder show the sort of results obtained. These may be taken as typical of tests made on various other insulators.

1. Sheet of presspahn, 5 mm. thick, alternate current:—

Test.	Length of Pressure Application.	Pressure. R.M.S.	Observations.
1	1½ minutes	9,000 volts	—
	30 seconds	11,000 volts	Punctured.
2	2 minutes	9,000 volts	Strong discharge.
	15 seconds later ...	9,000 volts	Punctured.

2. A similar sheet of presspahn, tested with direct current:—

Length of Pressure Application.	Pressure.	Observations.
2 minutes	10,000 volts	—
2 minutes	15,000 volts	—
2 minutes	18,000 volts	—
2 minutes	20,000 volts	—
4 minutes	25,000 volts	Punctured.

The above samples, after being twice punctured with alternate current with an average of 2 minutes' duration of pressure at

10,000 volts, stood a pressure of about 15,000 volts direct current, and broke down only after a further application of 25,000 volts direct current for 4 minutes.

3. Slabs of marble 20 mm. thick were tested; these punctured after 75 seconds' exposure to alternate current at 20,000 volts, or after 2 minutes' exposure to alternate current at 15,000 volts.

Direct current was applied for 15 minutes, starting at 10,000 volts and increasing gradually by increments of 5,000 volts at 2-minute intervals to 45,000 volts, when puncture took place.

Further tests were made to ascertain the relative sparking distances of alternate and direct currents, and the results of some of the tests are set out in the form of curves in Figs. 2 and 3. It will be noted from these that the sparking distance with the same pressure is about twice as great with alternate current as with direct current. As a result of many tests made it may be taken that a direct-current pressure

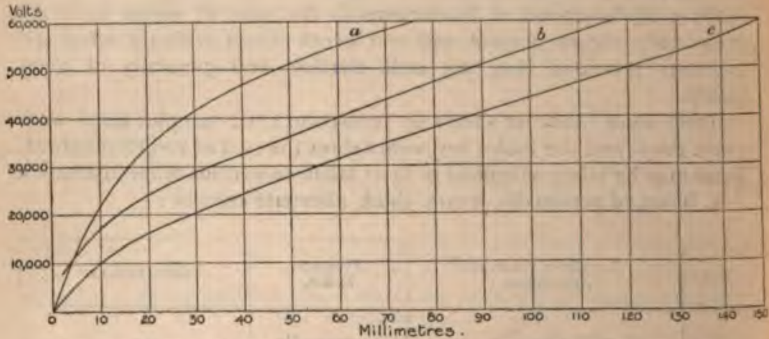


FIG. 2.—Curve of Sparking Distances (Alternating Current, 50 ∿).

a. Sphere to Sphere. *b.* Plate to Sphere. *c.* Plate to Point.

at least twice as great as an alternate-current pressure may be used on the same insulators and the same cable.

The photograph, Fig. 4, shows one of the poles used on the Lausanne line, which carries 4,476 H.P. 35 miles. It is not imposing, but appears to be quite adequate, and by comparison makes much work at home appear unduly expensive. The insulators are quite small, but many careful measurements with 20,000 volts between the line and earth show that on a very damp and foggy day the total loss over 3,000 insulators, together with lightning arresters, amounts only to 866 watts.

It is a simple matter to work a direct-current system on the equivalent of a 3-wire system—that is to say, the middle point of the system is connected to earth so that one line is worked at a pressure above earth and the other line at a pressure below earth. In this way, therefore, and using the same insulation, the effective direct-current pressure can be doubled.

It has been found practicable to work overhead lines with alternate current at 60,000 volts across the wires of a 3-phase system, say, 40,000 volts above earth. It would therefore be equally possible to work with direct current at 80,000 volts above earth, or at 160,000 volts between wires.

With the exception of a short length on one system all the Thury lines have been worked with overhead wires, but on systems where it is essential to carry the mains underground the advantage of the series system over an alternate-current system is much enhanced.

There is no special difficulty in making a single-core cable to work with direct current at 60,000 volts pressure to earth; therefore, with two such cables an effective pressure of 120,000 volts can be obtained.

FIG. 3.—Curve of Sparking Distances (Direct Current).

A. Sphere to Sphere. *B.* Point − to Plate +. *C.* Plate + Sphere − and Plate − Sphere +. *D.* Point + Plate −.

This cable can be more easily built than a 3-core cable to work with 20,000 volts between conductors.

Compared with a 3-phase line it may shortly be said that, apart altogether from the lesser difficulties of insulation, the overhead line carries two conductors instead of three, and the underground system consists of one twin or two single-core cables against one three-cored cable; in addition, and chiefly, all capacity and self-induction difficulties are avoided, however long be the line; in fact, so far as electrical difficulties count, the line may be of any length.

The actual pressures hitherto used are shown in Table I. (p. 489), which sets out the important features of schemes that have been in operation for several years. It will be noted that the highest pressure

actually in use to-day is 58,000 volts on a 3-wire system, that is, 29,000 volts above and below earth; the line was tested with a pressure of 100,000 volts above earth.

Series and Parallel Systems.

In all his systems M. Thury uses a constant current, as this greatly simplifies the design of the generators, motors, and regulating apparatus; and, as will be shown, the line losses due to this constant current have a very small money value. In most of these systems the main transmission line is taken into manufacturers' and other private premises in order to work motors for their use. In such cases it is generally necessary to keep the line current constant throughout the twenty-four hours, as, of course, any reduction in the line current would reduce the possible output from each machine, although the speed would not be varied. When, however, the system consists of generating stations transmitting to sub-stations where the whole of the series line and plant is under one control, and where all the consumers' supply is given on the secondary side, then it is not necessary to maintain the current constant throughout the twenty-four hours, but the current during light-load times may be reduced by about 30 per cent., thus considerably improving the efficiency of the line. The generators and motors are connected in series along the transmission line, the generators not necessarily being in one station, but in two or more stations.

It is so usual now to think of parallel working that it may be useful at this stage to point out shortly the differences between it and series working. These are of two classes, namely, the differences which affect the working and those which affect the initial design of the system.

Dealing with the working differences first: Fig. 5 shows diagrammatically a series system consisting of three generators and several motors, and Fig. 6 a single-series generator and motor, switches, and all instruments. The generators and motors are switched in series by means of a 4-way switch shown diagrammatically. When no plant is running each machine is short-circuited, and the line is connected in a single loop. To switch in a generator the machine is run up until it gives the proper line current. The switch is then opened, leaving the generator connected in series with the line, through which a constant current then flows.

To connect in a motor the switch is opened so as to allow a constant current to flow through the motor, and the brushes—which have been in the non-working position—are then rocked back so as to cause the motor to start; it is run up to speed by continuing to rock the brushes. When at full speed the automatic regulator gear takes charge, and by varying the brush position maintains the speed constant. Other motors and generators are run up in the same way as the load increases. To shut down a motor the brushes are slowly brought round to the non-working position so that the machine stops, and its short-circuiting

FIG. 4.

switch is then closed. The generators are shut down in the same way. As the load increases the total resistance in circuit, and, of course, the counter E.M.F. of the motors, steadily increase, so as to call for a greater pressure at the power station.

The effect of a short-circuit is to remove the load, and, provided the short-circuit is not put on too suddenly, the regulator gear can reduce the voltage so as to maintain the line current at its proper constant value. To provide for sudden short-circuits the generators are coupled to the prime movers through a friction coupling, which slips

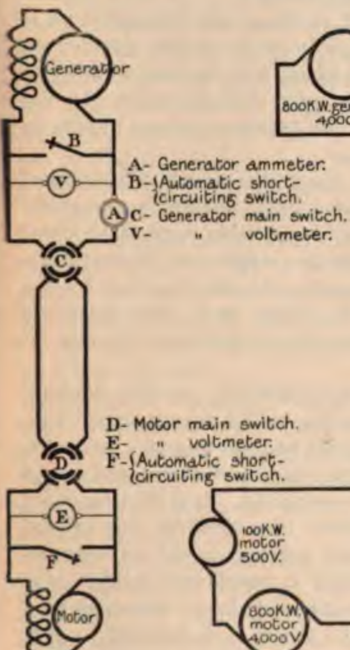


FIG. 5.

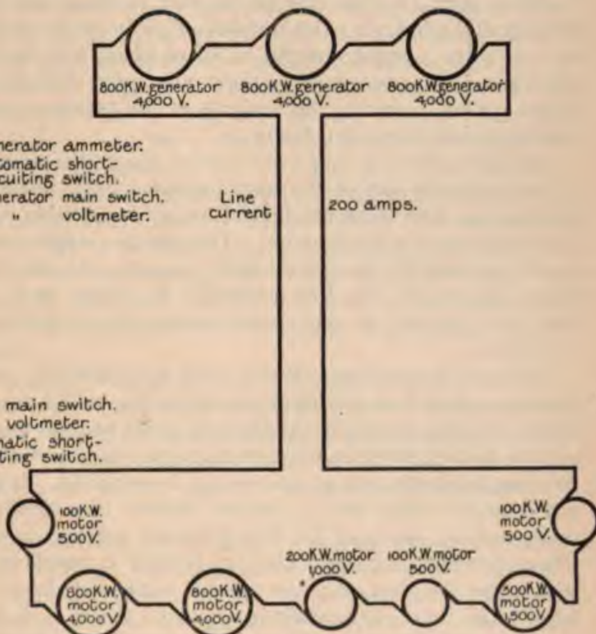


FIG. 6.

FIG. 5.—Diagram of Connections, showing all Switches and Instruments for One Motor and One Generator.
FIG. 6.—Diagram of Series Direct-Current System.

when the torque exceeds the full load torque by 20 per cent. This slipping allows sufficient time for the automatic gear to work, so that no damage is done to the plant. One important advantage of the series system is that failure of a prime mover resulting in the shutting down of a generator does not interfere with the supply, and no automatic gear is required to provide for such a contingency, as the current continues to flow through the shut-down machine.

Consider now the second class of differences which affect the design of the system. The simplest systems to lay out are those

where an ascertained load—not subject to increase—is to be supplied. In such a case the problem is equally easy with the series system as with the parallel system. Account must be taken of the amount of the load and its distance from the power station, the working pressure, the size of units, and the current density in the conductors.

So far as both systems are concerned, the considerations which decide the pressure of supply are the same, but in the case of the direct-current series line, owing to the fact that the cost of insulation is considerably less, under the same conditions it will usually be found cheaper to work at a considerably higher pressure.

With regard to the size of the unit, in the parallel system there is no difficulty whatever: the number of units can be readily decided on so as to allow a proper margin of spare plant, and the total number of machines is at once fixed without any other consideration. In the series system, however, the limiting of the pressure per machine is an additional and important factor.

Series machines giving from 2,000 to 3,000 volts on one commutator, according to the size of the machine, have been working successfully for years, and for large machines it would be possible to go up to about 6,000 volts on one commutator. The size of a single unit is, therefore, restricted both by the permissible pressure per machine and by the value chosen for the line current. In many cases two machines may be coupled to one prime mover, thus doubling the size of the unit.

As has been shown, 100,000 volts is a perfectly possible working pressure; therefore 500 amperes in the line will transmit 50,000 kilowatts. At this current each machine could be built to give 5,000 volts, and by driving the generators in pairs the size of each steam unit would be 5,000 kilowatts, and 10 units would be required. If it be desired for any reason to work with a smaller number of generators this can be done only by reducing the line pressure and increasing the current. When, however, the usual class of scheme is considered, namely, one where the business starts on a small scale and slowly increases to a large scale, more particularly in a scheme where it is difficult to predict what will be the ultimate extent of the demand, then between the series and parallel systems there are important differences to consider.

In the parallel system, provided the area be restricted, the pressure is decided according to the distance to which energy is to be transmitted. The rate of transmission is not important, the undertaking starting with small units, units of increased size being added as required. The distance, however, to which energy can be carried is at once limited by the pressure adopted.

With the series system, on the other hand, if at the start the load be relatively small, and only a small line current be therefore required, difficulties arise. Since the line current practically determines the maximum size of the unit to be adopted, it follows that the size of the unit is settled for all time unless the line current be increased,

and increasing the line current involves the changing of the whole of the machines. It is, therefore, usually necessary to adopt a larger line current than is immediately required, resulting in larger percentage line losses in the first years of the undertaking.

This difficulty may be got over by using two machines to form each main and sub-station unit. In the early stages each pair of machines is coupled in series, thus working with half the ultimate line current with one line. Subsequently, as the load increases a second line is erected to work in parallel with the first; at the same time the two machines forming each unit are coupled in parallel, thus doubling the current and the capacity of the system. This method involves some extra cost in the first years of the business, due to using double motors for driving each sub-generator, which motors are used first in series and subsequently in parallel. It would, of course, be used only with long and expensive lines. It is used on the Moutier-Lyon line, the current in the first years being fixed at 75 amperes, subsequently to be increased to 150 amperes.

The current density in the conductors must be decided according to the energy and volt losses that may be permitted. In the parallel system they should not be too high, otherwise the full load losses are great, resulting in bad regulation; also it must be remembered that the energy losses at maximum load have a great money value, and to ascertain their true importance it is necessary to determine exactly what this may be.

In systems supplying from water-power stations or from economical steam stations the apparent lower energy efficiency in the line is not of any importance; where the cost of energy is high, however, it may become important. In the latter case it will usually be necessary to work at a lower current density with the series system than with the parallel system.

The percentage loss due to the direct constant current, in many cases, appears to be large, but it must be remembered in all such cases that the important matter is the percentage money loss. It is like many other problems in engineering, which when considered purely from a scientific point of view appear to be of importance, but are not so when considered commercially.

In Fig. 7 are set out in the form of curves the line losses at various values of the line current and at various loads and load factors, in all cases the current density being fixed at 500 amperes per square inch and the line pressure at 100,000 volts. From the curves it will be seen that for a system designed for any maximum output at 100,000 volts and 500 amperes density per square inch when fully loaded the line loss will be 0·44 per cent. per 10-mile run at 100 per cent. load factor and 1·76 per cent. at 25 per cent. load factor, or if the maximum at first amounts only to 25 per cent. of the maximum load for which the system was designed at 100 per cent. load factor, the energy loss is 1·76 per cent. and at 25 per cent. load factor 7·04 per cent. The curves are useful in determining the proper line current to use for any fixed

or increasing load at varying load factors. If the pressure be reduced to, say, one-half, the distance for which the figures are given must, of course, be similarly reduced.

In the above curves no account has been taken of the fact that the line current may at times of light load be reduced by some 30 per cent.; if this were done, of course the line efficiencies would be increased.

In short, each system must be decided on by taking into account the many variables in the problem, but it is hoped that these few notes will show what care is necessary in deciding on the proper line current;

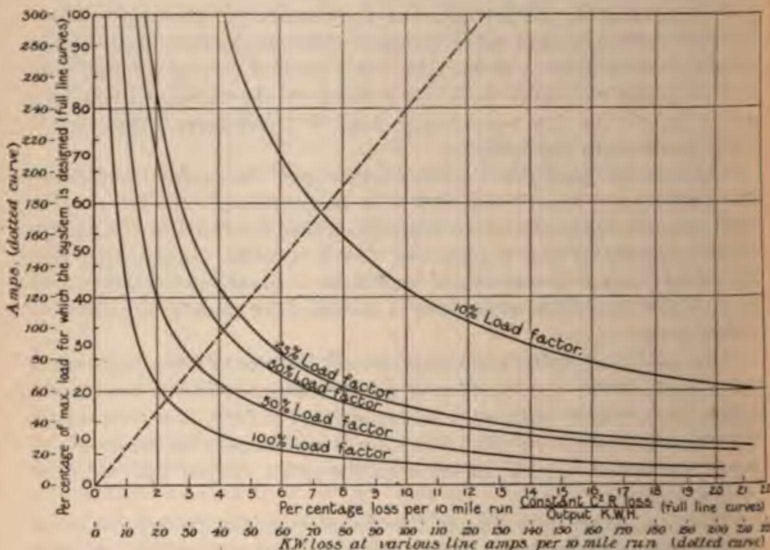


FIG. 7.—Curves showing Data for Conductors on the Thury System.

Working Pressure, 100,000 volts. Current Density, 500 amperes per square inch.

Full-line Curves showing the percentage the constant line loss in k.w.h. bears to the output at each load up to full load and at several load factors.

— — — — — $C^2 R$ loss at various line amperes.

in fact, it is a more important matter than any decision that has to be come to in laying out a parallel system.

TRANSMISSION SYSTEM.

The system can be worked either as an ordinary 2-wire system with both poles insulated, as a 2-wire system using earth as the return, or as a 3-wire system using the earth as the middle wire.

Where the pressure between the wires does not exceed 25,000 volts it is usually convenient to work with a 2-wire system, both poles being insulated, the line and other insulation of the system being sufficiently

good to stand easily the full pressure of 25,000 volts when one end of the series of generators is grounded. In this case, should an insulator break, or from any other cause one point on the system become connected to earth, no interruption will be caused. Further than this, the breakdown can be very conveniently repaired by temporarily grounding the whole system at two points, one on each side of the accidental earth. The damage may then be made good in perfect safety, the current flowing either through the earth or through the damaged conductor at the time. When all is in order, the two earths are removed and the circuit is worked as before. This method is regularly used in repairing the Thury lines in Switzerland. It should be remembered in this connection that the current dealt with is usually quite small, probably not exceeding 200 amperes, and that it is a constant direct current; therefore no trouble is caused on telephone and telegraph lines.

Another convenience of the 2-wire system is that when connections are made to the line or other work is required at any part of the system that point may be earthed.

In the 3-wire system the generators are divided into two approximately equal groups, and the middle point is connected to earth. This earth connection serves to limit the pressure at either side of the line to half the working pressure, so that with the same quality of insulation the working pressure may be doubled. Thus, if the system be started up as a 2-wire system, it is necessary only to connect to earth the middle point in order to enable the output to be doubled.

The system is not quite so safe as the 2-wire system, as in case of an earth in some part of the line, except where the earth divides the motors into two nearly equal groups, some part of the plant will be stopped, the worst condition being when the earth occurs near the power station, when half the supply will be stopped.

With the use of direct currents of comparatively small value, it might be possible, in open country districts, to use the earth as the return conductor. It would, of course, be necessary to make the earth connections in such a way that they were not rapidly destroyed by electrolysis. As compared with the 2-wire system, with the same total loss, one-quarter of the line copper would be required, since the resistance of the earth is negligible.

To fix ideas consider an actual case :—

Assume that power to the extent of 10,000 k.w. is to be transmitted over a distance of 100 miles, the current being 200 amperes and the pressure 50,000 volts, with two cables of 0·31 sq. in. section weighing approximately 1,188,000 lbs. the loss would be 10 per cent. Allowing for the same loss and using the ground for the return, the conductor could be reduced to 0·155 sq. in. section, the total weight of copper being 297,000 lbs. Using the earth as the neutral, thus enabling the pressure to be increased to 100,000 volts, the line current could be reduced to 100 amperes, and the line would consist of two conductors of 0·0775 sq. in. section weighing 297,000 lbs.

This system would cost more than the 2-wire earthed system; on

the other hand it would be safer, since failure at one point would not lay off the whole supply.

In order to ascertain what effect the continuous use of the earth as a conductor has on neighbouring telephones and other circuits, and what, if any, unexpected difficulties existed, experiments were carried out in the early part of 1902 on the St. Maurice-Lausanne transmission line, where, on several occasions, the whole of the supply has been transmitted at a pressure of 23,000 volts along one of the insulated transmission lines, using the lightning arrester earth connections as the return. The only difference observed as between using the double line and single line with earth return is that the loss is less in the latter case.

Further experiments were made at Laucey by a Commission formed by the French Government. These experiments were carried out at Laucey in conjunction with M. Thury, the line current being 110 amperes; they confirmed those made at St. Maurice, the loss being in proportion to the current density, the electrified zone extending only a very short distance into the ground. The drop in voltage was felt only in the immediate neighbourhood of the earthed plates. No disturbances were apparent either in the telephone or telegraph services.

In order to avoid any disturbance near the ground surface which might cause damage to local pipes or other metal work, the ground connection can conveniently be made by making a well of sufficient depth to get clear of any surface metal work and to get into a good conducting stratum. A mass of metal can be fixed at the bottom of the well, and connection can be made to it by means of an insulated cable. In this way, with the small currents used, in any ordinary formation, no disturbance would be caused anywhere near the surface.

Description of the Constant-Current Series System.

A description of an actual modern series system divides itself naturally into three heads, viz.: (1) The power station; (2) the line; and (3) the sub-station.

Since one of the generators must bear the maximum difference of potential between the line and earth, it would be impossible to insulate armature and field windings to carry the very high pressures required, and therefore a radical departure, really a reversion to older practice, is followed, and the whole frame of the generator is insulated from earth, and the armature is coupled to the prime mover by an insulating coupling; then, to secure safety to the attendants it is necessary also to insulate the whole engine-house floor, and to arrange that it is impossible to touch any part of the generators and make earth at the same time. These considerations involve difficulties which are overcome in the following manner: Fig. 8 shows a section of the engine-house with its insulating floor, consisting of a thick layer of asphalte concrete and a thin layer of pure asphalte. It is a simple matter to insulate for any

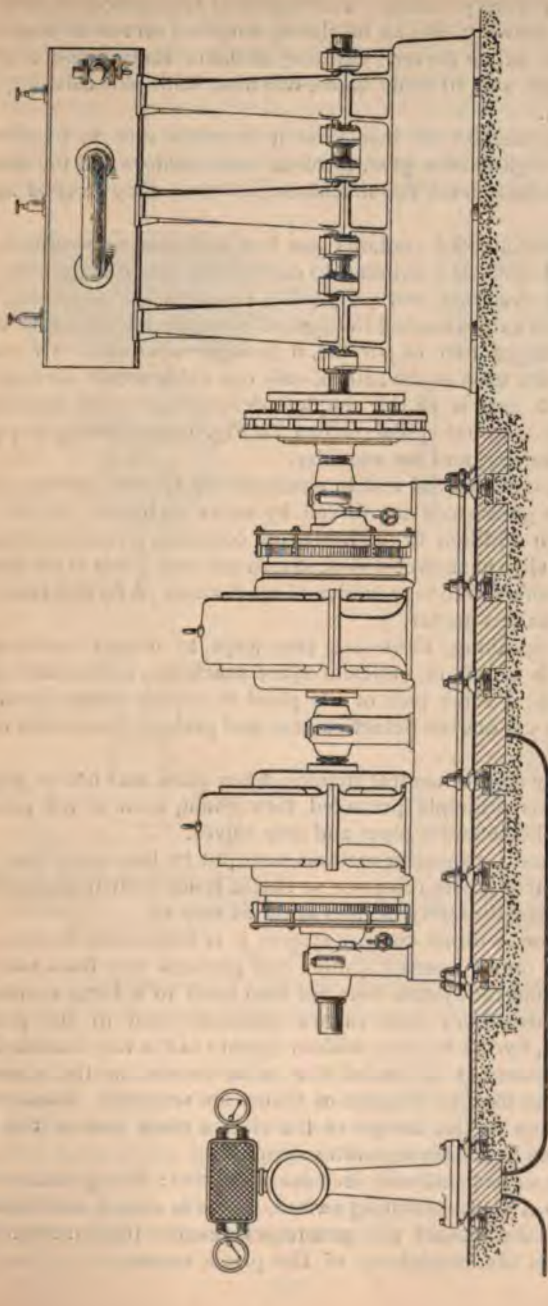


FIG. 8.—Cross-section showing Floor Construction and Plant for Direct-Current Series Generators.

practicable working pressure. The machines are carried on insulators shown in the drawing, and an insulating coupling serves to couple the machine to the prime mover. Ordinary Zodal or Raffard couplings are commonly used, and, in some cases, two discs with pins carrying hard rubber blocks.

With this method of insulation it is quite safe to handle the whole of the high-tension gear, to clean commutators and the like; in fact, the attendants treat the machines just as if they worked at low pressure.

The line switchboard contains one line ammeter, one voltmeter to show the total volts, and switches to cut out the line if required; also, of course, for overhead work, lightning arresters are required. The total number of cables leaving the station is usually two, but four might be used, arranged two in parallel, if thought advisable. Of course, if the earth were used as the return, only one cable would be required.

The switch gear is of the simplest description; each machine is controlled by a switch pillar, shown in Fig. 9, containing a 4-point switch, a voltmeter, and an ammeter.

It must be remembered that in nearly all the systems carried out by M. Thury the generators are driven by water turbines. In his later installations, in addition to the insulating coupling, a friction coupling is used which slips at an overload of, say, 20 per cent.; this saves damage to the generators due to very sudden short-circuits. A further reference will be made to this matter.

Generally speaking, there are two ways to design an electrical system—one is to put in sufficient spare machines, cables, and safety devices, so that if some part of the plant or cables breaks down the safety devices cut out the defective gear and prevent dislocation of the entire supply.

In the early days of central stations, when plant was not so good as it is to-day, this principle prevailed throughout, even to the point of providing duplicate steam pipes and stop valves.

The later and far superior method is to put in less spare gear and install the plant in a better manner, so that it is not likely to break down, and to dispense with safety devices as far as may be.

With the series direct-current system it is impossible to use more than one line out of a small station, and perhaps two lines out of a large station, and the system does not lend itself to a large number of automatic contrivances such as are generally used in the parallel system, which, by-the-by, very seldom operate in the way intended. It is therefore necessary to install the cable system in the strongest possible way, so that the chances of failure are removed. Similar care should be taken in the design of the station plant and of the little switch gear and accessory apparatus required.

The safety devices hitherto used are as follows: The generators are provided with a short-circuiting switch, which is closed mechanically and automatically should the generators reverse their direction of rotation due to the breakdown of the prime mover.

The slipping coupling already referred to protects the prime mover from the generator in the event of a very sudden short-circuit, and it appears to be a device of great utility. It should be remembered, however, that almost the whole of the existing series systems are driven by water power, which is by no means an ideal drive for a series generator. A series generator working at constant current requires a constant-torque motor to drive it, and is most efficiently regulated by varying its speed; the torque of a water turbine increases as the speed

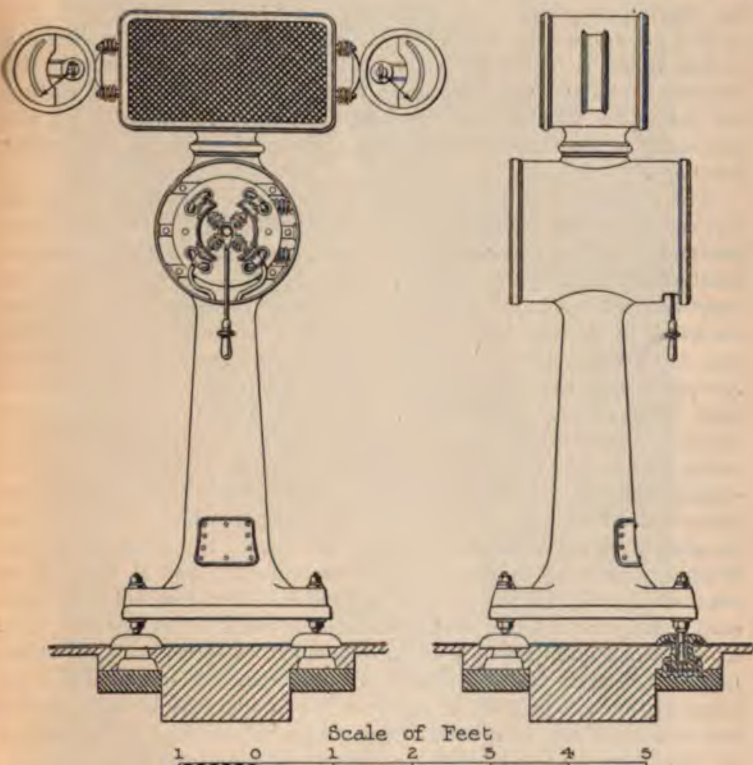


FIG. 9.—Generator Controlling Gear for 60,000 Volts.

is reduced, the maximum torque being about twice that at the most efficient speed. Also, with constant head, to maintain maximum efficiency the water turbine should run at constant speed, although in the existing series plants the regulation for convenience is carried out by varying the speed of the turbines. If, however, it were necessary to maintain maximum efficiency on varying loads, it is preferable to run the generators at constant speed and to regulate by varying the position of the brushes.

The number of series generators driven by steam is very small, the size of the units is small, and therefore there is not much experience on which to go, but since the torque of the steam engine with constant cut-off is constant, and does not increase in the same way as that of a water turbine, it follows that the necessity of the slipping coupling is not nearly so great in the case of the steam engine as with the water turbine. Its advantage would be that, in the case of a very violent short-circuit sufficient to stop the generator armature, the flywheel effect of the moving parts of the steam engine would not get further than the slipping coupling.

For the motors, the only safety device consists of a short-circuiting switch, which is closed by a solenoid when the pressure across the brushes exceeds a predetermined value. This provides for cutting out the motor should it become overloaded, or for maintaining the continuity of the line should some accident happen to the motor windings.

The regulation of the line current may be brought about either by varying the speed of the prime mover, the position of the brushes being constant, or, where it is convenient to keep the speed of the generators constant, by varying the position of the brushes. In either case the same type of regulator is used. It consists of a solenoid, carrying the whole of the line current which operates an armature in the usual way, as the line current makes small variations. The armature controls two pawls fixed to a rocking arm, which is kept in continuous movement by means of a small motor. Normally when the current is at its correct value both of these pawls are held out of engagement with a wheel, but should the current vary, one or other is lowered so as to engage with the notches in this wheel and drive it either forward or backward. The wheel is coupled to a shaft which controls the governors on the turbines or prime movers, or the position of the brushes, according to whether the machines are run at variable or constant speed. In the case of a 3-wire system two solenoids are used, connected at either side of the middle point of the system. The motor, which gives about ¼ H.P., is coupled in series with the main line, a resistance being coupled in parallel as the motor does not require the whole of the line current. In parallel with both the motor and resistance are two or three secondary cells, so that in the case of the failure of the line current the motor is still kept running.

The arrangement of the regulating gear is shown in the photograph, Fig. 10, and a photograph of the regulating gear in Fig. 11.

The same type of regulating gear is used for controlling the speed of the motors, but in this case, in place of the little motor the rocking arm and pawls are driven off the main motor shaft, which also serves to drive a small centrifugal governor which controls the position of the pawls in place of the solenoid referred to above. This device serves to rock the brushes by means of gearing. Should the speed rise, the governor operates in one direction so as to reduce the lead of the brushes; similarly when the speed falls the other pawl comes into

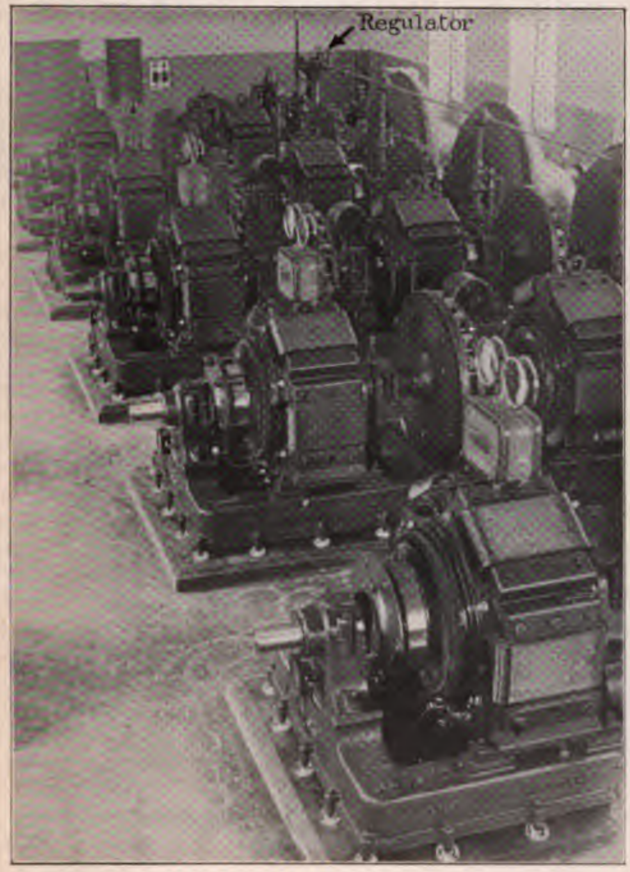


FIG. 10.

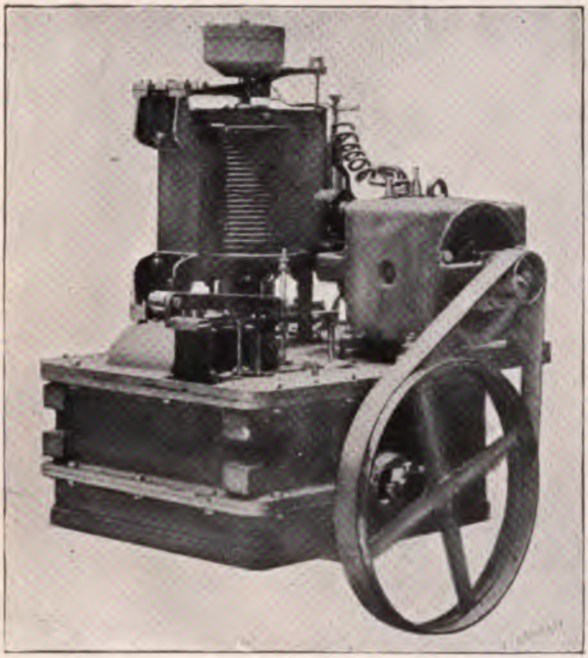


FIG. 11.—Regulating Gear.

action, serving to increase the lead of the brushes and to maintain the speed. The action of this device is exceedingly rapid; it is arranged to move the brushes throughout the whole range in 3 seconds. Dash-pots are used on these regulators to secure stability of action.

Fig. 12 shows a drawing of the switch column, Fig. 13 a photograph of the regulating gear, Fig. 14 being a photograph of two of the Lyon motors with the regulating gear in position.

In the earlier systems the line current given by constant-speed genera-

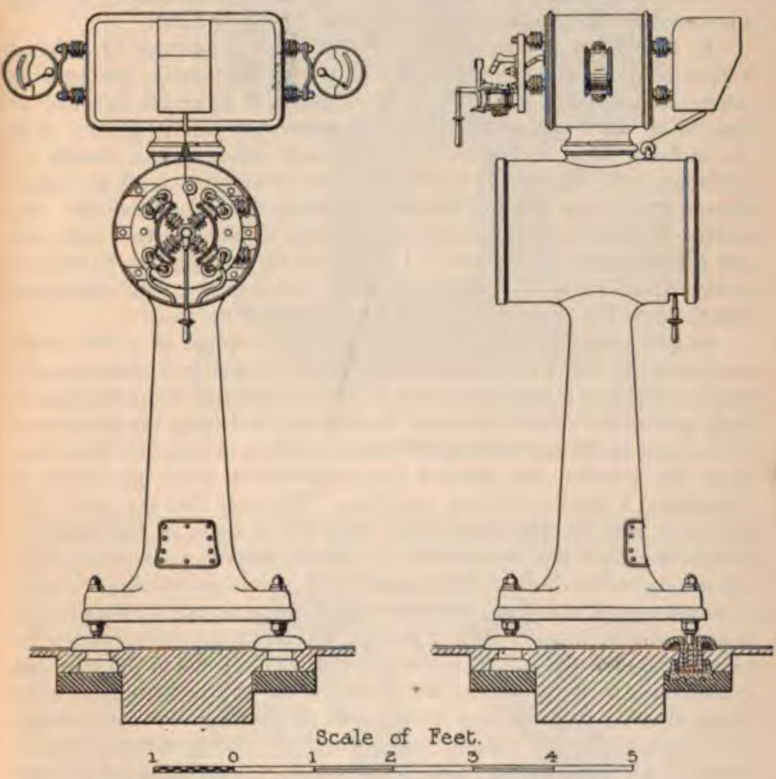


FIG. 12.—Motor Controlling Gear for 60,000 Volts.

tors and the speed of motors were controlled partly by varying the field and partly by rocking the brushes, the same regulator taking charge of both, but in all the later machines the whole of the regulation is done by rocking the brushes. The machines run quite sparklessly in all positions of the brushes. This is brought about by so designing the machines that the fields due to the armature and the field windings are equal, so that in the non-working position of the brushes they neutralise one another. I have seen many of M. Thury's machines under all

conditions of load; they run with carbon brushes with beautifully clean commutators—in fact, there are no commutator difficulties.

The machines at Chaux de Fonds, particulars of which installation are given in Table I., have been running for nearly eleven years with carbon brushes; they run continuously for a month without touching the commutators.

Prime Movers.—The power-station equipment varies according to the type of prime mover used, but wherever possible it is preferable, because it is more simple, to keep the line current constant by varying the speed of the generator, the brush position being fixed.

In laying out a transmission system for the purpose of driving rotary plant at the sub-stations it is always preferable, particularly where fluctuating loads have to be supplied, to place the flywheel as near to its work as possible. This is preferable in any system—it is particularly so in the series system. Each sub-generator should be provided with its own flywheel; this serves to protect all the plant behind it from the effect of sudden shocks on the secondary side. No further flywheels are required; in fact, they simply interfere with the self-regulation of the system. I believe it to be a mistake to lay out even a parallel system in the usual way—that is to say, with enormous flywheels on the generator and no flywheels near the load.

Reciprocating Steam Engines.—With the exception of a few small machines all the Thury plants are water driven, and consequently there is not very much experience to draw on for the steam driving of large generators; there would be no difficulty in driving the generators at constant speed and varying the brush position in order to keep constant the current, but since a constant-current series generator is essentially a constant-torque machine, whatever be the load, the governor may be dispensed with, and this is done in the cases to which reference has been made. A steam engine working at fixed cut-off, so as to admit a fixed quantity of steam per stroke, is also a constant-torque machine; consequently if the generator be coupled to a steam engine whose cut-off is fixed, it will run at the speed where the torques balance, depending in the one case on the value of the constant current and in the other on the point of cut-off, the steam pressure being steady; any tendency on the part of the engine to run too fast will increase the line current and so increase the generator torque; similarly a tendency to run slow will decrease the generator torque and so permit the engine to run faster. Consequently, when the generator terminals are short-circuited the engine will first crawl round, causing the generator to give the constant line current; an increase of resistance of the external circuit due to additional load will tend to decrease the line current, and with it the torque, and so enable the engine, working at constant torque, to speed up; consequently, as the load increases, due to extra resistance in the circuit, the speed will rise and with it the pressure; on the other hand, as the load decreases, due to the external resistance decreasing, the speed will fall. With several machines in series it might be thought that this system would be

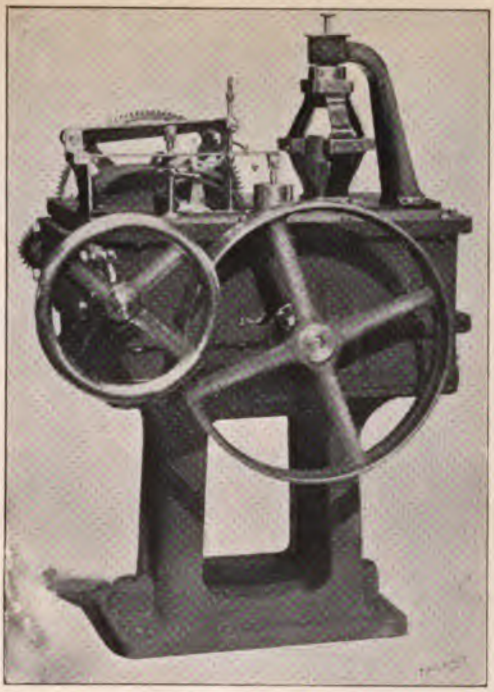


FIG. 13.—Motor Speed Regulator.

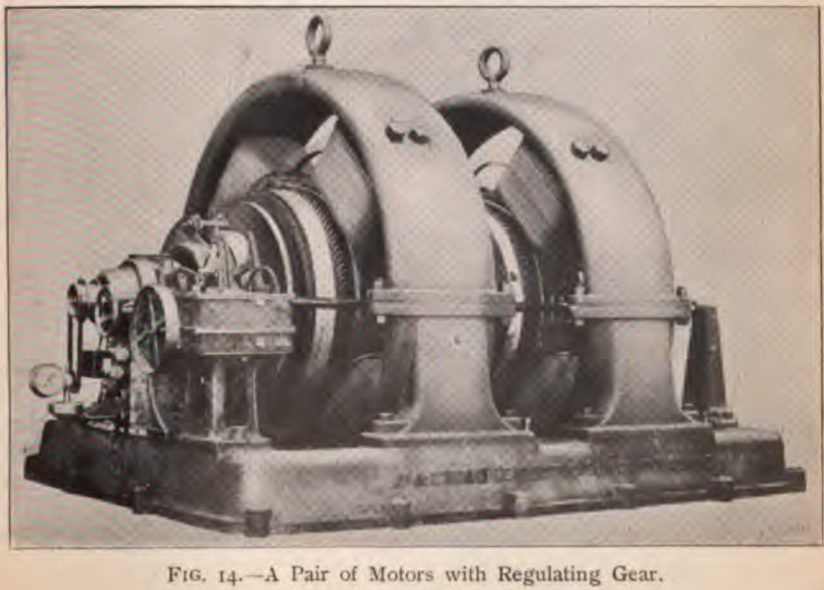


FIG. 14.—A Pair of Motors with Regulating Gear.

TABLE I.

Description of Undertaking.	Year of Starting up.	Line Current.	Total Length of Circuit.	Particulars of Machine Units.					Total Line Pressure.	Remarks.
				No.	Volts.	K.W.	Revs.	Total Output.		
		Amps.	Miles.					K.W.	Volts.	
Ste. Acquedotto de Ferrari–Galliers (Italy), Genes, 1889	1889	45	74·6	18	—	—	—	630	14,000	Ring.
Wasserwerke Zug (Switzerland) ...	1891	50	14·9	5	1,600	89	320	400	8,000	Ring.
Papeteries de Biberist (Switzerland) ...	1893	40	23·0	2	3,400	—	—	272	6,800	
Communes du Val de Travers (Switzerland)	1895	65	21·7	3 1	2,600 1,300	186 93	260 450	590	9,100	Ring.
Ste. d'Eclairage Electrique (Brescia–Italy), 1895	1895	50	32·3	3 2	1,500 3,000	—	—	525	10,500	
Ste. Romande d'Electricité (Switzerland)	1895	50	22·4	4	3,500	—	—	700	14,000	Ring.
Commune de la Chaux de Fonds et du Locle (Switzerland)	1896	150	32·3	7	1,800	288	300	1,890	12,600	Ring.
Usines Electriques d'Eisenbourg (Hungary), Ikervar–Steinamanger	1896	65	40·4	6	1,500	112	260	585	9,000	Ring.
La Papelera Espanola Renteria (Spain)	1896	65	17·4	3 2	2,600 2,740	186 194	—	865	13,280	
Ste. Industrielle d'Electricite Rieti (Italy)	1896	30	37·3	4	3,000	—	—	360	12,000	
M. V. J. Dunand à Batoum (Russia) ...	1899	50	12·4	2	1,300	—	—	130	2,600	
Usines Electriques d'Eisenbourg (Hungary), Ikervar–Sopron	1899	40	74·6	4	2,500	112	—	400	10,000	Ring.
Mines de Plomb Linares (Spain) ...	1900	60	37·3	3	3,500	238	320	630	10,500	
St. Maurice–Lausanne	1902	150	69·6	6*	2,250	373	300	4,000	27,000	Straight transmission 34·8 miles. The line is tapped twice before reaching Lausanne to supply local loads.
Moutiers–Lyon	1906	75	223·7	4*	7,200	582	300	4,300	57,600	Straight transmission of 112 miles consisting of 106 miles of overhead wire and 6 miles of underground cable, the underground cable being at the extremity of the line and working at the full pressure.

* Each unit consists of two generators.

unstable, and that one engine might take more than its share of a rising load, but the wire drawing due to the greater speed of the steam at high loads should prove sufficient to prevent any such tendency. When working in this way it is essential that only light flywheels be used.

Steam Turbine.—So far no high-tension direct-current generators have been constructed running at turbine speeds, and their design will probably be a difficult problem, but Mr. Hobart in the *Electrician*, vol. lvii. p. 424, has suggested that the problem is by no means impossible of solution. The turbine would be a constant-torque machine if the steam were admitted in a definite quantity once in a fixed number of revolutions, as in the Parsons turbines; the effect of running at blade speeds below the maximum would, however, be to reduce the turbine efficiency; on the other hand, by this method of running the leakage would be constant at all loads. I have no figures to show the comparative efficiency of turbines run at various percentages of load and at full speed, and at constant torque and variable speed.

Hydraulic Turbines.—Excepting the instances to which reference has been made, all the Thury plants are water driven, and since the water turbine is not a constant-torque machine, its use has caused more difficulties than would be met where steam driving is used. The necessity for the slipping coupling is probably greatly due to the great torque of a water turbine at low speeds. The pressure is regulated by varying the speed by a Thury regulator arranged to adjust the gate opening of all the turbines; the regulator works so as to keep constant current in the system. It is well known that should the proper peripheral speed be departed from the efficiency of a water turbine rapidly falls, consequently where falls are used having a great percentage variation of head at different times of year, to use the water economically the speed of the turbines should be varied to suit the variations in the head; this cannot be done with an alternate-current system, but with the series system it is the natural way of running. There are many cases where this possibility of suiting the speed of the turbines to the head would result in a greater output from a given fall than can be obtained at constant speed.

Gas Engines.—There is but one instance where a constant-current series generator is thus driven, and in this case it is set to run at full load, other machines in the series serving to regulate, but it would appear that with constant gas admission per stroke the gas engine would work between certain speed limits as a constant-torque machine, and so would run without a governor. Probably the limits of working would be not greater than from half to full speed, but this in most cases would be sufficient; in other cases it might be necessary to work at constant speed, with the usual brush rocking gear to maintain constant current in the line.

The Line.—For overhead working two single conductors are used, or if desired two pairs may be used, each pair forming in parallel one conductor. Unless separate poles are used it is probably safer to use

the two single lines and so to work with half the number of insulators. For underground work, two single cables have been used, and will probably prove the most secure and convenient arrangement, but this part of the problem is quite new, and many improvements may be possible. For instance, to give greater security two or three cables may be used in parallel working at such a density that should one break down the remaining one or two can carry the whole load, but modern cables properly laid are so very reliable that only for a very large system would the extra cost be warranted, more particularly as an earth at one point will cause but a partial stoppage of supply.

The Sub-station.—The motors are insulated from the ground and from the sub-generators, and the floor of the sub-station is insulated by the same means as have already been described; each motor is fitted with a regulator to control its speed by varying the brush position. In this case the regulator is controlled by a centrifugal governor so as to keep the speed constant. A switchboard is provided containing an ammeter, voltmeter, and short-circuiting switch to cut out the entire sub-station, and each motor has the usual switch pillar, which contains in addition to the short-circuiting main switch a device called a by-pass, which short-circuits the motor should the pressure between its terminals exceed a predetermined amount. The connections are arranged as in the power station.

Capital and Working Costs.

It is difficult to make any general comparison of the capital cost of different systems, as so much depends on the exact conditions to be met, but the tables show the order of differences to be expected. Table II. shows the cost of alternate-current stations using turbine machinery; for all but the smallest size the cost might be reduced somewhat by using larger sized units. The cost of buildings is taken at a low figure which could not be attained except under favourable conditions; the figures relate to the normal rating of the plant. Table III. shows the cost of series direct-current power stations, in all cases slow-speed reciprocating engines being employed. Tables IV., V., VI., and VII. set out the cost of sub-stations exclusive of buildings and all low-tension switch gear. Table VIII. shows the cost and other particulars for underground work; these results are also set out in Figs. 15 and 16 in the form of curves, but at different current densities. Table IX. is a summary of the other tables and shows the lengths of high-tension mains where the saving in the cost of the line work may make up for the extra cost of the direct-current power station. The tables have necessarily been largely prepared from estimates, but every care has been taken to make them as reliable a guide as possible; at the same time it must be apparent that they can give at best only a very rough idea of relative costs, and that each case must be considered on its own merits.

From these figures it may be assumed that where the power station is situated at or near the centre of a district whose boundaries extend

TABLE II.

Power Station—Alternating Current System.

Total Capacity ...	2,400 k.w.	14,000 k.w.	37,500 k.w.	110,000 k.w.
No. of Units and Size (Normal Rating)	6 ; 400 k.w. 10,000 volts	7 ; 2,000 k.w. 12,000 volts	10 ; 3,750 k.w. 15,000 volts	22 ; 5,000 k.w. 20,000 volts
1. Buildings, including Chimneys	£12,130	£38,695	£87,320	£248,325
2. Generating Plant	£21,690	£84,895	£200,765	£520,905
3. Switch Gear ...	£2,980	£5,760	£8,700	£18,440
4. Boilers, Auxiliary and Coal Handling Plants	£19,380	£68,780	£164,445	£498,370
Total Cost ...	£56,180	£201,130	£473,230	£1,318,040
Cost per k.w. ...	£23·4	£14·4	£12·6	£12·0

TABLE III.

Power Station—Thury System.

[redacted]

radially to, say, 8 miles, only in very exceptional circumstances is the series system superior to the parallel alternate system in cost or convenience.

With steam driving with present knowledge the series direct-current power station exceeds in cost the turbine-driven alternate-current power station, and therefore under such conditions the cost of the line is the deciding factor.

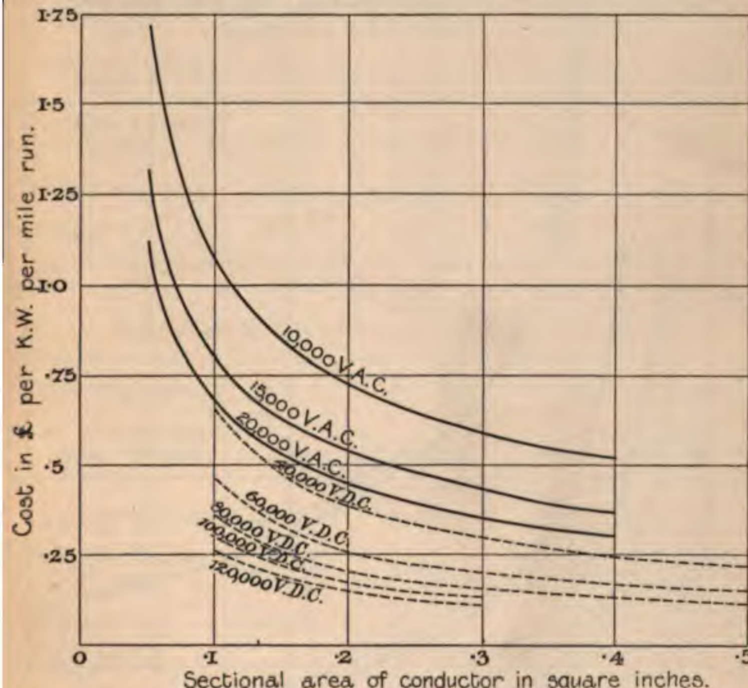


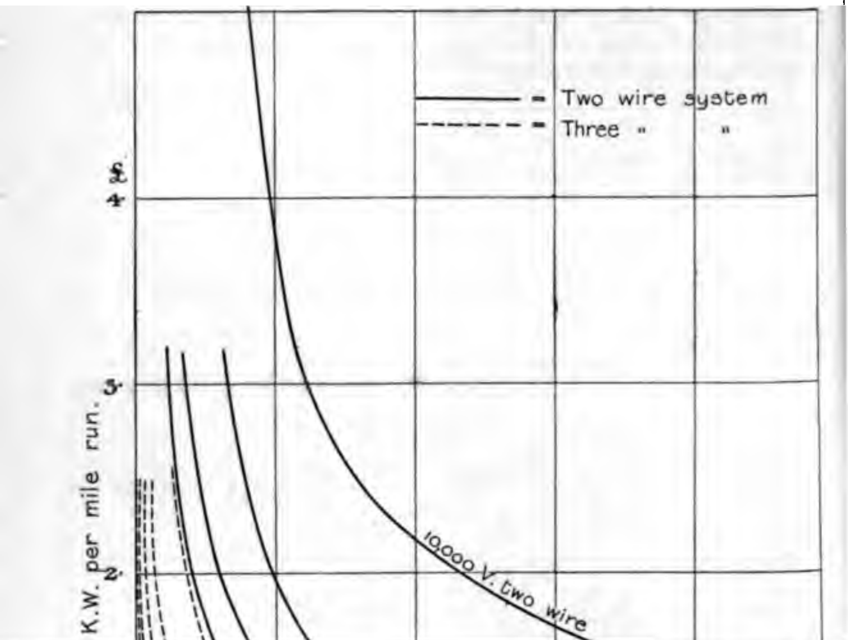
FIG. 15.—Relation between the Cost of Two 3-Core A.C. Cables and Two D.C. Single-Core Cables Working on a 3-Wire Thury System.

Density, 750 a sq. inch.
Cost of Copper taken at £87 10s. per ton.
Cost of Trenching and Stone Duct, £1,100 per mile.

The simplest class of transmission to consider is that where energy is transmitted from a single point to a far distant and concentrated load; in this case the tables show where the series system may be considered.

The second class of transmission is where a more or less circular district containing blocks of load at comparatively wide intervals is to be supplied from a power station either outside or on the circumference of the area; here it is not possible to generalise, and therefore it is convenient to study a special case.

Consider such an area as shown in Fig. 17, with a distribution of load and distances as shown on the diagram. The total load is 7,000 k.w.



[redacted]

TABLE IV.

Static Transformer Sub-station reducing from High Pressure to a Working Pressure of 500 Volts.

Total Capacity ...	200 k.w.	800 k.w.	2,000 k.w.	6,000 k.w.
No. of Units and Size (Normal Rating)	12 ; 17 k.v.a. S.P. Trans.	12 ; 70 k.v.a. S.P. Trans.	12 ; 170 k.v.a. S.P. Trans.	18 ; 335 k.v.a. S.P. Trans.
1. Converting Plant	£625	£1,870	£2,565	£9,670
2. High Tension S. Gear	£1,755	£2,295	£2,485	£3,525
Total Cost ...	£2,380	£4,165	£5,050	£13,195
Cost per k.w. ...	£11·9	£5·21	£2·52	£2·19

TABLE V.

Sub-station—Rotary Converter System, giving 500 Volts.

Total Capacity ...	200 k.w.	800 k.w.	2,000 k.w.	6,000 k.w.
No. of Units and Size (Normal Rating)	4 ; 50 k.w.	4 ; 200 k.w.	4 ; 500 k.w.	6 ; 1,000 k.w.
1. Converter Plant	£1,575	£3,980	£6,745	£19,925
2. High Tension S. Gear	£1,620	£2,360	£2,545	£3,580
Total Cost ...	£3,195	£6,340	£9,290	£23,505
Cost per k.w. ...	£15·97	£7·92	£4·64	£3·91

The cables are laid along roads, the solid lines showing the direct-current cable routes and the dotted line that of the alternate-current cables.

For alternate current two lines are taken from the power station, each consisting of two 3-phase cables, the conductors of 0·075 sq. in. area, the pressure being 20,000 volts; these are equal to a load of 7,200 k.w. at 750 amperes per sq. in., the C^2R loss at full load being 14·4 per cent. at the extreme end of the line. The cost is as follows:—

66 miles 0·05 cable at £900 per mile	£59,400
52 „ 0·075 „ £1,050 „	54,600
Trenching and duct, 85 miles at £1,000 per mile...	85,000
Total	£199,000

FIG. 17.—Diagram of Typical Bulk Supply Area, the Power Station being situated near the Circumference.

Alternate-Current Cables shown dotted lines.
Direct-Current Cables shown full lines.
Sub-stations shown thus ●

ALTERNATING-CURRENT CABLE SYSTEM AT 20,000 VOLTS.

Two cables of 0·075 sq. inch section from power station to sub-stations Nos. 1, 2, 3, 4, 14 and 15. One cable of 0·075 sq. inch section between Nos. 13 and 12 sub-stations. All other sub-stations have one 0·05 sq. inch cable.

Length of 0·075 sq. inch cable = 52 miles at £1,050 per mile	£54,600
" 0·050 " " = 66 " £900 "	59,400

Length of trenching = 85 miles at £1,000 per mile. (This includes price of conduit, trenching, and reinstating.)

Trenching	£85,000
Cable 0·05 sq. inch	59,400
Cable 0·075 sq. inch...	54,600
Total ...	£199,000

DIRECT-CURRENT CABLE SYSTEM AT 100,000 VOLTS.

One cable of 0·1 sq. inch section, length = 84 miles at £580 per mile ...	£48,720
Length of trenching = 84 miles at £900 per mile. (This includes price of conduit, trenching, and reinstating)	75,600
Total ...	£124,320

No allowance is made for power factor or capacity current or dielectric hysteresis losses. It will be seen that this is the cheapest possible scheme, the whole supply in the more distant centres depend-

TABLE VI.

Sub-station—A.C. Motors coupled to Direct-Current Generators giving 500 Volts.

Total Capacity ...	200 k.w.	800 k.w.	2,000 k.w.	6,000 k.w.
No. of Units and Size (Normal Rating)	50 k.w.	4 ; 200 k.w.	4 ; 500 k.w.	6 ; 1,000 k.w.
1. Converter Plant, Induction Motors, and Step-down Static Transformers	£2,730	£6,755	£11,735	£35,345
2. High Tension S. Gear	£1,620	£2,360	£2,545	£3,580
Total Cost ...	£4,350	£9,115	£14,280	£38,925
Cost per k.w. ...	£21·75	£11·39	£7·14	£6·49

TABLE VII.

Sub-station—Thury System (Thury Motors coupled to D.C. Generators giving 500 Volts).

Total Capacity ...	200 k.w.	800 k.w.	2,000 k.w.	6,000 k.w.
No. of Units and Size (Normal Rating)	4 ; 50 k.w.	4 ; 200 k.w.	4 ; 500 k.w.	6 ; 1,000 k.w.
1. Converter Plant	£2,635	£6,630	£11,580	£26,510
2. High Tension S. Gear	£560	£790	£1,195	£2,640
Total Cost ...	£3,195	£7,420	£12,775	£29,150
Cost per k.w. ...	£15·97	£9·28	£6·38	£4·85

ing on a single cable. The cost per kilowatt is £28·4, which is almost prohibitive and at once shows that a power station nearer the load centre is necessary. For direct current a single cable 0·1 sq. in. area

insulated for working at 50,000 volts to ground, is taken entirely the area; this cable could be worked at 800 amperes per sq. in., the cable a capacity of 8,000 k.w. when working at 100,000 vo 3-wire system. The cost is as follows:—

84 miles 0·1 sq. in. single cable at £580 ...	... £48,720
Trench and duct at £900	... 75,500
Total	£124,220

The complete costs of the two systems are approximate follows:—

	A.C.	Per K.W.	D.C.	Per
	£		£	
Power station of 7,000 k.w. (A.C. includes step-up transformers)	119,000	17·0	140,000	20
Sub-stations of 7,000 k.w.	70,000	10·0	70,000	10
Line 7,000 A.C. ... / 8,000 D.C. ...	199,000	28·4	124,220	17*
	£388,000	55·4	£334,220	47

* The cost of the series line at its full rating is £15·5 per kilowatt.

In the early stages of such an undertaking, since the cost of tren work and duct is so great a proportion of the whole cost of the mai the cost of the cable system would bear a greater proportion to whole, and if in the series system would show a still greater advanta and so enable the concern to start with perhaps 25 per cent. le capital—a matter of the utmost importance when the business is slow growing.

The cost of the alternate-current power station is based on turb generators with step-up transformers. The direct-current machin are driven by slow-speed engines. It is assumed that about thr quarters of the energy is converted through rotary machines.

The C^2R loss on the series system is at full load 3 per cent., an when the maximum load on the system is but 4,000 k.w. at 25 pe cent. load factor, the percentage the constant loss bears to the outpu is 24 per cent. This would compare with an overall loss in the main of, say, 10 per cent. on the parallel system; but, since the energy lo at full load costs about 3d. per unit as compared with, say, 0·3d. pe unit for the continuing loss, the actual money loss will be less on th series than on the parallel system.

As with alternate current, the series system is laid out at the leas possible cost, one cable serving each load centre; but the single cor

of Trenching,
16 15s. *per T*

Cost per K.W. Mile Laid.	
1,002 a Sq. Inch Density.	500 a Sq. Inch Density
2·04	4·08
1·09	2·19
0·57	1·14
0·39	0·79
0·31	0·62
0·28	0·57
0·20	0·39
0·15	0·31
0·13	0·26
0·11	0·23
0·37	0·74
0·20	0·40
0·14	0·28
	750 a Sq. Inch Density
0·54	0·72
0·40	0·54
0·33	0·45

TABLE IX.

Summarising the Cost per k.w. of Power Stations, Sub-stations of various types, and the Cost of Line Work underground, showing the limiting length of Line where the Cost of the Series System is equal to the Cost of the Parallel System under the stated Conditions.

	A.C.	D.C.	A.C.	D.C.	A.C.	D.C.	A.C.	D.C.
Capacity of System, k.w.	2,400	2,400	14,000	14,000	37,500	37,500	110,000	110,000
Working Pressure, volts	10,000	40,000	15,000	120,000	20,000	120,000	20,000	120,000
Cost of Cable System in £ per k.w.-mile laid	1·16	0·82	0·42	0·18	0·33	0·13	0·31	0·096
Cost of Power Station in £ per k.w. ...	23·4	25·05	14·4	19·25	12·6	15·86	12·0	15·5
Capacity of Sub-station, k.w.	200	200	800	800	2,000	2,000	6,000	6,000
Cost of Sub-station in £ per k.w. :—								
With Static Transformers... ...	11·9	—	5·21	—	2·52	—	2·19	—
With Rotary Converters	15·97	—	7·92	—	4·64	—	3·91	—
With Motor-Generators	21·75	15·97	11·39	9·28	7·14	6·38	6·49	4·85
Total Cost of Power Station and Sub-station, in £ per k.w. :—								
With Static Transformers... ...	35·3	—	19·6	—	15·1	—	14·2	—
With Rotary Converters	39·4	—	22·3	—	17·2	—	15·9	—
With Motor-Generators	45·1	41·0	25·8	28·5	19·7	22·2	18·5	20·3
Length of Cable run in miles, where the Cost of the Systems is equal :—								
With Static Transformers... ...	16·7	16·7	37·1	37·1	35·5	35·5	28·5	28·5
With Rotary Converters	4·7	4·7	25·8	25·8	25·0	25·0	20·6	20·6
With Motor-Generators	—	—	11·3	11·3	12·5	12·5	8·4	8·4
Particulars of Conductors :—	sq. in.	sq. in.	sq. in.	sq. in.	sq. in.	sq. in.	sq. in.	sq. in.
Number and Size of Cables	2, 0·1	2, 0·1	2, 0·4	2, 0·15	4, 0·4	4, 0·2	12, 0·4	8, 0·3
Current Density per sq. inch	775	660	750	780	750	780	735	765
Percentage C^2R loss per mile, at full load, alternating current with 90 per cent. power factor ...	0·59	0·13	0·38	0·057	0·28	0·057	0·27	0·056

cable is a better mechanical job than the 3-core, and is therefore less liable to accident; also the insulation at the stations is far less vulnerable, and the number of points where breakdowns generally occur, as in the switch gear, are far less in number than with the alternate-current system; and, further, the chances of electrical breakdowns due to sudden rises of pressure are much less with the direct-current system; in addition, as has been shown, earthing at one point does not upset the entire supply.

The demand may develop along two lines of route following, say, two diverging main roads, and it may at first be advisable to complete the entire ring. In this case, with the direct-current system, two single lines may be laid picking up the loads along the two roads; each line may be earthed at its end and the circuit completed through earth until the increasing demand at a greater distance calls for the completion of the ring with copper.

A further consideration in the problem of supplying this district is of interest: Assume the supply to be started with a line current and power station suitable to a fixed maximum load, and that at a subsequent date it be found that the load has exceeded expectations, it is then a simple matter to erect a second power station at any point on the line which approximately equally divides the load on each side; the load that can be carried can thus be doubled without laying any new mains, and the two stations can be run in series without the smallest difficulty. This method has been used in the Chaux de Fonds-Locle system, which was started with one small water power station, and as the load increased two other power stations were erected, all three running in series on the same circuit. In fact, one immense advantage is that the difficulties of running in parallel are entirely avoided, so that several power stations may be connected to the same ring as the total load increases, thus allowing the effective working pressure to be raised far above any figure possible with any other system; in short, in this respect the uniform section of main renders the series system far more flexible than any parallel system.

The above district may be taken as typical of many of the so-called bulk supply areas in Great Britain. In these, in general, at many of the local centres there are already in existence small stations supplying on diverse systems, some direct current and some alternate at various frequencies. It is usually necessary in the early days of the undertaking to give only a partial supply to each centre, the local steam plant handling the remainder; therefore, in most cases, it is necessary to give the bulk supply through motor-generators. It is always a convenience and often a necessity to run the motor-driven generators in parallel with the local steam plant. Where the local supply is direct current there is no difficulty, but where the local supply is alternate current it is always difficult, and under certain circumstances it is impossible. With the series direct system, on the other hand, there is no difficulty in giving any class of supply nor in running in parallel with any local plant.

The costs of working each system will be very similar, though it may be claimed that very large turbines would work more economically than very large reciprocating engines, but the convenience of working the series system is far greater than that of working the parallel system; the switch gear is immensely more simple, and the convenience of starting both motors and generators is far in favour of series direct-current working.

Special Applications.—There are some requirements to which the series direct-current system is especially adapted.

The first of these is when it is essential to run the sub-generators at speeds varying over wide limits for long periods. This is a very common condition for generators for electro-chemical work. There are many processes where it is necessary to maintain the current practically constant and to vary the pressure over considerable limits. Other cases are where the pressure and current are varied, and where the machines are called upon to run for considerable periods at reduced speed.

With alternate-current transmission it is commercially impossible to vary continuously the speed of the sub-generators over the wide limits necessary, whereas the direct-current system allows for any sort of variation to be made in the speed of the sub-generators with but small reduction in efficiency.

A second class of work where the series system is very convenient is for operating hoists and similar loads where the speed varies over wide limits, and where starting and stopping are matters of frequent occurrence, and where a high degree of acceleration is requisite.

It is of course necessary in such cases to provide that the generators are governed by a quick acting governor, so as to maintain the line current constant with sudden variations in load—that is, the ordinary difficulties met with in dealing with such loads require the usual special attention.

The whole operation of the motors is carried out by the brush movement, a rapid screw arrangement being the most convenient. A switch should be provided to short-circuit the motor when it is stopped for any length of time, so as to save loss of energy and heating in the motor winding. Braking is done, of course, entirely by the brush movement, the motors being reversed and run as generators. The torque of the motors is constant, the speed being varied, as explained by the brush movement.

In efficiency of working, ease of operation, and smoothness of starting and braking, the series system should in many such cases show better results than any other system; and where much of the supply may conveniently be handled by a 3-phase or other parallel system, it may often be convenient and profitable to work hoists, winding gear, and similar loads by means of a separate series system.

Concluding, I set out some of the possible directions in which the series system offers advantages over the parallel system:—

1. The ability to extend the possible commercial transmission distance far beyond that possible with alternate currents, and particularly in those cases where underground transmission is essential.
2. Simplification of switch and regulating gear.
3. Easy working of several stations in series on the same loads, so that the more efficient run always, and the less efficient run only, on the peak load.
4. The uniform section of the mains permits an increasing load supplied at first from one point to be readily provided for by the addition at any other suitable points on the main of other stations without addition to the cable system.
5. Efficient speed regulation of sub-generators where certain special loads, such as chemical loads, are to be served from a distance.
6. Greater all-round efficiency to be obtained when the generators are driven by turbines worked from a waterfall having a great percentage variation of head.

I have endeavoured to show that M. Thury has carried the series direct-current system to a point far beyond the experimental stage, and that in recent years he has made great advances in the direction of the improvement of the generators and the motors and in the use of very high pressures, and has shown that far higher pressures may be attained. At the same time it must be realised that the size of the machines hitherto built is comparatively small, but experience shows that there would be no difficulty in building machines of greater size, and there is no reason why any special difficulties should be met with in working much larger series systems than those now running.

6

DISCUSSION.

Lord Kelvin.

Lord KELVIN: I have listened with extreme interest to Mr. Highfield's paper; it is really one of the best scientific papers I have ever heard read. It has been so clear on every point that every one present has been thoroughly carried along with the author.

For myself the subject is one of extreme interest. I have never swerved from the opinion that the right system for long-distance transmission of power by electricity is the direct-current system. I do not say a word in depreciation of the beauty of the polyphase idea, and of the development of that idea, especially in the 3-phase working. The great convenience of the 3-phase system is well known to all; the ease with which, by transformers with no moving part, an augmentation can be made from, let us say, 10,000 volts in the generator to 20,000, 30,000, or 40,000 volts in the transmission line is very valuable and important. In fact, it is, I believe, the great convenience of the static transformer that has led to the wonderful and splendid development of the alternating-current system in modern electrical engineering.

Lord Kelvin.

We have now come to a position in electrical engineering, looked forward to twenty or thirty years ago, in which we are practically confronted with the problem of the transmission of electric power through very great distances. After what we have heard from Mr. Highfield, I have no suggestion to make in respect to the advantages that he has demonstrated of the direct-current system.

Some of the most subtle problems of electricity, problems depending even on resonance, are more or less familiar to all electrical engineers at the present time. The perfect freedom of the direct-current system from complications of that kind is very remarkable.

You all understand something about square root 2 being 1·41. Starting with that scientific fact, we have this practical application that 141 volts of direct current have no more tendency to produce sparks than 100 volts of alternating current. That, I think, is a statement that is generally understood. The 141 to 100 we have all known as the merit in respect to voltage attainable by direct current, as compared with voltage attainable by alternating current; but now we hear from Mr. Highfield that the advantage is far more than the ratio 141 to 100. Mr. Highfield will correct me, but my recollection is that he said it is more like 2¼ to 1 than 1½ to 1. The properties of matter concerned in the breaking down of insulation, and the gradual heating up by the alternating current till the insulation breaks, determine a result which to me was unexpected. Mr. Highfield finds, in respect to insulation, a much greater advantage of the direct current over the alternating current than has been hitherto known to us. I think I may safely say, as proved by the experiments which he has put before us, that 80,000 volts direct current are more easily worked than 40,000 volts alternating current.

That is enough to prove the case for direct current, unless there are difficulties in the practical management of direct current, as compared with alternating current. But the difficulties are all the other way; the practical difficulties are in the management of alternating current; the ease (except in respect to the brushes) is all for the direct current. So that, not only have we much greater capacities for bearing high pressure, but very much greater ease and convenience in practical working, when we adopt the direct current for long-distance transmission.

I do not go so far as to say that the beautiful systems that are now worked out for distributing power over somewhat long distances would be better carried out by direct current than by alternating current. I do think, however, with regard to the ordinary power-supply stations, which are now with but few exceptions founded on the alternating-current system, it is just possible that, in future, there may be a change to direct current. The penetrating skill and perseverance of Mr. Thury having led the way, we may, with the very clear ideas that have been put before us by Mr. Highfield, begin to see that, even in the ordinary distribution of power by electricity at 20,000 or 30,000 volts, the advantages of the direct current will bring it to pass sooner or later, that it may supersede the alternating current.

Lord Kelvin.

I am reminded of a little prophetic conversational statement made by Lord Rayleigh a good many years ago. He rejoiced to see the use of alternating current coming in, "Because," he said, "the whole world will now learn the subtleties of electrical science, which they had no chance of learning before." That prophecy has been fulfilled. "And," he added, "after that, they will come back to the continuous current." I do not know that you will all agree with that anticipation, but you will, I am sure, all enthusiastically agree with Lord Rayleigh in rejoicing that we have had this twenty years of alternating current, and of electrical science in its most interesting characters, including in fact wireless telegraphy, put before beginners in electrical science, as they now are in practical schools of electricity.

Dr. Kapp.

Dr. G. KAPP: We have every reason to thank Mr. Highfield for having given us a paper which may appropriately be called a pioneering paper, and I would couple with that expression of gratitude the hope that Mr. Highfield's pioneering labour may bear fruit in this country. There is some reason for expressing such a hope because in one instance at least previous experience was the other way. If you think of the time of which Lord Kelvin has just spoken, when alternating currents were first used for power and light, the pioneering scientific labour was done in England, but the practical application was reaped abroad. At that time the English manufacturer was busy with continuous-current plant and showed very little inclination to take up a system which many regarded as new-fangled and useless. I would like to add a word of warning: do not consider the high-pressure continuous current a new-fangled or a useless system. I wish manufacturers of dynamo machines would devote their energies at least to preparing themselves for what is likely to come, the use of continuous-current machines giving very high pressure. If we begin to work at the subject now we shall be ready when the time comes of which Lord Kelvin spoke. We do not desire, I am sure, to repeat the history connected with polyphase currents, when we had a lag of five years behind the Continent and America, with the result that we see in England at the present time so many Continental and American machines at work. Those machines could have been built here if we had taken in time to this system. For this reason particularly I think we ought to be grateful to Mr. Highfield for having brought before us this important method of working long-distance transmission. He has thrown out certain hints for improvements, which I admit are quite possible in the system which has been developed by M. Thury. That system, admirable as it is, is not, of course, perfect; it is one man's work, and a matter of this kind to be perfect must be the result of the labour of many men, and I hope that some of the younger engineers will help in carrying that work forward. I may say that I have been a convert to this system only for the last three or four years. I resisted conversion as long as I could. I have had trouble enough with the subtleties of the alternating current, and I did not want to burden my mind with something new. But when I had, in the course of my work,

Dr. Kapp.

to go and inspect some of the Thury stations, and when I saw how simple, safe, and reliable the working was, I could not help being converted, and it is as a convert that I now speak.

There are, of course, certain difficulties connected with this system. One is the question of commutation when there is a great difference of potential between neighbouring commutator bars and a large current to commutate. We heard of a machine which M. Thury has built where there were 500 volts between the bars. That is an experimental machine giving 1 ampere. There was no difficulty; but build a machine where a current of several hundred amperes has to be commutated and it will be found a difficult problem. M. Thury has found a solution, but only up to a certain limit of output. He has developed a certain type of machine which within that limit is fairly satisfactory, but which from the dynamo builder's point of view is not perfect. I do not want to make a reproach of that. The machine works well and serves its purpose, but for larger units the design will have to be altered to get perfect commutation. This should be possible by using Messrs. Parsons & Stoney's admirable method of compensating for armature reaction, and creating a commutating field in air. These gentlemen have applied this method to their steam turbine-driven dynamos with perfect results. There is not the faintest sparking at the brushes, even with quickly varying loads. The difficulty in the commutation of the direct-current machines at high pressure may thus be considered as solved. Another difficulty is the insulation of the machine from the motor which drives it. At present M. Thury uses either a Zodel coupling or, for higher pressures, a special kind of rubber coupling. Instead of the Zodel leather band there are indiarubber rings which press against each other. The distance between charged and earthed parts is thus twice the thickness of one ring when compressed, or, say, 6 ins. at most. This is not enough for 70,000 volts or more. There must be a long distance between the points of great difference of potential. For this reason the insulating coupling as now made appeared to me unreliable for very high voltage. I was at that time connected, as advisor, with a company which had a scheme for carrying electricity over 800 miles of veldt, and before going any further I wanted to see whether the difficulty of the coupling could be solved. I designed a coupling in which no charged point should be nearer than about 18 ins. from earthed parts. That, of course, required the use of drag links made of insulating material, and the coupling became very large. One could not build a whole coupling to test it, but I arranged an apparatus for testing the links under mechanical and electrical stress. An illustration of the apparatus is shown in the accompanying Fig. A. The links were suspended under great stress, and at the same time a pressure of 70,000 volts continuous current was put on and kept on for hours. I was not present at the test, but Dr. Tissot, of Bale, who watched the test, sent me a photograph and the particulars. These links stood perfectly well. They were of ordinary rubber and canvas belting without any special preparations

Dr. Kapp. to improve their insulation. The insulation resistance whilst under mechanical and electrical stress was 118 megohms. That works out at a loss of only 42 watts, so that practically it may be said the difficulty of the insulating coupling is solved. We can get insulating material which will stand a great mechanical stress and at the same time stand 70,000 volts and probably more. Another difficulty was with the insulators for the line. I was afraid that, with a very long line, the loss by leakage would be considerable, so I had tests made of some insulators, and it may interest the meeting to know what is lost through insulators. We had an artificial rain of 1 to 2 millimetres per

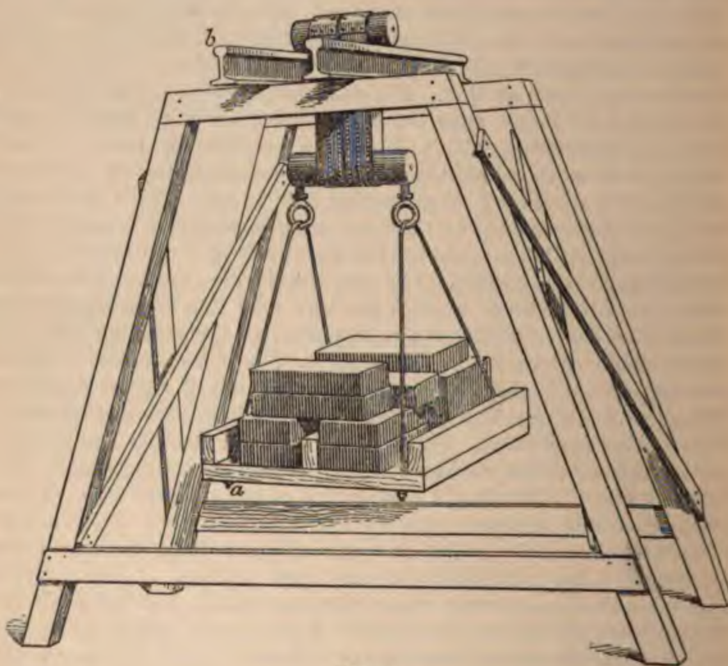


FIG. A.

minute, and at 50,000 volts alternating current each insulator lost $5\frac{3}{4}$ watts. There are about 80 insulators in a mile of 3-phase line so that with 50,000 volts to earth the loss would be 450 watts per mile. But at 70,000 to earth the loss in one insulator is nearly double, namely, 10·5 watts, so that a line of 500 miles would lose 400 k.w. I do not think we should have this great loss with continuous current. I believe the large loss is due to the fact pointed out by Mr. Highfield that the ripples or surgings superimposed on the fundamental harmonic reach much higher than the pressure which our voltmeter shows. The latter is the square root of mean squares, but the insulator feels the ripples

which act, so to speak, as pioneers, making a path for the first harmonic to follow. With continuous current this effect cannot take place, and the loss should therefore be much reduced. As regards the question of cost, I must admire the great pains Mr. Highfield has taken in working out his tables, and I think they will be very useful in future when one has to make preliminary estimates. Meanwhile, it may interest the meeting to hear some figures as originally worked out in very minute detail for a transmission plant in Switzerland, which is to bring about 12,000 k.w. to Zurich from the River Albula. The distance is not very great, only 85 miles. Estimates were made for two systems, one the ordinary 3-phase and the other the direct-current system. As regards the choice of pressure, it is interesting to note that the figures which Lord Kelvin has just mentioned are almost exactly the figures which the engineers of the Zurich transmission scheme decided on after careful investigation. Lord Kelvin said 80,000 and 40,000 volts, and as a matter of fact the design for the transmission to Zurich was based on direct current of 80,000 volts and 3-phase current of 46,000 volts. That is at the generating end. At the delivery end in Zurich it would be 40,000 volts 3-phase, and 68,000 direct current. In both cases the generating station is 15,000 k.w. The delivery was a little more with 3-phase current, namely, 11,800 k.w. delivered in Zurich under a pressure of 40,000 volts, whilst with the continuous current transmission only 10,500 k.w. would be delivered, but also in the shape of 3-phase current at 40,000 volts. The necessity of conversion at the delivery end is the cause why with the direct-current system the efficiency is not so great. This brings me to a point which Mr. Highfield has mentioned, namely, that efficiency should be considered financially. Translated into money value, the continuous-current system comes out better. The total cost of the continuous-current system was £220,000, and of the alternating-current £247,000. That means that the alternating system is slightly dearer, but it must be remembered that that system delivers at 40,000 volts into an existing ring main. It is an accident that that ring main exists, but it is very convenient for the alternating current. If sub-stations had to be put up, as is necessary for the continuous current, then the difference would be greater. In putting both systems on to the same basis, in this one particular case we find that the prime cost is £23 per k.w. delivered for the 3-phase and £21 for the direct-current system. The difference in total cost is not very great, but the difference in the cost of the line is very great. In the alternating-current 3-phase system the line cost £156,000, whereas the direct-current line only cost £78,500. This is with a comparatively short line. For greater distances the influence of the line on total cost would be greater, and the continuous-current system must come out considerably cheaper. Even on moderately long lines when cables have to be used the difference in total cost will be considerably in favour of the continuous-current system. Dr. Kapp.

Mr. C. P. Sparks: I have had my attention riveted on alternating-current work for the last twenty years, and in this country, Mr. Sparks.

Mr. Sparks. with the absence of water power and the comparatively short distance of transmission, the alternating current has served, and is likely to serve us, for a long time. But this country of ours is only a small unit in a great Empire, and in looking round the Empire I feel that the series direct-current system brought to our notice by Mr. Highfield is of great importance. Where we are dealing with water power, long distances, and a fixed amount of energy for transmission, I have no doubt of the advantages of this system. At the moment, without discussing that point, there are one or two matters that I should like to refer to in the paper, amongst the many interesting particulars that are brought to our notice by the author. I do this to elicit information, and not to criticise a paper that has been most ably put before us. First, with regard to the comparison of direct and alternating current. I have no experience of very high-pressure direct currents; but looking back to the early days when we operated with 3,000 volts, or sometimes with 6,000, with series arc-lighting machines, one remembers that, although with overhead wires the difficulties were not great, that with underground conductors the direct current proved at that time a source of great difficulty. Comparing lower pressures again, we have the fact that direct-current mains laid underground have generally proved a source of greater difficulty than alternating-current mains at the same pressure. That is due to the electrolytic action of direct currents, and to the fact that the negative conductor attracts moisture and tends to break down. Personally, I think the comparison, instead of being in favour of the direct-current system, is in favour of the alternating current with underground cables, and in place of being two and a quarter times in favour of the direct current, it is very much the same ratio in favour of alternating. In this I am speaking with an experience of comparatively small pressures; I have no experience of the big pressures that have been put before us to-night; but put the lines overhead, and then I am with the author; I think the advantages there are great. The conditions of working are so entirely different from underground, that I have no doubt the figure he gives in favour of the direct current is in the proportion mentioned. Secondly, I notice in the paper (page 475) an allusion to a short length of underground cable which has been laid for use with high-pressure direct current. It would be interesting to have particulars of the insulating material, and the working pressure of that particular cable to earth, which depends on the position of this cable relatively to the load and the earth connection. Another point I should like to refer to is the question of alternating distribution. The series system is new to me; I have not thought about it for over twenty years. There is a comparison between alternating- and continuous-current distribution in the diagram (Fig. 17). If I was starting from the point (power station) set out, I should not lay out the alternating system in the way proposed. The alternating-current system shown has 118 miles of conductor, and there is only a single supply to each sub-station. Alternatively one might connect sub-station (4) on the one side to

No. 10 on the other, and thus have two rings giving at least a duplicated supply to each sub-station. There would be 132 miles in this case, against 118 proposed. But the extra 14 miles give a duplicated supply to each alternating-current sub-station, which is of great advantage as compared with the single supply on the direct-current system. Then as to the method of carrying out the work, the alternating-current system can be developed gradually just as on the direct. One can begin with two legs, and when they have advanced a certain distance they can be looped, and if this is done they can be extended gradually over the whole area, finally duplicating the first ring. No doubt this is only a diagrammatic case put before us, but on looking at the demand in that area, shown at 7,000 k.w., it appears to me that, with a station situated at the edge of the district, the only economic method of supply is by an aërial line. If an aërial line is impossible, then the station should be in a more central position. With regard to the comparative losses in the two systems, in the case of the direct current, the low cost of the units lost spread over the whole year, and the high cost of the units lost on the peak with the alternating system is pointed out on page 498. Without criticising the figures given by the author of 0·3d. per unit in one case and 3·0d. in the other, I see he takes in the case of the direct current "the percentage the constant loss bears to the output is 24 per cent."; and then says, "This would compare with an overall loss in the mains of, say, 10 per cent. on the parallel system." In the parallel system the greatest loss is at full load at the furthest point, and the average loss on the system is less. In making out a balance-sheet for a year, it appears to me that the direct-current losses at time of full load in this particular case would be between 5 and 6 per cent., while average alternating-current loss at this load would be less, while the actual units lost during the year would be so much greater in the direct-current case than in the alternating current that, taking the units at the price given, it shows with 4,000-k.w. demand and 25 per cent. load factor that the alternating current is the cheaper one of the two so far as losses are concerned. The last point I should like to make is this: The author claims on page 502, "The ability to extend the possible commercial transmission distance far beyond that possible with alternate currents." With that I thoroughly agree; and then he adds, "And particularly in those cases where underground transmission is essential." With that I totally disagree. Mr. Sparks.

Mr. THOMAS HESKETH: There are two or three points in this paper upon which I would like to make a few remarks. Lord Kelvin has touched lightly on the first of them. The point I refer to is the question of the insulation resistance: the relative values of alternating as against continuous current as bearing on the insulation resistance of the cables. Lord Kelvin suggested that, viewed only from a scientific or laboratory experimental point of view of the spark-gaps, the continuous current had a very decided advantage over the alternating current. The second speaker agreed with the point that the author Mr. Hesketh.

Mr. Hesketh.

raised: that the advantages in favour of the continuous current were some two and a half times that of the alternating current. The next speaker said that, on account of the power the negative cable has of attracting moisture, and also from electrolytic reasons, the continuous-current cable was at a decided disadvantage as compared with the alternating current. They are all three, to my mind, correct, as far as they have gone, but they have not gone far enough. Nearly the whole of the trouble that we are accustomed to in practical working is not so much caused by the breakdown of dielectrics due to applied electromotive force, but is caused by phenomenal electromotive force when surges or resonance effects are set up. In my own station I am running a continuous current set at a voltage, you may call it, of 3,000—2,980 we have actually worked at—and the current generated is sent on one cable two miles, and on another cable four miles. We have, like many alternating-current stations, had breakdowns varying in degree, but I am satisfied, from the results of those breakdowns, that it was not the applied electromotive force which caused the trouble. Resonance effects undoubtedly were set up, although the lines were, one would imagine, the most suitable for continuous-current working. In one case the electrostatic voltmeter, one of Lord Kelvin's instruments, calibrated up to 3,000 volts, and which would stand in the laboratory at least 6,000 to 10,000 volts, sparked across. It could not have sparked across with anything like the normal working current. Again, the brush-holders of the particular machines I am referring to are extremely well insulated; mica and very large ebonite caps being made use of, but the spark jumped a distance of at least 4 ins. over these. This was not a spark of an ordinary 3,000 continuous-current voltage, although it might possibly have been dragged out of its track by the influence of the fields. But a more conclusive proof that a resonance effect is rather to be looked to as the cause of breakdown, than the applied electromotive force, is given by the perforations which take place in the armature windings every time an interruption of this class occurs. If one can follow in one's mind from the cables, through the main switches, up from the holders down the commutator, up to the inductive windings on the armature I think it will be agreed that the first real impedance for a current surge comes in where the conductor leaves the commutator segments and passes between the iron of the armature and the field; it is always at this particular point—the right-angle point where the wire leaves the commutator and runs along the armature—that the spark breaks through the insulation, not from one conductor to another, but apparently into space, sometimes leaving a puncture as large as the end of a lead pencil, at other times leaving a multiplicity of small pinholes, which I look upon as being much more serious in their effects. The reason I say this is, that when the larger perforations are left, one sees them and takes precautions to insulate them again before putting the machine into action. These pinholes, in the case I am referring to, occur not when a serious accident takes place, but

Mr. Hesketh.

when a heavy short-circuit—of, say, 150 k.w.—occurs. At other times the quick reduction of current flow will cause the evil, as, for example, when a circuit is opened on full load.

In one case, owing to negligence on the part of the switchboard attendant, the wooden blocks specially provided to put the automatic cutouts out of action were omitted to be placed in position, and the result was the main switches opened when a current of 120 amperes was flowing. We then got probably the biggest surge that we have ever had, and with regard to the machine, apart from the armature being destroyed, the commutator was burned all the way round, whilst, in addition to the pinholes I have previously referred to, the mains broke down at two places where sharp right-angle bends existed. When a serious perforation takes place it is bad enough, but what I strongly object to on the part of these resonance effects is that they make small perforations, because one is able to run the machines up again, and it is only after a lapse of considerable time that the dirt and dust works into the perforation and one is worse off than one was before. From this point of view the advent of the Thury system is to be regarded with the greatest of all pleasure, because with a theoretically light machine it is impossible, I take it, to get oscillations due to wide variations of current. If there is no reserve of energy stored up in the flywheel, the engine under theoretical conditions would automatically maintain a constant current, and not send a large current productive of surges. This, to my mind, is one of the very greatest advantages in the Thury system, and far outweighs the disadvantage of electrolytic or osmotic action, or the capacity the negative cable has for attracting moisture. I believe, as regards underground cables, if those cables always remained in the condition in which they were sent out by the makers the moisture would not be drawn into the negative cable, and I feel sure that surges which are never recognised by engineers cause minute microscopic perforations, and these permit of the moisture being drawn in. Apart from osmotic effects or punctures, there is a strange breathing action taking place in all large conductors when variable currents are sent through them. Imagine the case of a large conductor running at a light load in certain parts of the twenty-four hours, and heavily loaded later on; the conductor heats up to an appreciable extent, and expels any air that may be locked in the interstices. On cooling down the opposite effects take place; and if there are appreciable, although only microscopic perforations, moisture would be drawn in which would readily lend itself to electrolytic action, and the end of the cable would be assured.

I have dwelt at some length with the question of surges, for I have felt for some time that English engineers, owing to the restricted areas of supply, have not had the opportunities American and Continental engineers have had for practical illustrations of the effects they create, and in case it may be assumed that the effects I have drawn attention to are particularly associated with continuous-current working, I would

M . H -sketh.

briefly draw the members' attention to that admirable paper on surges by Steinmetz, and if, as I have endeavoured to prove, the greater portion of breakdowns are occasioned by resonance or other capacity effects, then it is clear, in addition to the advantages so ably put forward by Mr. Highfield, that the Thury system brings with it a great probability of eliminating these less usually recognised evils.

The author has raised the question of the difficulty of building continuous-current machines with voltages high enough to carry out to the full effect the Thury system, and he has advocated the use of a pair of machines to double the present working voltage. A suggestion was made to me by a firm of dynamo builders some time ago, which I cannot help thinking was based upon the supposition that commutation of 3,000 volts was difficult. It was to the effect that they were prepared to build a 300-k.w. machine to work at 3,000 volts continuous current, provided they were allowed to use a double-wound armature and two commutators, the brushes of these commutators being coupled in series, and so halving the E.M.F.'s between the segments on the commutators. Now, as a matter of fact, there is absolutely no difficulty in commutating 3,000 volts, but the provision of twin commutators and double-wound armatures would excellently overcome the difficulties Mr. Highfield has mentioned on page 479 in the way of doubling the line current in the event of the growth of the station warranting such change.

Discussion at Meeting of March 14, 1907.

Mr. Patchell.

Mr. W. H. Patchell: The subject of direct-current transmission is one which has always had a particular interest for me. Twenty years ago I was engaged as works manager of the Electrical Power Storage Company with the late Mr. Frank King, engineer of the Company, in working out a patent series system of charging batteries with the discharge in parallel. The experience we then had in trying to build up several hundred volts by putting Edison, Siemens, Schuckert, and sundry other machines in series was very entertaining. We also experimented with a Thomson-Houston and a Statter constant-current machine for the same purpose. We got the system to such a stage that we found we could promote a company, and in 1888 a Provisional Order was obtained for the area which is now supplied by the Chelsea Electric Supply Company. As the high-tension steam dynamos for charging the batteries in series at 500 volts would be doing nothing at the time of the peak load, we then had to develop a rotary converter. We had considerable opposition in getting the machine from the Electric Construction Corporation, who considered we should never be able to make such an arrangment work, and had in the meantime bought the E.P.S. works. We solved the difficulty by building an armature at Millwall for a Wolverhampton frame, and having proved that such a machine was feasible, Chelsea was equipped with them. A further development of the scheme was the "Oxford system" with the long range control. Walsall and Hull

Mr. Patchell.

were also equipped on the system. Some years later I went to the Charing Cross Company, and when I had to develop the station at Lambeth I carried out the extensions on 1,000 volts continuous current, and I suppose for many years the Lambeth 4,000-k.w. plant was the largest of that type running. That brings me to the opening page of the paper, where the author says that the use of direct current gets over the difficulties to which we are more accustomed with alternating current. That is not my experience. Direct current does not cause breakdowns and surges so frequently as alternating current, but when a breakdown with continuous current does happen, one then begins to realise that something special is going on! Although it is not distinctly stated in the paper, I gather that in M. Thury's machines his fields are excited with the full line current, and they work as series machines. I did not care to put 1,000 volts on shunt fields, so I had them all separately excited at 200 volts. At first there was no interlocking gear, and the apparatus was not fool-proof. When a man pulled the wrong switch, we got surges and jumps far larger than could possibly have been due to 1,000 volts. We had not Mr. Duddell's oscillograph and the other means we have now of telling what the pressure really was, and all we could do was to put up shields and increase the distances between live parts. If we had had the insulating floors round our machines and switchboards which M. Thury now works with, and had not earthed the switchboard frames, we should have been saved a good many breakdowns. I should be afraid to say how many inches that "1,000 volts" jumped; sometimes with a faulty machine it was a matter of a couple of feet. I think, therefore, we must take the statement on the first page that recourse to this system will eliminate all those difficulties, with a little reservation for the present. A great deal of the trouble that we experienced was no doubt due to the large power behind the spark. On page 473 the author says that M. Thury's machine was working at 1 ampere. It is possible to do anything with 1 ampere, but with big machines on a short-circuit, and with untold current flowing, the interest begins. The initial breakdown is not the worst part of the trouble; it is the arc which follows that does the damage; so if the current is limited, the breakdown may be a very tame affair! All of us would wish we had such a system that we need not mind whether a machine broke down or not, and could leave it in circuit and let the current run through it, as it is stated on page 477 M. Thury does. That is most convenient; and we who have been responsible for parallel stations would devoutly wish we had machines of that sort! On page 482 the author refers to a rather interesting novelty—the point of making an earth connection for a system down in a well. In mining work we are doing that constantly; but as to the possibility of doing it in town, I am reminded of what happened some years ago on the south side of London. The engineer of a supply company was blamed for charging up a man's railings. He did not see that he could be charging those railings, although his mains were in the street. He went round and found that

Mr. Patchell.

he could get a kick on the galvanometer that he took with him off the railings, and that proved to him it was not his own current because he had alternating supply! It then appeared that the current was coming from the bowels of the earth, from a tube which was running at a considerable depth beneath, upon which there was no station anywhere near. I think as we cannot quite rely on the current which is discharged down at the bottom of a well coming up exactly where we want it, that sort of earthing will have to be done very cautiously.

On the question of earthing, the system seems to me admirably adapted for a set of sub-stations under one control, either direct or indirect control. Last week when reading his paper, the author dilated on the flexibility of the system, and told of the case of the man who could take out his threshing machine and connect up the motor with the overhead transmission wires and earth the return without interfering with the supply. But he did not say what would happen to the rest of the circuit if another man went out at the same time with another threshing machine on the opposite side of the ring!

The author's remarks on flywheels I do not quite follow. On page 488, when discussing prime movers, he says, "I believe it to be a mistake to lay out even a parallel system in the usual way—that is to say, with enormous flywheels on the generator and no flywheels near the load." Those of us who are used to working with reciprocating engines rely on the flywheel for getting even turning, and especially with an alternating plant that is absolutely important. With the series system it is not so important, but I fail to see how an engine could get over its centres comfortably without a certain amount of flywheel on it. As to the flywheel near the load, that has been done years ago, and has been done again lately on the Ilgner system for getting steady turning for winding motors. A 25-ton or even larger flywheel on a motor-generator is placed close up to the winder (it does not matter how far away from the source of power), and then with a steady load on the power station all the time, an intermittent huge amount of power is obtained at the winder. The author remarks that we get a constant torque on reciprocating steam engines, and then a little further on in the paper he says that we get the same advantage, constant torque, with a gas engine. Surely the most ardent advocate of the gas engine would hardly dare to claim for it a constant torque, because I think it has probably the most uneven turning moment of any prime mover. If the author means something by constant torque which we do not quite understand as such, I hope he will clear it up. Certainly one trouble, with running gas engines nowadays is the anything but constant torque that they give.

On the last page the author sketches out the adaptability of this particular system to mining work. I am referring particularly to his second class of work on page 501, where he claims that the series system is very convenient for operating hoists and similar loads, and a little further down he evidently refers again to winding machines. I should think if there is anything which this series system at

present is not well adapted to, it is a winding engine, unless with the Ilgner system, and then it does not matter a straw whether one adopts a 3-phase, single-phase, or the series system: they are all equally adapted. I should be very glad if, in his reply, the author will clear up a few of these points, because really this is a most interesting description of a novel system. Mr. Patchell.

Mr. FRANK BAILEY: It is somewhat difficult to criticise a paper of this kind in the light of any direct experience of such a novel method. I can only express my own admiration at the marvellous way in which M. Thury has overcome the novel difficulties, and reached such a success with apparently so little trouble. It is quite true, from the figures given in Table I., that the scheme as carried out in Switzerland dates back a good many years, and apparently at first sight it is a little difficult to explain why a method which has proved so admirable there has not yet been copied, or at any rate carried out to the same extent in other countries. If I may suggest a reason, it would be that we have been somewhat blinded by the extraordinary success of the admirable points of the 3-phase and other multi-phase transmission. While it is to be regretted, on the one hand, that it has not enabled us perhaps to appreciate the good points of the other system, on the other hand, I must admit that it has been an educational training for all concerned. We have learned a good deal from alternating-current work; we have learned what to avoid; we have learned how to overcome difficulties; and last, but not least, the investing public has learned how to lose its money. We have all heard of power schemes in this country. Very few of them have been quite so successful as we would like; and how far those power schemes would have succeeded had such a system as this been put before them remains to be seen. I cannot speak from experience of such high pressures as the designer of this system is using, but it seems to me a remarkable thing that one man unaided, with no previous history or experience to guide him, should by himself have carried such a marvellous scheme to a successful issue. In our own experience—and I am quite sure many here must have had more experience than I have had of 3-phase and other transmission—we have seen what troubles there are. It would be wrong to suggest that the 3-phase is the perfect or ideal system. It is quite true we have overcome difficulties. But because we have got over difficulties, it is no reason why we should neglect to consider the admirable claims of a system of this kind. We have seen what it is to put alternators in parallel and to run two steam stations in parallel—I know to my cost. We know the extraordinary effects we can get—impedance, inductance, and surges; but, at any rate, in a continuous-current system we have a flexibility of system and an elasticity of control which only those who have had continuous-current work before them can fully appreciate. Many years of my life were spent in trying to overcome alternating-current difficulties, and it is only within the last seven or eight years I have realised what absolute peace was, and that was after changing the system from alternating current to con- Mr. Bailey.

Mr. Bailey. tinuous current. We shall, no doubt, be told that in Switzerland this system has been a success, partly because the transmission is by overhead conductors. If it is possible to work the system overhead in such a manner as M. Thury has done in Switzerland, why not do it underground in this country? We have all had trouble with cables, and we have heard of terrible things which have happened—we have called it quite a new name, osmosis. Before we knew that word we never dreamed of those terrible things that would happen to cables. If a cable broke down, we found the fault and probably condemned the cable. I have taken out many miles of defective cable, mostly india-rubber, and replaced it by paper cables. For many years past I have been running many miles of paper cable with 3,000 volts continuous current, and we have not had a single fault. If osmosis exists, I have not seen it. If cables break down, no doubt moisture gets in, but the system must not be condemned for that; it is because the cable is bad. If cable makers and dynamo builders are ready and willing to undertake the responsibility connected with the new system, which has reached a successful conclusion, why should we throw any doubt on the possibility of introducing it? I noticed with a good deal of gratification that, in the tables the author has given comparing the capital cost of the two systems, he has not erred by any means on the side of exaggeration. To my mind the figures he has given are quite reasonable, and certainly warrant our best study.

Mr. Jenkin. Mr. B. M. JENKIN: It seems to me that one of the most important things in this paper is the question of the use of the 3-wire system, and I should like the author to explain that matter further. Having had experience of the troubles of 3-phase current, one naturally wants to find out what the troubles are in this direct-current system, and therefore if I suggest certain troubles, it is only to enable the author to explain that they do not exist rather than make too much of them. He says on page 481 that, "In the 3-wire system the generators are divided into two approximately equal groups, and the middle point is connected to earth." I understand him to mean by that, that he connects the circuit to earth at the power house, and that there is no other earth on the system outside. That, at first sight, would appear to be all right. He says further down, "The system is not quite so safe as the 2-wire system, as in the case of an earth in some part of the line, except where the earth divides the motors into two nearly equal groups, some part of the plant will be stopped, the worst condition being when the earth occurs near the power station, when half the supply will be stopped." I have tried to work that out, and the diagrams will show my difficulty. Quite likely it is entirely wrong, but I cannot quite understand what happens.

Fig. B indicates, diagrammatically, by the full line the potential drop over the circuit at full load with the system earthed between the two generators at the power house. On the occurrence of a fault at the point indicated, the potential diagram is altered to the dotted line for generator A, and to the dot-and-dash line for generator B. About

three-quarters of the load is thrown on A, which is more than it can deal with, and the current drops in consequence. The drop in volts along this section of the circuit is also reduced. The remaining part of the load is taken by generator B, which continues to work with full Mr. Jenkin.

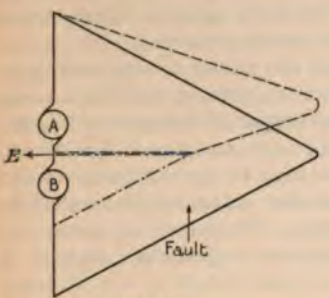


Fig. B.

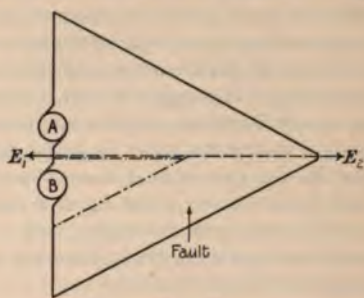


Fig. C.

current, but its voltage is reduced to correspond to the reduction of load. Only one-quarter of the circuit is working properly.

Fig. C shows a similar arrangement, but with a permanent earth half-way along the external circuit, as well as an earth between the generators at the power house. This is a true 3-wire system, and on the occurrence of a fault, as before, generator A continues to work with its full current, and maintains the load on its side of the system. Generator B takes a quarter of the load, maintaining full current, but at reduced volts. The part of the circuit between E2 and the fault is cut out, the current passing by earth from E2 to the fault. Three-quarters of the circuit is working properly.

Fig. D shows a similar system, but with the middle point of the

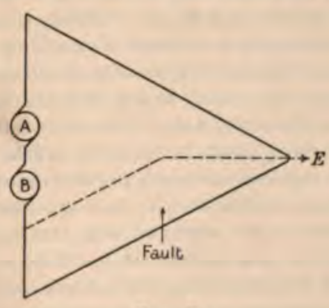


Fig. D.

external circuit permanently earthed only, there being no earth at the power house. In this case, on the occurrence of a fault, the same results as in Fig. C are obtained on the external circuit, but the two generators share the load equally, the volts being reduced on each to

Mr. Jenkin. the same extent. Three-quarters of the circuit are working properly. Of the three arrangements, this on the whole appears to be the best.

The above description and the diagrams are, of course, only an approximate indication of what occurs with a fault at the particular point chosen. The matter is further complicated if we consider the effect of putting a second power house on to the circuit. What would happen then as to earthing the system I cannot quite see.

The next question I would like to ask the author is with regard to governing. He suggests that, with steam engines, governors are not necessary, because one can let them work against the generators, the current doing the governing and the engine running as fast as it can. But in the case that I have shown in Fig. B, the top half of the load becomes too great for the engine; the current is decreased in consequence, and the engine will run away. Therefore we must certainly have an emergency governor to hold the engine from racing. I imagine that was intended.

The question of the C^2R losses I feel is very important indeed, and if the author could say anything further than he has done upon that point I think it would reassure us. At the foot of page 498 he says that "The C^2R loss on the series system is at full load 3 per cent." He then speaks about what it would be on a certain load factor, and further down he says, "This would compare with an overall loss in the mains of, say, 10 per cent. on the parallel system." Does that 3 per cent. and the 10 per cent. mean the loss in the mains at full load, simply kilowatt loss? (Mr. Highfield: Yes.) If that is so, it seems to me that it is hardly fair to charge the constant 3 per cent. loss (the losses due to the direct current) at 0·3d., and to charge the losses in the alternating-current system at 3d. After all, the series system losses occur in the mains during the peak load, and perhaps that proportion of these losses ought at any rate to be charged at 3d.; but I do not see why, when they occur at the peak in the parallel system, they should be charged at 3d. Further than that, we have in the generating station usually a number of auxiliary motors, and so on, and I do not think it is customary to charge all the current to them during peak load at 3d. It seems to me that the losses in the mains should be charged at the same rate. The only difference apparently being that somewhat more plant is required to take up the loss in the mains at the peak of the load with the parallel system than is required with the series system. In the former case we have a loss of 10 per cent. and in the latter 3 per cent., so that there is, say, 7 per cent. difference. The cost of that additional plant may be charged to the parallel system, but I hope the author will make that clearer.

The last point I want to mention is one which strikes me as of immense importance in the Thury system, and that is the possibility of using it for coupling alternating systems together. Mr. Highfield refers to that at the foot of page 500, where he says, "Where the local supply is direct current there is no difficulty, but where the local supply is alternate current it is always difficult, and under certain circumstances

it is impossible. With the series system, on the other hand, there is no difficulty in giving any class of supply, nor in running in parallel with any local plant." By the Thury system a flexibility is obtained between the two power houses which cannot possibly be got by any arrangement with alternating current. If one has an alternating-current feeder between two power houses, and it is attempted to couple it on to the two alternating-current systems, those two stations are coupled rigidly together by that cable and its motor-generators, and I do not think one would dare to do it; Mr. Highfield says it is impossible. But with the Thury system the flexibility of the series constant-current machines, enables it to be done with absolute safety, as one cannot overload the motor-generator on the series side. The motor slows up and will not, I understand, take more power than it ought to. Mr. Jenkin.

Mr. J. J. FASOLA: The paper is especially interesting to me because I have had occasion to see the system working, and during the several years I was in Zurich this question was a topic of much discussion. I refer to the Albula-Zurich scheme mentioned by Professor Kapp, which involves the transmission of 16,300 k.w. over 85 miles between two points of concentrated load. Perhaps it may be of interest if I give the final results arrived at in this money scheme, where, after a prolonged study of all details influencing the question, the less favourable alternatives were eliminated point by point, and finally the two great rival systems were compared at their best respective advantages. Professor Kapp has given figures showing estimated cost of the scheme as originally worked out, but the figures I now propose giving are very carefully compiled from last September's tenders, and were submitted to the Town Council of Zurich in the form of an exhaustive comparative report by Mr. Wagner, Director of the Zurich Electricity Works. For the transmission line they adopted in the 3-phase parallel system a voltage of 45,100, dropping to 40,000 through four sets of three conductors, each 8 mm. in diameter, spaced 0·8 metre apart, and compared this with the direct-current series system, whereby a constant current of 170 amperes at 96,000 volts would be transmitted through two pairs of conductors 6·7 mm. in diameter. In each case the line loss was taken at 11 per cent., and two entirely separate transmission lines were provided, with iron or concrete posts 80 metres apart in the direct-current case, and 60 metres apart in the 3-phase case, the lowest wire hanging not less than 8 metres above the ground level. In the 3-phase case five section isolating and arrester houses were provided for each line, interconnected in pairs; whereas in the direct-current case six houses, common to both lines, were considered sufficient. The existing yearly load diagram for about 8,000 k.w. was available to base the probable future load diagram upon in calculating the 3-phase line losses. The cost of 3-phase transmission line worked out about £74,500 higher (accounted for by 590 tons more copper, 1,100 more posts, 675 more miles of copper, and 20,200 more insulators to erect, and four more section stations), but the losses in the direct-current case came out to 17,300,000 — 2,400,000 = 14,900,000 k.w.-hours per year higher, Mr. Fasola.

Mr. Fasola. which, even taken at as low a figure as $\frac{1}{10}$d. per unit, and capitalised at 7 per cent., represents a money value of £85,000, thus leaving a balance in favour of 3-phase transmission of £10,500 for the transmission line. The power-station comparison was made between eight 3,000-H.P. 7,000-volt 3-phase hydraulic units, running at 600 revolutions, connected direct, without intermediate switchgear, to 3-phase transformers with extra high tension secondary switchgear; and, on the other hand, eight 3,000-H.P. hydraulic units, each coupled to two double commutator series dynamos, giving twice 3,000 volts each at 375 revolutions, together with simple switchgear and the necessary insulated building. The direct-current series generating plant also worked out in this case the more expensive by £8,000 (accounted for by an extra expense on the 3-phase side of £3,520, £1,520, £8,400, and £2,640 in connection with the buildings, turbo-exciters, transformers, and switchgear respectively, and an extra expense on the direct-current side of £2,320, £21,300, and £460 in connection with turbines with their automatic regulators and insulated couplings, dynamos, and turbo-generator for local supply respectively).

The transforming sub-stations for transforming down to 6,000 volts 3-phase, and the direct-current emergency storage plant, likewise worked out about £30,000 more expensive in the direct-current series system. The existing load consisted of a 5,200-k.w. single-phase lighting load, supplied from 2,000-volt cables, and distributed on the 3-wire system at 2 × 105 volts, a 1,500-k.w. 3-phase power load supplied at 6,000 volts and distributed at 500 volts, and a 1,700-k.w. direct-current traction load supplied from 3-phase motor-generator sub-stations at 550 volts. About 3,500-k.w. were being taken from a distant supply company's circuit. Altogether a total balance of £48,500 was obtained in favour of the 3-phase parallel system, although this figure was subsequently reduced to £30,000 by a special inquiry commission, who submitted revised figures on the basis of a 5 per cent. line loss in the series system, which was proved to be the figure for highest economy with that system. Any advantage as regards higher security in the transmission line of the direct-current system was considered outbalanced by lower security in the stations. Administration expenses, wages, and maintenance were considered as about balanced in the two systems. The period of highest demand coinciding with the season of water scarcity (December to February), constant line and machine losses, which absorbed more than one-third of the daily water supply, were practically inadmissible. Further, in the 3-phase case a very considerable part of the capital outlay might be postponed until the power demand called for it, or a favourable market opportunity occurred. Everything taken into account, therefore, the result of the comparison was in favour of the 3-phase system. This result does not necessarily conflict with the figures given by Mr. Highfield, as the 3-phase system appears to be at a disadvantage, in long transmissions at any rate, at the comparatively low voltages of 10,000 to 20,000 volts taken in the paper in connection with cables. Further, I think the

fact that the peak of the load will only be likely to occur for two or three hours justifies a comparison between normally rated constant-current machines and 3-phase machines normally rated to give 15 to 25 per cent. lower output, so that they could still give the overload during the peak period. Mr. Fasola.

I am informed that the cost of the turbines, buildings, and 3-phase line will actually be made up as follows: £43,080 for the electric equipment of the generating station, £19,200 for the hydraulic equipment of the generating station, £13,120 for buildings, £72,000 for the transmission line, making altogether £147,400 without the sub-stations. The cost of the sub-stations has not yet been definitely fixed, but the figure would be of secondary importance, as it is influenced by the special local requirements at the distributing centre. When the Town Council of Zurich introduced a Bill for raising £429,400 in connection with this scheme last June, the price of the sub-stations for the direct-current series system was estimated at £61,473.

Thury automatic gears are the best I know of their kind, but the results of a possible failure may be very disastrous. Constant maximum demand C^2R losses in the line and the machinery windings are decidedly a great objection. The seriousness of a possible interruption of current tends to necessitate large spare generating plant at the distributing centres on long transmission lines. The series system appears to be at a disadvantage where transmission between other than two or three large concentrated load centres is concerned. Increase of the voltage, especially with large amounts of power, is attended by a costly increase of the number of units. Water-power generating plant may run at a favourable speed for series dynamos, but the manufacturers are still experiencing difficulties with the design of direct-current steam turbo-generators at low voltages, and the prospects of success in the direction of large high-voltage direct-current turbo-dynamos seem very remote. The application of the series system to direct individual power supply does not seem to promise much on account of the insulating dangers and the comparatively high cost of machinery for the same maximum torque where high overload torques with constant speed gear are involved. It is not an easy matter to lay out economically a series system for an unknown future demand, especially when the voltage has to be near the upper limit to start with. The necessity for the immediate full capital outlay in long transmission lines is in some cases a serious drawback. If to this are added the prospective troubles arising from commutators, and, where cables are concerned, from electrolysis and osmosis, it may be concluded that only a most exhaustive comparison of the two rival systems under the particular conditions of each individual case can decide whether the many apparent merits of the direct-current series system really justify its adoption.

Mr. H. M. HOBART: In Figs. 2 and 3 there are some curves of sparking distances, with 50-$\sim$ alternating current on the one hand and direct current on the other. If for the alternating-current tests curve *a*, which gives the sparking distance between two spheres Mr. Hobart.

Mr. Hobart. of 20 mm. diameter, is examined, it will be found that 40,000 volts corresponds to a sparking distance of 30 mm. On curve *c* for direct current it will be noticed that 59,000 volts bridge that distance. The ratio is thus about 1½ and not 2. Taking the case of tests from plate to point, it will be found that with alternating current 50 mm. were bridged by about 28,000 volts, and with direct current 50 mm. were bridged with 41,000 volts when the point is positive and the plate is negative. Thus we again have a ratio of about 1·5. In other cases the ratio is 2 and more; but it seems to me that it will not be safe to design insulators for other than the most severe conditions that are likely to occur. I do not myself consider that there can be much in that higher factor of 2. Much depends, in insulation tests, upon very many at first sight unimportant conditions, and I suggest that it would have been interesting if, when giving these tests on the presspahn and on the piece of marble, the dimensions and shape of the electrodes had been given, as well as various other details regarding the tests. One might judge from the paper that these tests were drawn from a large mass of such data; but inasmuch as these same tests on the presspahn and the marble were given considerable publicity three years ago, and these alone, it seems almost as if only a very limited amount of testing had been carried out with reference to this important point. If any other tests made by the advocates of this system are available, it would be very interesting if the author could publish them. The effect of the periodicity will also change this constant of 1·5 or 2. Mr. Rayner brought this out in some tests he made at the National Physical Laboratory, and it may be that he has made still further tests regarding the matter. If the disruptive strength varies with the periodicity, one cannot state the ratio of the disruptive strength on direct current and on alternating current without further qualification. These are tiresome details, but they are necessary in leading up to a rather far-reaching point at a later stage of the paper. Turning to page 492 estimates will be found of the generating-station costs for the two systems. These are, in the main, fairly prepared. Nevertheless I should have taken four 600-k.w. generators for the alternating system for the smallest station, and perhaps six of 6,000 k.w. for the next to the largest station, and others in proportion, instead of the larger numbers of smaller units set forth in the table. In that case the buildings, because of the less floor space necessary, would be a little cheaper and the generating plant as well; the switchgear, because of the smaller number of circuits from generators to switchboard; the boilers, because less steam would have to be supplied to the more economical larger plant; all these ought also to be cheaper, and the total cost ought to be a little less than given here. The author has obtained the total cost for his 7,000-k.w. installation by interpolating it between the 2,400-, and the 14,000-k.w. plant, and has taken it at £17 per kilowatt for the alternating current plant. I should think it would be quite fair, in view of these criticisms, to bring that figure down to £16. Then, in the case of the Thury system, while the large

Mr. Hobart.

high-voltage direct-current generators which it is proposed to use, namely, 1,000 k.w. and 2,500 k.w., and 2,500 volts and 4,000 volts, do not seem to me, when taken by themselves, to be in any sense bad engineering propositions, nevertheless it must be remembered that the largest generators used in any of the Thury systems given in the table are those installed last year. These are of less than 600-k.w. rated capacity. That is the very largest yet installed, and the next largest size is something like 400-k.w. rated capacity. Therefore it is, in a sense, rather a bold proposition to figure on having 1,000- and 2,500-k.w. direct-current generators at pressures ranging from 2,500 to 4,000 volts per machine. Nevertheless this ought to be perfectly possible so far as relates to any difficulties that can be foreseen, and the more so because the author, as I am very glad indeed to see, suggests low-speed plant for driving the direct-current generators. If one must have direct-current generators driven by steam turbines, the difficulties are less the higher the voltage; and this is true up to rather high voltages. Nevertheless direct-current generators driven at steam-turbine speeds are, in my opinion, abominations. In each case the author has taken twice as many direct-current generators as alternating-current generators. If it seems desirable to be governed by any plant that has yet been built on this system, we would have to take very many more. Hence the cost of buildings ought to be a little larger; the cost of the many smaller sets will be larger; the switchgear will cost more because of the greater number of cables to the switchboard, and the boiler plant also, because each of the smaller generating sets will have a higher steam consumption. Hence the total cost ought to be taken a little higher in the case of the Thury system than the figure given in the paper. I have put it up £1 per kilowatt in the table on page 498. I have not gone into the sub-station equipment. It appears to be satisfactory. The other item is the transmission line. The 3-phase cables seem all right as regards price, but I take exception to the price of £580 per mile for the direct-current single cable of 0·1 sq. in. cross-section. I had to make some certain assumptions with regard to the thickness of insulation. There is 50,000 volts from copper to ground; and it did not seem unfair to the direct-current system to give the insulation a thickness of 25 mm. Taking the author's cost of copper and lead for comparison at £87 10s. and £16 15s., and taking £50 per ton for insulation, my cost for cables, for material alone, comes out at £630; and the cost of the complete cable, including labour and establishment charges, would be £945, which, added to the £900 per mile for laying, would make a total of £1,845 per mile. This, for 84 miles, would come to £155,000 for the transmission line.* Thus

* No data were given in the paper which could enable me to know the thickness considered desirable by the exponents of this system for 50,000-volt underground cable. A sample of 50,000-volt graded cable subsequently exhibited has an insulation thickness of only 14 mm. from copper to lead. If the factor of safety is sufficient, and if the grading process does not entail much greater expense per ton of insulation, the author's figure for the cost of this cable is sufficient. But to be consistent, he should

Mr Hobart. in my study of the case, instead of the values in the table on page 498, I obtain a final result of £53 per kilowatt instead of £47 7s. per kilowatt for the direct-current plant, and £54 4s. for the 3-phase plant instead of £55 4s. While these results only relate to a single case, it is significant that it bears out the conclusions arrived at in other cases in comparing the Thury system with the 3-phase system, as already shown by other speakers. I am very much in favour of employing the CONSTANT POTENTIAL high-tension direct-current system for traction, but that is because there is not any good single-phase motor; but the 3-phase system for generating is in every respect so very satisfactory that I cannot see that any good purpose is served by substituting another system which is not any cheaper.

Mr. Irwin. Mr. J. T. IRWIN: I had the privilege about two years ago of assisting Professor Ayrton in some calculations regarding the transmission of power from the Victoria Falls to Johannesburg. It was found, when we came to calculate the cost, that the cost of the line was such a large proportion of the total cost of the plant, that the direct current came out much cheaper than the alternating current. In addition it was possible to allow a larger loss in the line on the direct-current system, as with alternating current it would be practically impossible to get machines to run efficiently at the receiving end if a large drop of voltage were allowed on the line, especially with asynchronous motors, where the torque is roughly as the square of the voltage, and one must limit the total voltage variation in the line. Therefore it was found much more efficient to have direct-current transmission from the Falls; that is, if any transmission was to take place from the Falls to Johannesburg the only possible commercial scheme was the direct-current series scheme. Professor Ayrton was aware at the time that there were a good many disadvantages in the direct current. There is the disadvantage of having constant losses. If generation is by water power these are not at all important, because the losses in the line do not cost anything, except that at full load the extra cost of generating the amount at the Falls has to be taken into account. In addition to that, there is the constant loss that takes place in the motor. When one designs an ordinary machine to run on a constant voltage system, if the load varies, practically constant losses occur in the motor, due to hysteresis, eddy current and excitation, but the losses in the armature depend also on the square of the current in the armature, and therefore the losses are only a maximum when the load is a maximum. On the other hand, when one is dealing with a constant-current system, and regulates the torque of the motor by varying the position of the brushes, the hysteresis, eddy current, and excitation losses are constant, and, in addition to these, there is a constant C^2R loss in the armature as well, because the current is constant. This is especially important in dealing with transmission of

take correspondingly less insulation thicknesses for the 3-phase transmission line, as the costs he has assigned to the 3-phase cables correspond to disproportionately great insulation thicknesses, even when granting the 2 : 1 disruptive strength ratio.

the kind referred to in the paper. One of the great objections to the house-to-house system of transmission is the constant iron losses which take place in the transformers. There is also a great disadvantage in the constant-current system, namely, that the maximum torque is limited; the motors cannot be overloaded. The motor is designed for so many pounds-feet of torque, and it will not give any more because the limit is reached when the full strength of the field is attained with the brushes in the neutral or normal position. Therefore, in the case of a machine where it is subject to big fluctuations of load, the machine must be designed not for the normal torque but for the maximum torque, and it would be inefficient for any ordinary torques. With regard to transmission by a constant-current system through underground mains, the case is entirely different from transmission through overhead wires. It is also entirely different from the case where transmission is partly through overhead wires and partly through underground mains. That is perhaps the worst system of all; because in case of a storm a break may occur in the overhead wire, causing enormous surges of current round the circuit, and the generation of very high voltages. The voltage of the system may easily exceed the normal by two or three times if the circuit is suddenly broken, and therefore the cables, with these very large voltages superimposed on them, are likely to break down. The fact of the underground mains acting as condensers would aggravate this trouble. Although Professor Ayrton advocated the direct-current system where it was purely overhead, he would not advocate it for a system such as the author has sketched out where it is purely underground, or where it may be partly underground and partly overhead. Mr. Irwin.

Mr. R. A. DAWBARN: I share the appreciation of the paper that has generally been expressed, and I also admire the boldness of M. Thury, who has developed this series system practically single-handed. I take it that the system is essentially a long-distance transmission system. As such it is, of course, especially adapted for water-power schemes. Reference is made to generation by steam power. Long-distance transmission where steam is the source of power is very rarely met with; long-distance transmission is almost invariably from water power. But taking a case where steam is the motive power, there is one feature of this system which I think is worthy of notice. Attention is called in the paper to the fact that it is a constant torque system, and as such it admits of the steam engine working at all loads under its very best economical conditions. That cannot fail to show its effect in reduced coal consumption—probably to a very appreciable extent. But, on the other hand, the C^2R loss being constant gives away a great deal of this advantage—the amount depending upon the length of circuit. It is quite conceivable it might give away more than the advantage which is derived from the excellent conditions under which the steam engines work. Another point arises out of the engines working so economically at light load on this system, namely, that it is not necessary and not desirable to adopt so many Mr. Dawbarn.

Mr. Dawbarn. generating units as we are accustomed to adopt in parallel working. A station in which there were two running sets and one spare set would probably be very much more economical in working than one having five or six generating sets. The maintenance of the generating plant would also be very considerably reduced with this system, because the cost of upkeep is practically proportional to the number of revolutions and not to the output, and the number of revolutions per annum on variable load with this system would be very considerably below the number of revolutions made on a parallel system with constant speed in the same period. The smaller number of generating units would also affect the attendance expenses favourably, and the capital outlay as the foundations, buildings, switchboards, and all accessories would be kept down to a minimum. It appears to me, however, that for anything approaching a large scheme, with a large number of distributing points such as are shown on the diagram, if those points are in themselves small the ordinary parallel system with static transformers would work out best. If, however, the loads at the distributing centres are large, one would desire to know what the special conditions are which prevent these large centres from generating economically for themselves. It appears to me that this system is best adapted for serving from one station, say, four or five comparatively small industrial centres, any one of which is not quite large enough to permit of a local station working under economical conditions.

Mr. Peck. Mr. J. S. Peck: The author has made quite a strong case for the direct-current transmission system, but in many instances I think he has made virtues of necessities. The paper appeals to me as showing strongly the possibilities rather than the probabilities of that system.

On page 474 the author says that when the neutral point of the system is grounded, the voltage between wires may be twice that with the ungrounded neutral with the same insulation strain. With this statement I do not agree, for when both sides of the circuit are insulated there should be only one-half the total voltage between either wire and ground, and this strain is not reduced when the neutral point is grounded. It is true, however, that with the neutral point grounded, the voltage between either wire and ground is limited to one-half the voltage between wires, whereas with the ungrounded neutral it is possible to obtain full voltage between one wire and ground in case the other wire is grounded. On the other hand, it is possible to operate with the latter condition, while with a grounded neutral, a ground on either line wire will cut out part of the system.

But assuming that the author is correct, then the same argument would apply to the A.C. system, and with the neutral grounded, we should be able to operate with 73 per cent. greater voltage across the phases than with an ungrounded system. There are several ungrounded A.C. systems operating at 60,000 volts. By grounding the neutral, they should therefore be able to operate at 104,000 volts with the same insulation strain. This would appear to be a sufficiently

high voltage to cover any reasonable distance without excessive line loss. Mr. Peck.

Granting that the direct current gives less strain on the insulation, that there are no capacity and inductance troubles, and therefore that it is easier to transmit, is there any necessity for adopting it? That is, have we reached the limiting distance for A.C. transmission? I refer now to long-distance transmission, where current will be carried on overhead wires, for all *very* long transmissions will probably be operated in this manner. Limiting voltages have not been reached so far as the insulator is concerned; certainly not on the transformers, and if we have reached limits on account of capacity and inductance at normal frequencies, there is no reason why we should not go to lower frequencies. As the frequency is reduced, the capacity and inductance effects are decreased proportionately, and I see no reason why 10 or 15 $\sim$ should not be adopted for very long transmissions where extremely high voltages are required.

It appears to me that the troubles due to capacity and inductance in transmission circuits are more theoretical than real, at least up to voltages of 60,000 and frequencies as high as 50 or 60 $\sim$, for systems of this voltage and frequency are operating with complete success.

There are several very serious drawbacks to the direct-current series system:—

1. Large number of small units.
2. The difficulty of insulating generators and motors from ground and from prime mover, this difficulty increasing with increased size of unit.
3. The difficulty of providing a suitable friction coupling between generator and prime mover.
4. Difficulty of insulating armatures and fields for very high voltages.
5. Danger to attendants from shock in the event of armature or field insulation breaking down.
6. Necessity for expert attendants for operating machines.
7. Necessity for providing complete sub-station with expert attendants wherever power is to be used.
8. The commutator.

The last item is perhaps the most important of all, for ever since direct-current machines have been built, there has been more or less trouble with commutators, and although the commutation of D.C. machines has been greatly improved of recent years, and we now hear much said about forced commutation, it is still a fact that the commutator is in general the weak point in D.C. machines, and while currents of a few amperes may be commutated successfully at high voltages, the difficulties will increase rapidly as the amount of current is increased, and I think that almost any practical engineer would have to be assured that alternating-current transmission was almost impossible before he would agree to instal high-voltage commutating machines of compara-

Mr. Peck. tively delicate construction instead of stationary armature alternate-current generators of the present rugged type of construction.

Mr. Andrews. Mr. LEONARD ANDREWS: The chief point on which there appears to be a difference of opinion is, as to whether it is possible to combine the principles of the 3-wire systems with the direct-current high-tension system. The whole question turns upon the point Mr. Sparks drew attention to at the last meeting, that is, whether the osmotic attraction of moisture by the negative conductor is likely to prove a serious difficulty. During the past few years I have had the opportunity of discussing cable troubles with a large number of station engineers, and I have been forced to the conclusion that even with ordinary low-tension direct-current systems this osmotic trouble is quite a serious one. I have heard of a number of cases where breakdowns have been attributed to it. Only a few months ago my attention was drawn to an interesting case where the effects of osmotic attraction were apparent. A number of conduits had been laid side by side; some of these conduits had single, positive, and negative rubber-covered cables drawn into them, and others were empty. The conduits containing the negative cables contained a large quantity of mud and water, which had evidently been drawn into the ducts from the draw-in boxes. It is, I think, conclusive proof that the moisture and dirt had been drawn in by osmotic attraction, as the ducts containing the positive cables and the empty ducts were as dry and clean as the day they were laid. It was also noticeable that similar ducts containing lead-covered, single negative cables were perfectly clean and dry. None of the various electrical text-books that have been written appear to give any very clear explanation of the theory of osmotic attraction by the negative conductor, unless it is explained by the experiments described in Lord Armstrong's book on "Electric Movement in Air and Water." If this can be considered an explanation, it would appear that the effect is screened by a metallic shield round the conductor, and this would account for the effect being noticeable with rubber-covered cables and apparently entirely obviated by the use of lead-covered cables. This appears to be a subject that we ought to know more about, and I would like to suggest that it would be an interesting topic for a paper before this Institution. Mr. Bailey has told us that he has had direct-current cables working at a pressure of 3,000 volts, and that he has not experienced any trouble from this cause. I should like to ask Mr. Bailey if the cables he refers to were lead covered, and whether they were single cables, or concentric, or multiple cables.

A reference has been made to the author's suggestion that it is a mistake in connection with parallel systems to use heavy flywheels on the generators. It appears to me that one reason why flywheels in this position are necessary is to prevent the heavy surging of the current between the generators that invariably occurs, both on alternating- and direct-current systems, when a short circuit occurs on the feeders or distributing systems; and if the fly-wheels are abolished on

the generators, one would certainly expect that this surging would be very much more disastrous. Mr. Andrews.

On page 498 the author refers to the high proportion of the cost of the ducts and trench work, compared with the cost of the cables in the series system. I see in the tables he estimates the cost of ducts and trench work at £900 per mile, that is to say, something over 10s. per yard. This appears to me to be abnormally high for trenchwork, &c., in districts where the series system is likely to be used, viz., in comparatively open country districts. Mr. Watson, Borough Electrical Engineer, of Bury, in a paper he read before the Manchester Section of the Institution, gives the cost of a good three-way duct laid in concrete as being £513 per mile. This price includes 2s. per yard for excavating and reinstatement, and approximately 1s. 4d. per duct-yard to cover cost of handling, carting, laying, and concrete, making a total cost of 3s. 4d. per yard for single-way duct. Whilst this estimate is, I should think, on the low side, I should have expected, from my own experience in laying conduits in country districts, that the cost would not have exceeded 4s. per duct-yard. It would, of course, be very much more in or near very large towns. This point appears to me important, because it will be seen from Table VIII. that the cost of conduits and trenching in some cases amounts to more than twice that of cables, and the whole question of the advantages or otherwise of the high-tension direct-current system compared with alternating-current systems appears to turn on the relative capital cost.

The author has referred on page 484 to the simplicity of the Thury system, which is certainly very marked, particularly as regards switching generators and motors in and out of circuit. He adds that "the system does not lend itself to a large number of automatic contrivances, such as are generally used in parallel systems," but it appears to me that there are quite a large number of automatic devices used in connection with it. Take, for instance, those Mr. Highfield refers to. We first have the device for short-circuiting the generator should the direction of rotation become reversed. Then we have the automatic slipping couplings, and the automatic regulators for regulating the speed of the generator, or the position of the brushes, and it must be remembered that this last device has to be applied to the motor as well as to the generators; and finally, it is, I believe, usual to use a device which is not referred to in the paper for short-circuiting the line in the event of an open circuit, the object of this being to prevent the abnormal rise of pressure that would otherwise occur. I should have thought that these appliances provided sufficient scope for the most enthusiastic believer in automatic devices, and they are certainly far more likely to give trouble than the simple automatic devices used in connection with parallel systems, which, if properly constructed, can be *guaranteed* always to operate in the way intended.

Mr. A. Russell: At this late hour I shall merely refer to one point in the paper. The author mentions that in some cases insulating Mr. Russell.

Mr. Russell.

materials break down more readily with alternating than with direct pressures of the same maximum value. Several experimenters have obtained similar results for cables. A pressure of 10,000 volts alternating is sometimes more effective in breaking down cables than 20,000 volts direct. The reason I consider to be that in the cables used in practice the dielectric is not isotropic. It is formed of various layers of insulating wrappings, and the potential differences across these layers are not necessarily in phase with one another; and hence the algebraical sum of all the potential differences is sometimes greater than the potential difference between the two outside ones. If a non-inductive resistance, for instance, be put in series with a condenser, the sum of the two potential differences may be about 40 per cent. greater than the potential difference applied across the terminals. By taking the resistivity and the dielectric coefficients of the various insulating wrappings into account, I find that in some cases the potential differences across the various layers of the dielectric are not in phase with one another, and therefore the ratio of the disruptive pressures for direct and alternating voltages, instead of being the ratio of the maximum to the effective value of the applied P.D., can sometimes be appreciably higher.

(*Communicated*): It is now generally admitted that the insulating medium at a point breaks down the moment the electric stress at that point attains a definite value depending on the physical condition of the medium. Whether a disruptive discharge ensues or not depends on whether the breaking down of the medium at the point raises the pressures at other points to their breaking-down values. By considering the experimental results obtained by Kelvin, Carey Foster, Heydweiller, Steinmetz, Thury, etc. (see *Phil. Mag.* [6], vol. 11, p. 259, 1906), I have shown that the breaking-down voltage for air is determined merely by the maximum value of the electric stress (3·8 k.v. per millimetre under normal conditions). It is the same—at least for frequencies not greater than 125—for alternating as for direct pressures.

M. Thury's experimental curves B and D, shown by Mr. Highfield in Fig. 3, are in apparent contradiction with this result. From these curves it appears that when the pointed electrode is negative we require a greater voltage between the electrodes than when it is positive. It would not be safe, however, to draw this conclusion. The maximum electric stress between two electrodes does not depend merely on the difference of potential between them. It depends in a very special manner on the absolute values of the potentials. If in one case the plate were $+V_1$, and the pointed electrode $-V_2$, it would be exceedingly difficult to arrange so that the plate was $-V_1$ and the point $+V_2$ in the other case. Yet it is only under these conditions that we would be justified in inferring that the breakdown occurs more readily when the pointed electrode is positive. With pointed electrodes also the air in the immediate neighbourhood of the points is broken down (ionised) before the disruptive discharge takes place, and hence the

Mr. Russell.

time element introduces uncertainty into the experiment, and the calculation of the stress is exceedingly difficult, if not impossible. In actual cables and in sheets of various insulating materials the dielectric is similar to Maxwell's composite dielectric. Several engineers have accidentally found by painful experience that the phenomena of leakage and residual discharge are strongly marked in high-tension cables. These cables often give dangerous shocks after they have been discharged and left isolated for many hours. When considering the disruptive voltages of cables, therefore, it is only permissible to neglect the resistivity of the insulating wrappings in certain special cases. It is not difficult to see that a cable which is suitable for high direct voltage work may be unsuitable for high alternating voltages and *vice versâ*. The electric stresses to which the wrappings are subjected in the two cases may be quite different. The experience of several cable manufacturers is that the high voltage cables which they make at the present time will withstand direct voltages at least twice as great as the effective value of the disruptive alternating voltages.

Mr. Highfield gives an impartial summing up of the relative advantages of direct- and alternating-current systems of power transmission. It is exceedingly difficult, however, to deduce general conclusions without knowing the nature of the load. If the load were almost constant, then, up to certain distances, an alternating-current series system would have advantages. The management of a row of series static transformers would be preferable to standing charged to a high potential on an insulated platform and gradually growing callous to the static discharges from the machines.

I think Mr. Highfield's method of earthing cables by boring to a good conducting stratum well worth consideration. In certain cases it might lead to great economies. The "bonding" of geological strata may be an important engineering operation in the future.

The Chairman.

THE CHAIRMAN (Mr. W. M. Mordey): I am sure you would like to hear the opinion of Mr. Guido Semenza whom we had the pleasure of seeing among us last year in company with other members of the Italian Electro-Technical Association. He has, as you know, had a large experience of electrical engineering in very important positions in Italy. I was corresponding with him on this subject last autumn, and he wrote to me from Milan on October 13th, a letter, the important part of which I will now read:—*

Mr. Semenza.

Mr. GUIDO SEMENZA (*extract of letter communicated by the Chairman*): "Direct current has many advantages for long-distance transmission, the principal being as follows:—

(*a*) Simplicity of lines, as there is no necessity or reason to use more than four wires for a transmission (only two wires do not secure sufficient continuity of working).

(*b*) Possibility of insulation with very high tension, as wires can be

* Since then Mr. Semenza has informed me that he thinks the direct-current system should be compared with alternating-curreut system only for very heavy transmissions, say over 100 miles.

Mr. Semenza.

placed as much apart from one another as one thinks best, while with 3-phase current wires cannot be placed very far from one another on account of self-induction, thus :—

(*c*) Possibility of reaching much higher tensions than with 3-phase currents.

(*d*) Absolute independence of the question of parallel running.

When the transmission distance is very considerable the parallel working of the alternators on the two ends is not sure.

(*e*) Absolute independence of voltage regulation.

(*f*) Possibility of using cables for higher tensions in direct-current than in alternating-current transmissions, as it appears from recent researches.

On the other hand, we have the following drawbacks :—

(*g*) Necessity of using small generating units.

(*h*) Direct-current high-tension generator with all its inconveniences.

(*k*) Necessity of rotary transformers for every application.

Finally we have some unknown points to consider, namely :—

(*l*) Behaviour of such lines under atmospheric disturbances.

(*m*) Behaviour of such lines under instantaneous variation of current.

In conclusion, I believe it is worth while studying the question very carefully, especially the transmission by direct current being so much cheaper."

Mr. Heaviside.

Mr. A. W. HEAVISIDE (*communicated*): Though I may be repeating common knowledge I am tempted to observe that on page 482 it is stated that in M. Thury's experience "no disturbances were apparent in either the telephone or the telegraph services." As every one is aware, such a thing as a steady current from a mechanically driven plant does not exist, there is always a ripple caused by the brushes collecting from the moving commutator segments; however, as the undulations are very slight as compared with those from alternating plant in general, the inductive disturbances to double telephone and single telegraph circuits from direct-current plant are not serious. But when sensitive telegraphic recorders are used, as, for instance, Lord Kelvin's syphon recorder, or the Danish modification of that beautiful apparatus, called the undulator, they respond to most of the vagrant electrical impulses in the environment of their circuits. Especially so if the impulses arise from tramway circuits with a potential drop of 7 per cent., or a railway service with a drop of, say, 20 per cent. How sensitive the undulator is will be apparent if I quote from an actual test. An undulator will record graphically the presence of a current through its coils on a paper tape of the value of 0·0000428 of an ampere, or stated on a telegraphic basis, that is the milliampere, 0·0428, or the twenty-third part of a milliampere, at a speed of about 50 undulations per second. And Lord Kelvin's syphon recorder is very much more sensitive.

Mr. Heaviside.

FIG. E.—Coarse Adjustments of Apparatus.

FIG. F.—More Delicate Adjustments of Apparatus.

FIG. G.—Fine and Sensitive Adjustments of Apparatus.

SYPHON RECORDER INDUCTION MARKS.

Mr. Heaviside.

On the introduction of electric tramways in Newcastle and Gateshead some years ago I joined up spare telegraph wires in iron pipes under the pavement parallel with each rail of the tramway service, the ends being brought into my office, where they were joined up at will either to the syphon recorder or the undulator. In addition, means were taken for measuring the voltage and current either from earth leakages or parallel induction. In one case leakage currents into a very short telegraph circuit varied from 0 to 400 milliamperes, with a voltage from 0 to 6·5 volts. As there are many electrical activities in the neighbourhood of Newcastle, to ensure that what we were measuring was leakage or induction from the tramways, it was necessary to take special tests as well as general observations: hence trial runs of electric cars were arranged for on Sundays, or after midnight in the small hours, when most, if not all, other services were shut down.

Summarising the Results.—The acceleration or retardation of the speed of the cars was recorded on the paper tape as a kick or a series of kicks governed by the number and abruptness of the steps in the controller resistance, the marks on the paper being either above or below the zero line. Starting, above; and stopping, below. I submit herewith a scale drawing of a concrete case derived from an undulator record in which the start and the stop is clearly recorded. I also submit the actual tape record of disturbances responded to by a syphon recorder.

Of course, all this led to some apprehension as to what would happen when the N.E. Railway was electrified on the north bank of the River Tyne with a suggested potential drop of, say, 20 per cent.

In further investigation of the tramway case I resorted to an exploring device which I first used in 1880. I took a coil of insulated wire wound on a triangular frame about 5 ft. high with a telephone in circuit and placed it in the space between the up and down rails on the Gosforth section of the Newcastle Tramways. On listening one could hear (1) the 90 revolutions of the power-house engines; (2) the steady note of the ripple over the commutator bars; (3) the whistle of the car motors rising shrilly as the car speeded up; (4) the starting and stopping of the cars; and (5) the sparking as the trolly wheel passed each post. To the expert telephone user these sounds can be easily differentiated. When the N.E. Tynemouth branches were electrically driven, I proceeded to Benton and placed this triangle detector above the railway on the road bridge parallel with the rails, say 20 ft. above the railway. On listening the usual sounds were faintly heard, the whistling of the motors predominating.

Of course, the question arose as to how much was conduction and how much was induction. In order to set doubts at rest more than anything else, I placed an insulated circuit, roughly in the form of a square, spread over the adjoining fields with one side lying in the gutter of the railway, approximately a mile in length (4 miles in all). Into this circuit was joined in turn the telephone, the syphon recorder, and the undulator, and the disturbances were recorded with more or

Mr. Heavisi

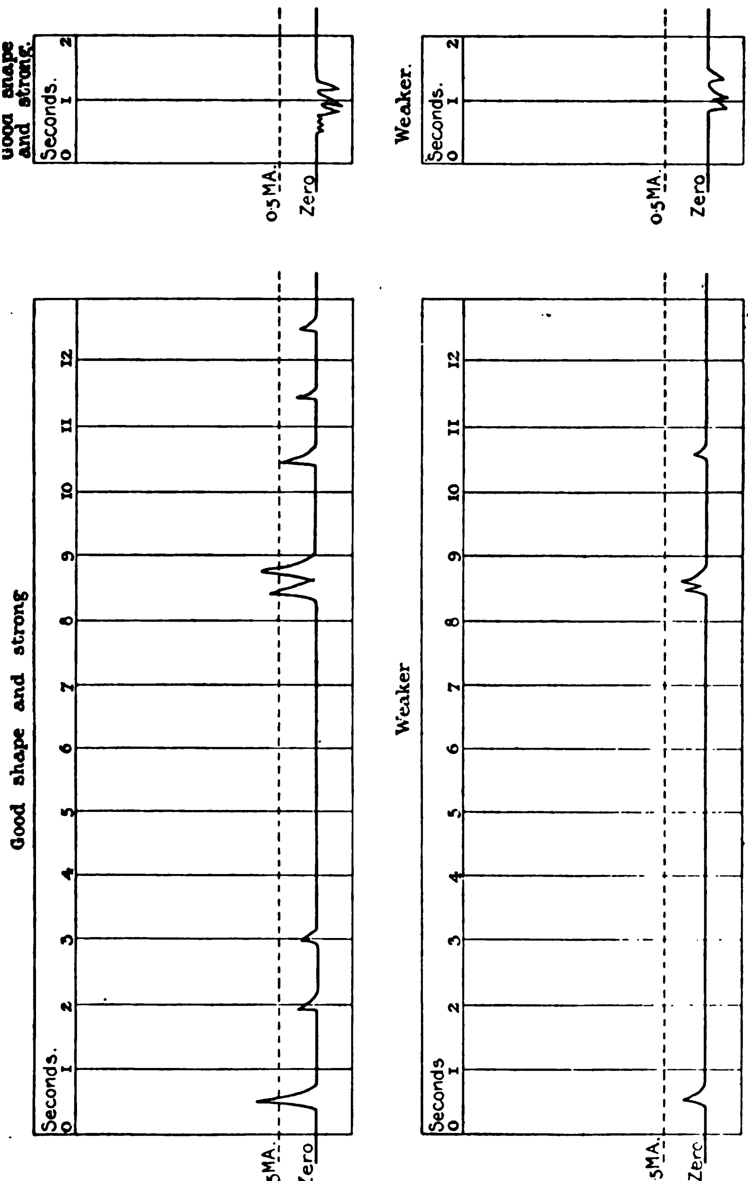


FIG. H.—Series of Tram Disturbances, Frequently Recurring Experiments, December 20 and 24, 1903.

Mr. Heaviside.

less distinctness, as might be expected. The N.E. system is one where no special return rail is used, hence the electromagnetic external field is a wide one. Making some kindred experiments alongside the District Railway at Ealing, where a special return rail is used, the range of the external field is so reduced as to be nearly negligible as an interfering agent.

In the Newcastle case the practical result of the interference was such that it has been necessary to double the wiring of an important telegraph circuit that was carried on poles for a few miles parallel with the railway on an adjoining road.

Of the merits and simplicity of a direct-current high-potential sequence system advocated by Mr. Highfield, from my point of view there can be no question. It has its parallel in an arc-lighting service. Even the automatic devices may be arranged to assist in choking violences, and the insulators form a distributed leak of value in such a case. And as regards the high potentials, the danger is not greater than with any potential that will kill. It is as it is with an express train or a slow train, with either one or the other if any one gets in the way he will be killed; so it is with high electrical potentials or extra high ones, with this difference that in the express train case those within the train are in greater danger than those in a slow train.

But returning to the interference problem. It is quite evident that telegraph and telephone circuits are, and will be, interfered with, and I am sure that at the present time that telegraph engineers and linemen in some cases suffer from the sins of others when blamed for imperfect maintenance of their lines. For consider the gauntlet a telegraphic signal has to face in passing from London to Aberdeen, etc.—a terrible ordeal!

In my first paragraph I refer to double telephone circuits not being free from disturbance by external fields. This is so, if the voltage and the frequency of the primary are only increased sufficiently and slight errors in the symmetry of the telephone circuit, either in the Hughes' twisted line wires or in the balance of the circuit are made apparent by noises in the telephone which interfere with speech.

Mr. Tilney.

Mr. M. J. E. Tilney (*communicated*): There is one point in connection with Mr. Highfield's paper that does not appear to have been touched on by any of the speakers, and that is the fact that under certain circumstances the curves of "sparking distance," as given in Figs. 2 and 3, may lead to erroneous conclusions if any one should mistake them for "arcing distances." By this I mean that although the sparking distance for a given voltage is much greater in the case of A.C. than it is for D.C., the distance across which a given D.C. voltage will *hold* an arc is much greater than the distance across which the corresponding A.C. voltage will maintain an arc. Some recent experiments show that an arrester that will quite easily discharge a surge on 5,000 volts A.C. will *hold* an arc, when it is once started, on 500 volts D.C. It will, I think, therefore be of great interest if Mr. Highfield can give us particulars of the type of arrester M. Thury has adopted.

This question, it seems to me, will also have a very considerable effect on the design of overhead mains equipment. Mr. Tilney.

In the photograph, Fig. 4, in the paper, the wires for this 27,000-volt transmission look rather unduly close to one another, to say nothing of the pole, and assuming an arc were once started I should expect some difficulty in its extinction.

I think perhaps rather too much stress has been laid on the effect of surges by some of the speakers, due to the fact that they have been drawing conclusions from existing high-tension D.C. systems with shunt-wound generators. The surges in these cases appear to be entirely caused by rushes of current, and in the system, as Mr. Highfield describes it, there would seem to be very little chance of such rushes taking place. There would therefore be very little to cause a surge except in the case of a break in the circuit, which is, I understand, provided for by an automatic short-circuiting device.

I should like to mention that I do not think Professor Kapp need fear that British manufacturers are behindhand in the construction of high-tension D.C. machinery. As an illustration of what was possible over two years ago, before the use of commutating poles came into practice, I may say that at an official test of a 200-k.w. set for use at 2,650 volts D.C. which I witnessed at the maker's works, the switch, owing to a mistake on the part of an assistant, was closed on a dead short-circuit, with the machine up to speed and fully excited independently. Though the switch did not look very healthy afterwards, we did not touch either commutator or brushes but took the six hours' test, and at the end of the test switched off full load without any sign of sparking on the machine, although owing to bad governing the machine volts rose to something over 3,800.

Mr. PERCY R. ALLEN (*communicated*): The Thury system will be at once recognised as an amplification of the old Brush-series system with motor-generators substituted for arc lamps, and as a matter of fact more than twenty years ago belt-driven machines of the Brush and Wood type were sometimes operated in series by being placed on insulated wooden platforms, the regulation in the Wood machines being obtained by shifting the brushes, and in the Brush machines by having a variable shunt across their terminals. Of course these early experiments in no way detract from the credit due to M. Thury for the ingenuity he has shown in developing what is practically a new system of long-distance transmission. The author in his paper makes a specific comparison between the Thury system and an alternating one, over an area where the sub-stations are fairly close together and where the power is apparently generated at one central station on the edge of the area, and the calculations he bases thereon are of considerable value, as the conditions given are those that would most likely be met with in practice. However, there are other cases where this system would seem to have distinct advantages. The ease with which several generator stations can be placed in series on one ring with the sub-stations to absorb the power placed anywhere on the Mr. Allen.

Mr. Allen same circuit renders it an exceedingly convenient method of collecting power from a number of sources, transmitting it a long distance, and then distributing it through a group of sub-stations.

The suggestion that power from blast furnaces and coke ovens in the Midlands should be utilised and transmitted to London has been frequently proposed, but so far no accurate estimates appear to have been got out in connection with the matter. The modern gas engine in units up to 3,000 H.P. and worked off blast-furnace gas is a perfectly reliable generator, and in many of the iron and steel works on the Continent the furnaces are blown entirely by gas engines without any reserve in the way of steam power. The new steel works of the Deutscher-Kaiser Company will depend entirely on blast-furnace gas, 48,000 H.P. being provided by 24 gas engines of 2,000 H.P. each, 12 for blowing and 12 for the generation of electricity, and a very brief visit to the Continent will convince anybody that there need be absolutely no hesitation in using gas engines to supply power in bulk.

With regard to the possibilities of supplying London, it will be found that by following very closely the Midland Railway only as far as Glendon we get into a group of 20 furnaces in Northamptonshire, of which 12 were in blast at the end of last year, and after supplying sufficient gas to heat the stoves and blow the furnaces and the general work, which the French term "the service of the furnace," this group would have 30,000 H.P. continuously to dispose of. By going about 35 miles further the Nottingham and Leicester furnaces are touched. There are 7 of these, 5 of which were in blast at the end of the year, giving under similar conditions about 12,000 H.P. Going further North one gets into the Derbyshire district, and between Derby and Sheffield there are 44 furnaces, 38 being in blast, corresponding to 96,000 H.P. If a detour was made from Sheffield of a few miles into Lincolnshire a further 35,000 H.P. could be obtained from the Frodingham group, 14 of which were in blast. In the South and West Yorkshire group, between Leeds and Rotherham, still in close proximity to the Midland Railway, there are 25 furnaces in this district, of which 16 are in blast giving 40,000 H.P., making 213,000 H.P. in all which could be picked up.

Following the North-Western Railway into the Midlands there are in South Staffordshire and Worcestershire 33 furnaces, of which 19 are in blast, which would supply 48,000 H.P. In Shropshire 3 are in blast which would supply 7,000 H.P., and in North Staffordshire 16 were in blast out of 32, capable of supplying 40,000 H.P., so these three groups would supply 95,000 H.P., but the transmission line would be of a more sinuous nature. It must be understood that these figures may not be quite correct for the individual groups, but they have been pretty well verified as to the total power available. It would be exceedingly interesting to see whether the Thury system of transmission would enable this power to be collected and delivered at a payable price in London. 3,000 k.w. can be put down even in small

Mr. Allen.

units, inclusive of continuous-current generators and gas engines, for the sum of £10 per kilowatt. To this must be added the cost of the gas-cleaning plant, compressed air arrangement for starting, cooling water arrangements, engine house and overhead traveller and switchgear. The extreme simplicity of the Thury switching arrangements renders the expenditure under this head a small item. It should be borne in mind that the gas producer already exists in the form of the blast furnace, and the generating plant is strictly limited to the engines and the dynamos. Boilers, economisers, coal-handling plant, and condensers are absolutely done away with. In the two routes already referred to a considerable amount of power is becoming available by the more general introduction of recovery coke ovens, but there are no statistics at present as to how much of this can be relied upon.

The possibilities of a transmission scheme of this sort obviously depend very largely upon the cost of the cable, and having in view the fact that continuous current of a constant value has a negligible disturbing effect on surrounding wires, it seems possible that satisfactory arrangements might be made with the railway companies to run naked overhead conductors on either side of their track, at all events along the straight runs in the country districts. These would also form a convenient means of supplying sub-stations for the electrified portions of the railway. The possibility of being able to use the earth to cut out a faulty piece of line is a feature of the Thury system which removes the objection that is sometimes heard against the supply of power in bulk from a distance, as it prevents any lengthened interruption to the supply owing to a temporary breakdown, and, moreover, the system has the advantages that as the machines are simply worked in series with one another any accident to an individual generator is very unlikely seriously to affect the other units. The faulty machine is simply cut out and the remainder speeded up or a spare machine put in. Of course, the idea of feeding a power-supply system from a number of sources is not a new one, but it is hardly realised in this country how far small power houses over a scattered district can be linked up. The Rhenish-Wesphalia Company, which has been exceedingly successful commercially, operates over a series of circuits more than 600 miles long, and besides the two main power stations at Essen and Hoerde purchases power from a number of blast furnaces and coke ovens in the district, and feeds this into the general system, the price paid being somewhere about one-third of a penny per kilowatt-hour.

In analysing the comparative costs of the Thury system with a 3-phase, it might have been mentioned that before the transmission line from St. Maurice to Lausanne was decided on an independent committee of five experts was appointed to go into the matter. The scheme involved transmitting 5,000 H.P. thirty-five miles, there being two sub-stations on the way. The committee, after very carefully going into all points, came to the conclusion that the cost of an installation on the 3-phase system would be £324,200, and the cost of the continuous-

Mr. Allen. current arrangement on the Thury system £294,600, showing a saving of £29,600 on the complete installation.

The projected transmission line between the Falls of the Rhone and Paris is intended to be worked on the Thury system, and it will be exceedingly interesting to compare the cost of erection and operation of this line with the one that will be built on the 3-phase system between the Victoria Falls and the Rand. The system is already in use in Switzerland, Italy, Spain, France, and Russia, and it is to be hoped that a trial will soon be given to it in this country.

For the purposes of distribution over a compact dense area it does not appear to offer any advantages over a 3-phase network, but where a number of isolated power houses can be linked up to give a common supply it certainly does appear to offer many advantages, and the facilities which it gives for the use of large gas engines coupled direct to continuous-current machines will certainly cause it to be studied with interest by engineers who have projects in hand for gas-driven generating stations.

Mr. Highfield. Mr. J. S. HIGHFIELD (*in reply*): I would like to deal with the point that was first raised, I think, by Mr. Sparks, namely, the difficulty in connection with the phenomenon of osmosis. I first heard of the matter when I was at King's College as an assistant to Dr. John Hopkinson, and I think he first noticed it in the underground main system at Manchester, consisting of bare copper conductors and stoneware ducts. He carried out a large series of experiments on the whole question, on which I must not occupy your time by saying very much; but, shortly, we found that when there was a conducting path from pole to pole, then any moisture was transferred from the positive to the negative pole under certain forces, varying with the pressure and the nature of the connecting link and so on; but as long as there was no conducting path between the two conductors, then osmotic pressure was not exercised and there was no difficulty. In my own experience with underground net works the trouble with moisture occurs in the connecting boxes. It is quite certain that an underground low-tension net work, working with alternating current, does not give quite so much trouble in the joint boxes as in the case of direct current; but at the same time there is next to no difficulty with direct current, and therefore the matter is hardly worth speaking further about.

Then with regard to M. Thury's tests, it was impossible in the space of my paper to do justice to the data at my disposal. All I had space for was just the few simple tests I have set forth. When I stated that the effect of alternating current was 2¼ times the effect of direct current, I meant that was the general conclusion M. Thury told me he had come to. If I am able I will add something further on that subject.

With regard to the possibility of carrying the direct current on the cable systems, it is actually so carried to-day 6 miles, the last part of the journey from Moutier to Lyon; that is to say, there are about 96

Mr. Highfield.

miles overhead and the rest is underground, and a sample of the cable is exhibited on the table. Before this underground system was adopted, very many tests were made to ascertain if there were any unexpected difficulties to be met, and up to this time no difficulties have been encountered. I should remind you that for years, that is to say ever since the system was started, the cable used in connecting the machine together in the power-houses has been under the full direct current-pressure. All the work has been done with underground cables. I would call your attention to this sample, one of the Moutier insulators, which is designed for working at 60,000 volts. I should also say, in connection with the St. Maurice-Lausanne transmission line, that there have been a good many difficulties through trees and snow falling on the wire, and also, of course, through lightning. It is a most important transmission, because both the tramways and the lighting of the whole town of Lausanne depend on that line, simply two conductors. I do not say it has been decided, but I believe that so soon as they have sufficient money the whole of the line will be put underground.

One of the most important points I wanted to bring forward in the paper was the fact that this direct-current system gives us the means of going long distances underground, which we cannot possibly do with 3-phase currents. That was the first point I wished to bring before you.

It will be seen from the tables that the whole cost of the system depends on the generating station, the sub-station, and the line. In the case of the direct current, it may be taken that the cost of the generating station, in considering turbo-generators, that is to say steam turbine driven sets, is greater than the cost of the alternate current station, and therefore the determining factor is the cost of the line. Consequently when the line, as is the case with underground transmission, bears a very high proportion to the cost of the whole installation, it follows that the limit is very much higher than it is in the case of overhead work, where the cost of the line is a much smaller proportion of the whole. As a matter of fact, I got out figures for overhead work, with the idea of preparing corresponding tables for overhead work similar to those which I have prepared for underground work. But it is very difficult to make a general comparison between two lines overhead, that is to say, the direct-current and the alternating-current line overhead. Another thing was that the cost of the whole line was a smallish proportion of the cost of the whole installation—(I am speaking of distances inside 50 miles)—and therefore there was not very much money argument in the proposition.

Then I want to say a word or two about the constant loss, to which many references have been made. First of all I want to suggest a method of arriving at the value of the loss. I think the simplest way to adopt is to take the cost of the cable system or transmission system, and add to the cost of that system the cost of the plant

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required to force the energy into it. In a parallel system it is the cost of the plant required at full load. Add to the annual interest on these amounts the annual cost of the lost energy, and then deal with the transmission as though it worked at 100 per cent. efficiency. With regard to the cost of the line loss, reference was made to the use of the house transformer system. In the last four years I have had to clear out a large part of a house transformer system, and to substitute for it the ordinary low-tension sub-station method of working. If you calculate the efficiency of the house transformer system, you find you have to make something like 175 units for every 100 units that you sell; and if you work out that the 75 units cost you 2d. each, and you capitalise that, it is then clear that a large sum of money may be spent on changing the system. The cost of the energy on 100 per cent. load factor is, however, exceedingly small, far less than is generally supposed; it is usually wholly represented by a small addition to the coal bill. I wish also to remind you again that the series system can run at an overload, or, at any rate, if you please to look at it that way, the efficiency of the line may be improved when all the sub-stations are under the control of one operating body by reducing the current when the load is light; or, if you like to look at it the other way, by increasing it when the load is heavy. If you simply have two series machines, a motor and a generator, there is nothing to prevent both the machines being run at the ordinary 25 per cent. overload just as though they were parallel working machines.

Mr. Bailey referred to the work M. Thury has done. I have inserted one or two paragraphs in my paper on that subject, but I put them as shortly as possible. I quite agree, however, with what Mr. Bailey said about the marvellous ability that has been displayed by one man in working out the whole of the details of a system of this kind, and carrying it out quite by himself, with no assistance except the ordinary people in the works.

Several speakers criticised the tables of the cost of the plant. I had enormous difficulty in getting any figures at all, especially in getting them checked. I looked up books, quotations, and tenders; and, so far as I could see, there is no law at all as to what a British manufacturer charges for plant. The prices here could be reduced by 50 per cent. if I had taken certain figures; and the only result I could come to was that they do not know themselves what the cost of the plant is that they are selling—and if you look at some of the balance sheets I am bound to say they prove my contention. All I could do was, in order not to mislead myself (because these figures were prepared while I was considering a scheme to be actually carried out), to take what I fairly thought was about the price that I might expect to pay for plant two or three years hence, when the manufacturers have come to their senses. Although I, as a buyer, greatly rejoice at buying cheap plant in this way, as one taking a kindly interest in British manufacture I very much hope to see the prices go up.

Mr. Highfield.

Mr. Peck referred to the difficulties of insulating the bedplates and the couplings, to which Professor Kapp also referred. There is no question that the insulating of the coupling wants a good deal of designing when we come to large units. What M. Thury has done is this: when he has met a difficulty he has faced it, and gone one better than he had gone before. He has just kept up with the requirements of the particular system he has put down; he has gone just as far as is required with these schemes; but I have not the least doubt that, if you took him by steps up to 5,000-k.w. machines, he will make an insulating coupling that will work successfully; and I am equally certain that many other men can do the same thing. With regard to insulating the holding-down bolts, there are a good many other methods which may be used instead of the little pot insulators shown in the drawing. The insulators seem to work quite well, but, at the same time, it is an easy matter to design something a good deal better. The man must be protected, and it does not seem to be an insuperable difficulty to insulate him properly.

With regard to the commutator difficulty, I do not think a large low-tension D.C. traction generator gives very much difficulty. I have seen most of M. Thury's machines of various ages running; some have run ten or eleven years without any difficulty, and I think the high-tension direct-current machine will work just as well as the low-tension direct-current machine.

I thank you very much for the kind attention you have given to this paper; and, if you will allow me, I would like to say one word more, namely, that I am sure we all feel extremely honoured that Lord Kelvin has favoured our meeting with his presence. We all heard with great delight what he had to say on the direct-current system, and I think that by his presence alone here he teaches us a very much greater lesson than anything he can say technically, a lesson in magnificent constancy of purpose which we can all do our best to learn.

Communicated: Several speakers referred to the fact that large rises of pressure took place on direct-current circuits. Clearly it must be the case that any sudden alteration to the current in a closed circuit must lead to corresponding variations in pressure, and under similar conditions the rise in pressure will be as severe as in any other circuit carrying current.

The great distinction between the series system and the ordinary parallel systems is that in the ordinary everyday operations of switching nothing is done in connection with the former system that is likely to cause these sudden variations, whereas in parallel alternating work every time a machine is started up or shut down some degree of pressure rise will be caused, and a small fault on the part of the attendant may lead to serious trouble.

Several speakers also referred to the different effects of alternate and direct current on insulation, particularly in connection with underground cables. M. Thury carried out a large number of tests by taking different makes of cables and dividing them into two,

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testing one half on alternating current and one half on direct current. An immediate difficulty was, however, experienced, owing to the fact that with direct-current pressures up to something less than 100,000 volts it was found impossible to break down almost every class of paper cable where the thickness of the di-electric exceeded 2 mm. As I have explained, the tests were made with a special machine having a smooth core rotor, as it was found that when tests were made by taking energy from the ordinary town supply with several machines in parallel, the ratio between alternate and direct current was very much greater than 2 to 1—in fact, the ratio in some cases rose as high as 10 to 1; consequently it was found necessary to use a special machine. There also appears to be a tendency for the ratio between alternate and direct current to increase with increases in the pressure.

Mr. Fasola's data with regard to the Zurich installation are of particular interest. Taking his figures as they stand they show an advantage in 3-phase working. In the Zurich case, however, the line loss appears to be taken at altogether too high a figure, and by taking advantage of the possibility of reducing the current at times of light load probably the figures would be considerably altered. It is impossible within the limits of such a discussion to handle a special case of so great importance as the Zurich system, and, therefore, I do not propose to say any more on this matter, except to point to the fact that the 3-phase system in this case requires three times the total length of conductor and 20,000 more insulators than the direct-current system; and, further, a short circuit on the alternate system affects the whole of the plant, which is not the case with the series system. Also, the difficulties due to lightning are much less with direct current than with alternate current. Therefore it would seem that the reliability of service of the direct-currrent system in such a case is greater than the reliability of the alternate-current system.

The question of grounding the direct-current system is one of some difficulty, and my own impression is that when possible it is very much better to work with a completely insulated system, as in this way one ground will cause no trouble.

Mr. Semenza's communication seems to me to summarise the case for and against the series system in a most admirable way. One of the objections, as he states, is the use of small units, but I hope in the future it will not be impossible to overcome this difficulty. I am bound to say I was disappointed with Mr. Hobart, as, from the article in the *Electrician* to which I made reference in my paper, I hoped that he would show us a way out of the difficulty of building high-speed direct-current machines. I do not despair myself, as very good progress has been made in the design of direct-current machines up to the present time, and perhaps partly by reducing the speed of turbines and partly by the clever design of direct-current machinery it will, in the future, be possible to make satisfactorily running series machines.

Mr. Tilney, in referring to the difficulty of the holding power of the

direct current arc, criticises the Lausanne pole line. There is no doubt that greater security would be achieved if the two wires were carried on separate poles, but, as I have explained above, probably in the future this line will be put entirely underground, as really it is impossible to obtain the same security with an overhead line as with an underground line. Mr. Tilney's remarks as to surges of current exactly agree with what I have said above. Mr. Highfield.

I am glad that Mr. Allen confirms my opinion that the series system is of particular service where the power is supplied by gas engines, for the reasons he states. In this connection Mr. Patchell has somewhat misunderstood me. I do not, of course, suggest that the gas engine gives constant torque through one revolution, but only that it was a machine giving under certain conditions constant average torque, which, of course, is not the same thing. I am sure, however, that the speed reduction that would be possible would not exceed 30 to 40 per cent. of full speed, and this reduction would only be possible at comparatively long intervals; therefore, the regulation in connection with gas plant would in all probability be done by moving the brushes.

The writing of this paper was to me a great pleasure, and I am gratified that so many engineers have taken an interest in this subject, which to me appears to be one of very great importance.

On the motion of the Chairman a vote of thanks was unanimously passed to Mr. Highfield for his paper and carried with acclamation.

Proceedings of the Four Hundred and Fifty-fourth Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, March 14, 1907—Mr. W. M. Mordey (Vice-President) in the chair.

The minutes of the Ordinary General Meeting held on March 7, 1907, were taken as read and confirmed.

Messrs. W. H. Molesworth and F. C. Polden were appointed scrutineers of the ballot for the election of new members, and, at the end of the meeting, the following were declared to have been duly elected :—

ELECTIONS.

As Associate Members.

Horace Marler Anning.	Ernest Alexander Laidlaw.
Ernest Alexander Fella.	James Frederick Loader.
Henry Francis Joel.	Albert Pickersgill.

[redacted]

Donations to the *Library* were announced as having been received since the last meeting from Dr. A. Muirhead, W. H. Patchell, and Professor S. P. Thompson, to whom the thanks of the meeting were duly accorded.

The discussion on Mr. Highfield's paper was concluded (see p. 512), and the meeting adjourned at 9.45 p.m.

MANCHESTER LOCAL SECTION.

MAGNETIC LEAKAGE AND ITS EFFECT IN ELECTRICAL DESIGN.

By WILLIAM CRAMP, Associate Member.

(*Paper read January* 8, 1907.)

In every electromagnetic machine there are coils, generally called field coils, whose function it is to set up a magnetic flux; and other coils so placed that they may link with as much of this flux as possible. Very rarely is it possible to arrange the machine so that all the flux from the field coils links with all the coils placed to receive it. Generally the ratio of the flux set up by the first coils to that received by the second coils is called the coefficient of leakage.

Hopkinson used his leakage coefficient to denote the ratio of the number of C.G.S. lines set up in the field magnets (of direct-current machines) to the number of C.G.S. lines which, linked with the armature-turns, produced the machine E.M.F., *i.e.*—

$$\begin{array}{c}\text{Leakage coefficient}\\ \text{(Hopkinson)}\end{array} = \frac{\text{No. of lines in field magnets}}{\text{No. of lines in armature}}.$$

Professor Thompson has used the term "coefficient of dispersion," to replace what I have called above "leakage coefficient," but in general I shall adhere to the term "leakage factor" as being shorter and more expressive. In any case, to me, the terms "leakage factor," "leakage coefficient," and "dispersion coefficient," mean one and the same thing.

Now the total flux generated by the field magnets = useful flux + stray flux;

Hence dispersion coefficient or leakage factor

$$= \frac{\text{useful flux} + \text{stray flux}}{\text{useful flux}}$$

$$= 1 + \frac{\text{stray flux}}{\text{useful flux}}.$$

With given magnetising ampere-turns the stray flux is proportional to the permeance of the leakage paths, and the useful flux is propor-

tional approximately to the permeance of the armature teeth and air-gap. Thus the dispersion coefficient—

$$= 1 + \frac{\text{permeance of leakage paths}}{\text{permeance of armature teeth and gap}},$$

or—

$$1 + \frac{\text{reluctance of armature teeth and gap}}{\text{reluctance of leakage paths}}.$$

I have put the above down at some length because I wish to make this leakage coefficient perfectly definite; and to take care that this term means the same thing whether alternating- or continuous-current machines are referred to. In alternating-current motors particularly it is of the greatest importance.

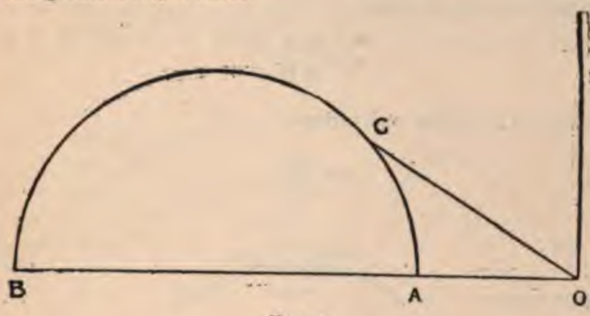


FIG. 1.

In the Heyland circle for an induction motor such as Fig. 1, the coefficient of dispersion is sometimes taken as—

$$1 + \frac{OA}{AB}$$

sometimes as—

$$1 + \frac{OA}{OB},$$

while, unfortunately, some writers even go so far as to call

$$\frac{OA}{AB} \text{ or } \frac{OA}{OB}$$

the coefficient of dispersion.

Now neglecting stator resistance, OB represents the short-circuit current of the motor at a certain voltage, while OA represents the stator current at the same voltage when the motor is stationary with rotor open-circuited; and since under these conditions the flux linked with the stator-turns is constant, it follows that OB represents the ampere-turns to drive a certain flux across the leakage paths, whilst OA represents the ampere-turns required to drive the same flux through the useful paths, leakage at no load being neglected. Thus OB represent

the reluctance of the leakage paths, while OA represents the reluctance of the useful paths.

Our dispersion coefficient or leakage factor, therefore, is really—

$$1 + \frac{OA}{OB}, \text{ not } 1 + \frac{OA}{AB}.$$

This reasoning, of course, applies to all electromagnetic machines.

Among writers upon induction motors the letter σ has been adopted to represent another leakage ratio. Behn-Eschenburg uses—

$$\sigma = \frac{OA}{OB},$$

whilst Hobart and others take—

$$\sigma = \frac{OA}{AB}.$$

I shall adhere to the latter and write—

$$\sigma = \frac{OA}{AB}$$

and

$$v = \frac{OA}{OB},$$

thus leakage factor—

$$= 1 + \frac{OA}{OB} = 1 + v$$

$$= 1 + \frac{OA}{[illegible]} = 1 + \frac{\sigma}{[illegible]}.$$

have specially arranged leakage paths to increase self-induction for constant-current purposes, etc.

Thus it will be necessary to distinguish between "harmful leakage" and "useful leakage," and to consider continuous- and alternating-current machines separately. It will be granted that the subject of harmful leakage has received much more attention than that of useful leakage. This was bound to be the case since the designer is forced to take into consideration those factors which tend to prevent a given machine from fulfilling its special purpose. It is to the useful leakage that I wish to draw in certain cases special attention in this paper. I shall commence, however, with harmful leakage in the field systems of continuous-current machines; and at the outset it is clear from the definition given that the coefficient of leakage will increase with the length of the air-gap, and with the reduction of the dimensions A and B in Fig. 2. So while it is true that magnetic leakage does

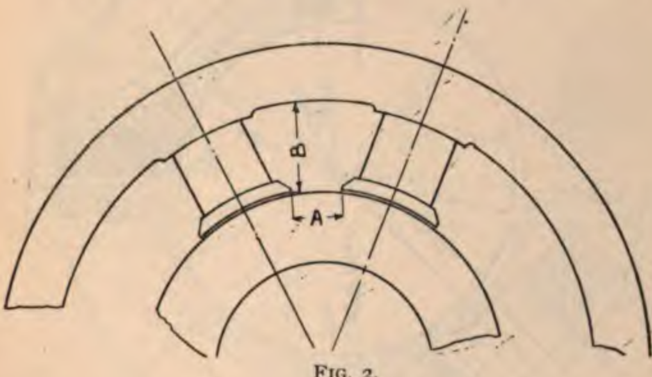


FIG. 2.

not represent a definite loss of power, it is also true that any machine whose leakage can be got rid of will have a distinct increase in output for a given speed; or a decrease in weight for a given output, efficiency, and speed. It is therefore of importance that the leakage should be reduced as far as possible, as may be seen from the following reasoning.

A machine having a large magnetic leakage will have either larger field poles, or a higher magnetic density in those poles than a machine having no leakage, so that the amount of field copper in the case of the machine with leakage is greater for a given field loss.

Or conversely, with constant flux in the field magnet, a machine with large leakage will require more armature copper than the same machine with no leakage. Thus for a given efficiency magnetic leakage increases the cost of the machine.

Now from whichever point of view we approach the subject, *i.e.*, whether we reduce the armature copper or the field copper by reducing the leakage, we shall arrive at practically the same change in cost, as is easily seen from a few trial cases. As a matter of fact, however, it is

almost impossible by alteration of the armature alone to change the leakage to any extent. Hence we return to the question of alteration of field shape.

We note, in the first place, from what has just been said concerning the dimensions A and B in Fig. 2, that a small magnetic leakage coefficient can be obtained by abnormal increase of armature diameter coupled with decrease of ratio $\frac{\text{pole arc}}{\text{pole pitch}}$ and lengthened field pole; which conditions also involve large diameter of field frame. But

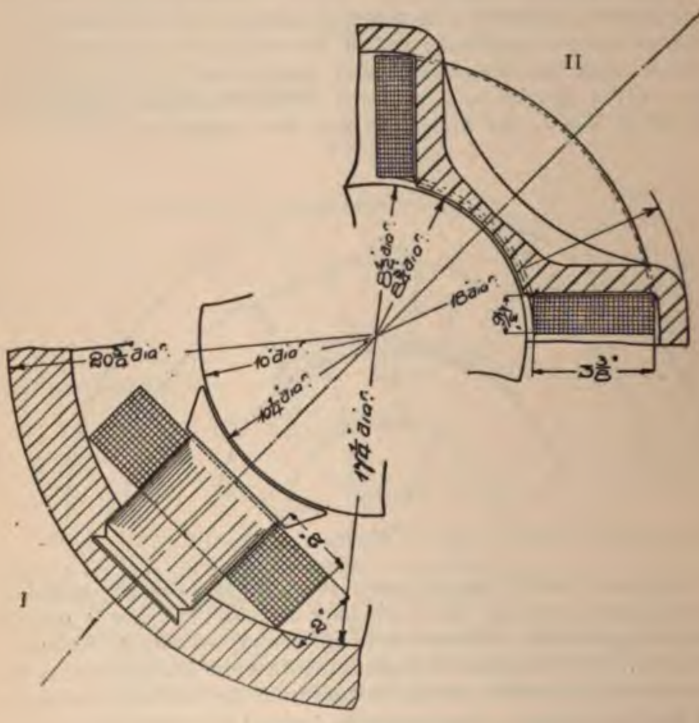


FIG. 3.

reduction of the coefficient by this means only results in a more expensive machine, supposing that the original linear dimensions were settled by sparking and temperature considerations. Further, it is only in certain cases that any improvement at all can be expected. For, consider the following values of the leakage coefficient.

In multipolar machines, with normal ratio of pole arc : pole pitch (that is, an average of 0·72) the value for the coefficient varies between 1·5 and 1·15 from 1 to 200 k.w. Beyond 200 k.w. the coefficient steadily decreases till we reach 1·1 at 1,000 k.w. It is evident, then, that

beyond 200 k.w. the decrease of cost to be obtained by reduction of the dispersion is very small.

Let us take a small size of machine, say, 5 k.w., and see what improvement can be effected by reducing the leakage. Fig. 3 I shows the field-magnet circuit of a machine having 4 poles, which I designed some time ago to give 5 k.w. at 1,000 r.p.m. The dispersion coefficient is about 1·35. At high saturations it might become 1·4, and at this figure we will take it. The weight of field copper per spool is 8·22 lbs. and its cross-section 2 ins. by 2 ins.

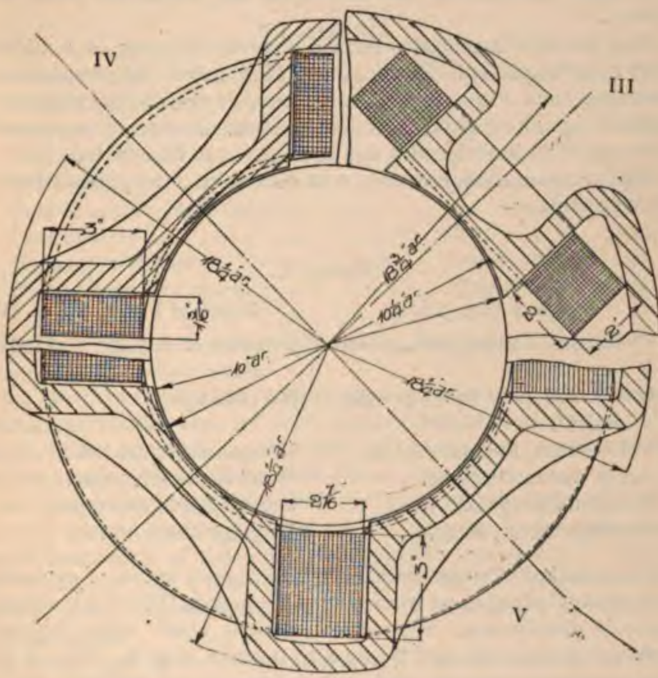


FIG. 4.

If the leakage could be reduced to 1·10, the pole area, which is now 9·6 sq. ins. would become 7·55 sq. ins., and its diameter would be reduced from 3·5 ins. to 3·10 ins. This reduces the field copper to 7·77 lbs. and the cost of the total copper by about 6 per cent., a reduction hardly worth making for its own sake unless other advantages ensue. The question is : Can this leakage be decreased without proportionately increasing the amount of field copper or the cost of the frame? In answering this question we notice that to reduce the leakage we must either—

1. Increase the length of the leakage paths, A and B, Fig. 2, or
2. Decrease their area.

In considering the first expedient we have already determined that to apply the remedy of a larger field frame is inadmissible. Examination of Fig. 3 I shows us at once that the main leakage will take place from pole tip to pole tip. There will be also considerable leakage from pole to pole and also from tip to yoke.

Now the leakage path from tip to tip can be reduced by placing the magnetising coil nearer the armature, *i.e.*, around the tip itself, and the area of the path between pole and pole can be reduced by shortening the pole, while the area of the path (and its length) can be changed by bringing the sides of the coils upon the respective poles nearer together.

Thus the ideal machine to reduce magnetic leakage to a minimum would be arranged as in Fig. 4, either III or, better, IV; but in making the change from Fig 3 to Fig. 4 we have considerably increased the length of mean turn of the field coil, and some method of compensating for this must be introduced if any economy is to be effected.

This compensation, however, is at once seen to be present from the following table :—

TABLE I.

Design of Fig. 3 I.	*Design of* Fig. 4 III or IV.
(*a*) Cylindrical cooling surface—A.	Cylindrical cooling surface quite 2A.
(*b*) Laminated pole tip to provide and fit.	No pole tip.
(*c*) Pole to machine and cast in.	No machining on pole.
(*d*) Large diameter frame.	Small diameter frame.
(*e*) Commutation good.	Commutation excellent.
(*f*) Leakage factor, large.	Leakage factor, small.

The question of commutation requires a word or two of explanation. Good commutation, apart from special devices, entails steady interpolar magnetic distribution. Such distribution is much more marked in machines of types III and IV than in those of type I. This is partly to be accounted for by the decrease in leakage and partly by the close proximity of the field coil to the armature.

We can therefore take advantage of the new construction in five ways:—

1. Increase the field loss *a*, thus bringing the weight of copper to the original value and saving, if possible, on *b*, *c*, *d*; this is inadvisable owing to the necessarily low rating when enclosed.
2. Let the weight of copper remain, and save if, possible, on *b*, *c*, *d*.
3. As the whole machine is likely to keep much cooler, the armature dimensions may be reduced and a saving effected there without practically affecting the efficiency.
4. While, if possible, saving, as in 2 above, increase the overload capacity on account of *e*.

5. Decrease the armature turns in ratio of old to new leakage factor; increase section per turn and thereby the output.

In applying these possibilities in practice, it has been assumed that the field loss remains about constant, while the length of mean turn is increased from 17·6 ins. to 28 ins. Since the resistance of mean turn must be kept constant if the voltage per coil and ampere-turns are constant, it follows that a wire of slightly larger diameter is used in the new form. The comparison of one coil of Figs. 3 I and 4 IV is as follows:—

TABLE II.

	Fig. 3, I.	Fig. 4, IV.
Ampere-turns	1,725	1,725
Length mean turn	17·6 ins.	28 ins.
Current	0·575	0·62
Watts	115	124
Yoke diameter	20¾ ins.	18¼ ins.
Cost of copper (four coils)	36s.	52s.
Cost of steel	16·4s.	13·4s.
Leakage coefficient	1·35	1·1
Output for given speed	5·00	6·00

NOTE.—Fig. 3 has a cast-iron yoke. This in the above table has been reduced to cast steel.

So, then, the alteration results in a machine of small diameter costing specifically less, yet with better commutation, slightly larger output, better overload capacity both on account of the commutation and the smaller distortion because of the pole shape.

In the above comparison little or nothing has been said of the reduced cost of machining Fig. 4 IV, because it may be argued that if lamination of the pole tip is good for the one it is necessary for the other. Fig. 4 is at least as good as Fig. 3 I, from the enclosing point of view, since its radiation is so much better and its field loss so little increased.

Now in making the above comparison I have changed the original machine without reference to the fact that in so doing we have missed the best proportions for the new machine. The original machine had a round pole which is the best shape. "Round" being out of the question for the new machine, we shall adopt "square" as the next best shape. Keeping, then, pole area and flux per pole constant, we reduce the armature diameter to 8½ ins. and alter its length to 4·7 ins. instead of 4 ins. The cooling surface is therefore almost the same while the field length of mean turn becomes 26 ins. instead of 28 ins. All this is shown in Fig. 3 II. Thus a comparison of Figs. 3 I and II yields the following table:—

TABLE III.

	Fig. 3 I.	Fig. 3 II.
Ampere-turns	1,725	1,725
Length of mean turn	17·6 ins.	26 ins.
Field-watts per cent.	2·3	2·3
Yoke diameter	20·75 ins.	18 ins.
Yoke cost	16·4s.	12·5s.
Field copper cost	36s.	48s.
Total cost (yoke and field copper)	52·4s.	60·5s.
Leakage factor (est.)	1·35	1·1
Output (k.w.)...	5·00	6·00
Shillings per kilowatt	10·5s.	10·08s.

Note that in the above contrast the costs of the two armatures are the same, so that the cost per kilowatt is really far more favourable to Fig. 3 II than appears. From this comparison it is evident that even without considering the reduced cost of machining, Fig. 3 II is the better machine.

These principles should, I think, be applied in machines up to about 20 B.H.P. Beyond this size the ratio of the cost of active material to labour, together with the small decrease of the leakage coefficient, render the advantages to be gained more doubtful.

Now Fig. 4 immediately suggests the possibility of advantages to be gained by replacing the four original coils by one of zigzag shape. This is an important point, the more so as several patents have recently been taken out in this direction. That the idea is not new is evident from a perusal of patents No. 7079 of 1887, and No. 3970 of 1892. The newer patents are No. 6666 of 1902, No. 21202 of 1902, and No. 9604 of 1903, and they all have for their object what is termed "magnetisation of the armature" as against the usual plan of magnetisation of the field magnet.

The clear meaning of this is that the inventors have aimed either at abolishing magnetic leakage, or else magnetising the armature, so that the armature itself carries a greater flux than the field poles. This might easily be the case if there is any considerable gap between the wire of the coils and the poles themselves. It also incidentally leads us to discuss the whole question of single field-coil machines.

In approaching this subject we must realise first that the watts dissipated in any field depend upon the ampere-turns per pole required, the resistance of the mean turn, the number of poles, and the field current. With given voltage across the field and given watts to be dissipated, the field current is fixed, and, of course, the ampere-turns per pole are usually also fixed beforehand. Thus the product of the resistance of the mean turn per pole, and the number of poles, will in such a case be a constant, and the ampere-turns of the single coil will always have to be equal to those required for one complete magnetic circuit, *i.e.*, twice the ampere-turns per pole. Consequently, as a basis of comparison, if we can show that the length of the mean turn of the single coil is less than the product of half the number of poles and

length of mean turn per pole in the ordinary multipolar case, then it will pay to use the single coil, unless indeed it be of such an exceptional shape that the expense of winding it places it out of the question.

In the above machines, Fig. 4, if we substitute a single coil for the four existing coils, it must be of the shape shown in Fig. 5 approximately, and since the centre of its section must correspond with the neutral point between the poles, it follows that its length of mean turn will be 62 ins. as compared with 2×26 ins. or 52 ins. of Fig. 3 II. It will be obvious that the greater the number of poles, and the shorter the length of the armature, the more likely is the single-coil machine to compete with one with as many coils as poles; but it can rarely be worth while to add poles for the sake of using the single coil. From this it follows that the original Lundell motor is the very worst form of machine for a single field-coil machine, as it only has two

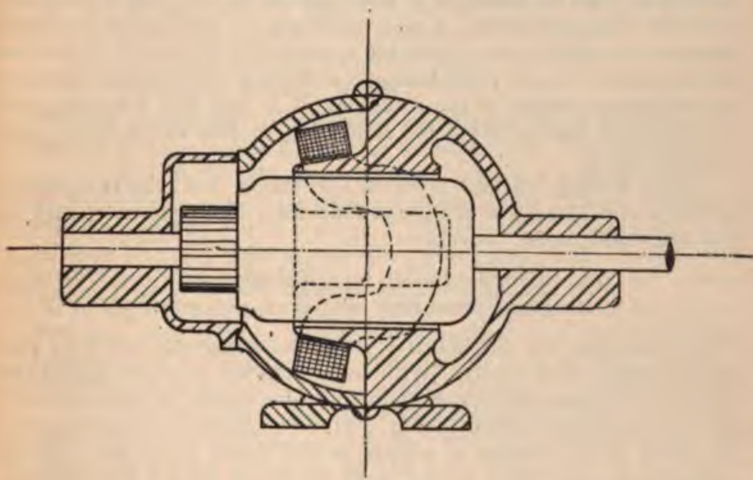


FIG. 5.

poles; and it is further at a disadvantage in that the axis of the coil is inclined to the armature in such a way as to set up great magnetic leakage along the shaft and bearings. It is a proof of how much the price of small motors depends upon machining (and not active material) that the Lundell motor could ever compete against bipolar machines with ordinary field coils.

A better shape of coil than that employed in the Lundell motor would be the bipolar form of Fig. 5; but this, though it is used by one firm, can hardly be economical since the ends have to be bent so far out of their shortest path to clear the armature top and bottom, thus resulting in an abnormal length of mean turn far greater than that of the ordinary bipolar motor. It is a fact, however, that so excellent is the commutation produced by field coils which lie midway between the pole tips, while closely enveloping the armature, and so much is

the leakage coefficient reduced thereby, that machines far smaller than those of ordinary construction can be built for a given output. This has been already proved in Table III. There is little doubt that the excellent commutation is in all these cases due to the fact that the position of the field coil renders definite the interpolar field; while the fact that the yoke comes much nearer the armature (which would in itself, as a rule, render commutation difficult), is discounted also by the position of the coil. Indeed, it is not too much to say, that by the use of coils, whether single or double, passing through the interpolar space, an effect can be obtained approaching that produced by special commutating poles. The field shape, of course, is then not unlike an ordinary slotted stator, with as many slots as poles.

As regards the question of the nearness of the yoke upsetting good commutation, I have found that in certain machines of the bipolar Lahmeyer type, the nearness of the armature to the yoke upset commutation upon overloads. A cure was found for the trouble by fitting from pole to pole in the interpolar space and quite close to the armature a sheet of copper; this having the effect of considerably reducing the reactance voltage of the machine. It is said that the field coils of single-coil machines possess something of this effect, but I very much doubt it.

I have already touched upon the question of the relative economy of single-coil as against multi-coil machines. The question turns (as has been said) upon the ratio—

$$\frac{\text{Length of mean turn of single coil}}{\text{Length of mean turn per pole} \times \text{pole pairs}}.$$

With two poles the single coil is never advantageous, and with four and six poles, very rarely. With more than six poles the single coil may pay, but, generally speaking, the machine easiest to wind and most economical is that shown in Fig. 3 II. The single-coil machine can be made as economical of copper as this latter form, but only by cutting away alternately on the right hand and on the left the poles of the machine (Fig. 5). This resolves itself into slightly staggered poles with a shoe alternately to right and left, which by increasing the leakage factor does away with some of the advantages of the type. It must be remembered, however, that the provision of shoes upon the sides of the poles is a very different matter to the use of ordinary pole horns or tips, since it does not increase the leakage factor to the same extent.

In the above analysis I have tried to be as fair as possible to all shapes of machine by taking the very best of each class. In this respect, then, I think the figures I give form a much more reasonable basis for comparison than those used by H. F. Joel * in comparing a cast-steel 6-B.H.P. single-coil machine of very careful and economical design with a 4-B.H.P. cast-steel ordinary 4-pole motor of such design that a first glance condemns it as hopelessly bad. Naturally the com-

* *Electrical Review*, vol. 55, 1904, pp. 203, 273.

parison is much more favourable to the single-coil machine in his case than under the conditions adopted by me.

Summarising now the results so far attained :—

1. In machines up to 20 H.P. the leakage is sufficiently great to make it worth while to adopt special means to reduce it.
2. One of the best remedies is the placing of the field coils close to the armature and practically spanning the pole pitch.
3. A single coil may be adopted in place of the usual one coil per pole, but this is not as a rule economical.
4. The single coil will be more economical than the separate coils if the length of mean turn of the single coil is less than the length of mean turn of any one of the separate coils multiplied by half the number of poles.
5. Adopting either of these methods of field winding incidentally reduces both the weight of iron and the cost of the machine, and at the same time improves the commutation.
6. As the general result, therefore, smaller machines at a reduced cost can be built for the same output.

Having shown that considerable advantages are to be gained by surrounding the armature closely by its fields coils, the logical deduction is that if both field and armature coils are placed upon the armature the results will be better still, since any harmful leakage is then converted into useful leakage. This system, of course, would do away with the field coils proper altogether, and would resolve itself into the equivalent of an armature and commutator with the brushes set at an angle to the neutral line, which arrangement in the case of the series motor might be adopted ; but in the case of the shunt motor we should need two commutators and two sets of coils, with the "field brushes" at right angles to the "armature brushes." Considering first the case of the shunt machine, and remembering that the full voltage must be put across the field brushes, and that the field current will be limited only by the resistance of the coils connected to that commutator, we are at once confronted with the fact that the number of field-turns would render such a winding very costly compared with the simple field coils of an ordinary dynamo. Hence, from ordinary commercial considerations, this variety must be considered to be ruled out of the question.

The case of the series motor is different. Here the field-turns will only be about the same as the armature-turns, so that it is worth while going into the question to see if any substantial advantage may be gained. From the theoretical point of view, we have two factors to consider :—

1. The field coils being distributed about the circumference of the armature will not have along the field-brush axis the effect of a concentrated coil producing the same ampere-turns, but only about $\frac{2}{\pi}$ times this value.

2. The leakage factor will, so far as mutual induction between the armature and field coils is concerned, be practically unity.

It is evident that the first consideration to some extent discounts the second, so that we shall not expect to find any value in this type of machine except in special cases. For having placed both field and armature coils on the same member, we can make the yoke and poles either exceedingly cheap or exceedingly light. If cheapness is desired, a simple iron casting bored out is all that is necessary; if lightness is desired, cast steel may be used, and in either case the magnetic circuit is economical because very short, so that the overall diameter of the machine will, if the armature dimensions are not much increased, be much reduced by the new construction.

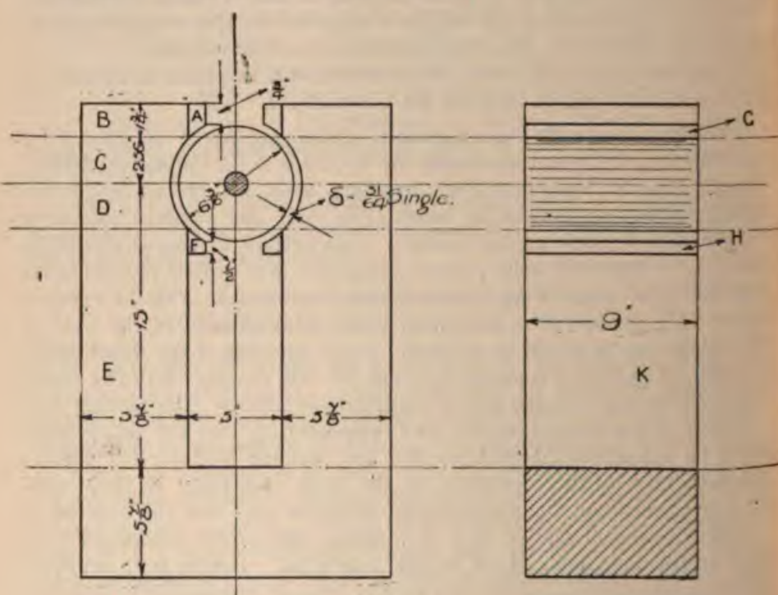


FIG. 6.

In order to put these theories to a practical test, I have, with the assistance of Mr. Garner, carried out at the Municipal School of Technology a series of experiments upon a bipolar double-commutator machine, a diagram of whose frame is given in Fig. 6.

The following are particulars :—

Machine used.—Motor transformer No. 6,616, made by the Electric Construction Company.

Type.—Elwell Parker. Bipolar overtype drum (smooth).

Armature.—High-tension (H.T.) side, 220 volts, 280 conductors. Low-tension (L.T.) side, 20 volts, 28 conductors.

Field Winding. — Each limb 27 layers, 152 turns per layer, in 0·052 ins. diameter wire. Resistance, 45 ohms. Total, 90 ohms hot.

Output.—2 kilowatts.

Leakage Coefficient.—Calculated by Forbes' Lemmae, 1·51.

It will be seen that though from the point of view of leakage, this is an excellent example, yet it is difficult to obtain upon the armature a number of ampere-turns corresponding to that which would normally be upon the field.

The first point of importance is the fact that while the leakage coefficient as calculated is 1·51 we can hardly expect to change this to unity, since part of the leakage is due to the shape of the field, and leakage will take place even when exciting from the armature; also on account of the abnormally low saturations in the test, the coefficient would be reduced.

The second point is the importance of setting the field and armature brushes in such relative positions that the effect of lack of symmetry in the pole horns, etc., is entirely eliminated. This was accomplished as follows :—

The machine was driven at constant speed by a small motor, and no excitation being on, both H.T. and L.T. brushes were moved till they gave their maximum volts (due to residual field magnetism) so that both were upon the neutral line. Since the H.T. brushes were to be used for the field excitation, the L.T. armature brushes were first fixed by choosing such a position that no matter what current was passed through them, the generated volts did not increase, *i.e.*, there was no resultant field at right angles to the L.T. brushes due to their own current. The H.T. brushes were then moved round till they were at right angles to that position where they would give a voltage ratio = ratio of turns as compared with the L.T. side.

The results which I am about to quote are those of the third series taken. The first two series were each found to contain errors due to lack of field symmetry or other causes which gradually showed themselves as the work advanced.

The normal maximum field ampere-turns are 18,300. This would entail a H.T. current of 260 amperes. The maximum current we could use was 35 amperes, and this corresponds to 2,450 ampere-turns. Thus the saturation was much lower than usual, and an examination of the magnetisation curve showed that we never reached the knee.

The argument used was as follows: The ampere-turns of the H.T. armature acting along the normal field axis will be about $\frac{2}{\pi}$ times those of a concentrated field coil having the same current and turns, but placed centrally around the armature between the poles.

I. If, then, we excite the H.T. armature with a certain current and apply a voltage to the L.T. side, the machine will run as a motor, and at a fixed speed the L.T. back volts will be proportional to the flux set up by the field ampere-turns.

Now, leaving the H.T. armature unexcited, providing on the field coils ampere-turns $\frac{2}{\pi}$ times those of the armature, and measuring the L.T. volts at similar speeds, will give us the corresponding flux set up by a corresponding number of field ampere-turns, and the ratio of the L.T. volts in the two cases will be the change in the leakage factor produced by the change of position of the exciting coils, provided always that the above ratio $\left(\frac{2}{\pi}\right)$ is correct.

II. Similarly, if instead of using the L.T. armature as a motor armature, we, with similar excitation conditions, drive the machine independently, then the direct ratio of voltages on the L.T. side is the change of leakage factor.

The results of Test I., as above, are given in Tables IV. and V., and the results of Tests II. in Tables VI. and VII. The only possible source of error is change of H.T. carbon brush resistance, which practically does not affect the results. (The L.T. brushes were of gauze). Some of the results of Test II. are plotted in Fig. 7, and these show the equivalent of a change in the leakage factor from 1·42 to 1·24.

Table IV.

Conditions.—Excitation provided by H.T. winding, L.T. winding as motor armature.

I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.
	H.T.	L.T.	H.T.	L.T.	L.T.	H.T.	H.T.	L.T.	
Speed.	Volts (applied).	Armature Volts.	Exciting Current.	Armature Current.	Armature C.R. Drop.	C.R. Drop.	Back Volts.	Back Volts.	Mean Temperature for Windings.
168	17·30	1·50	35 amperes.	158	1·045	18·7 volts.	1·40	0·455	126° F.
190	17·20	1·64		169	1·118		1·50	0·522	
300	16·75	1·94		184	1·218		1·95	0·722	
400	15·65	2·25		198	1·310		3·05	0·940	
530	14·40	2·52		204	1·350		4·30	1·170	
740	12·05	2·91		214	1·416		6·65	1·404	
1,000	10·10	3·39		218	1·440		8·60	1·950	
1,220	8·56	3·80		222	1·468		10·14	2·332	
1,640	5·55	4·56		226	1·495		13·15	3·065	

Low-tension armature resistance $=$ 0·0066 ohms.
High-tension armature resistance $=$ 0·535 ohms.

Table V.

Conditions.—Constant exciting current in main field winding. Motoring from L.T. winding.

I.	II.	III.	IV.	V.	VI.	VII.	VIII.
Speed.	L.T. Armature Volts.	H.T. Generated Volts.	C.R. Drop in L.T. Armature.	L.T. Armature Current.	L.T. Back Volts.	Field Current.	Temperature (average).
170	0·678	0·155	0·206	35·0	0·402	0·190 amperes.	Field windings, 66° F. Armature do. 80° F.
192	0·745	0·174	0·212	36·0	0·533		
240	0·825	0·220	0·224	38·0	0·601		
330	0·970	0·330	0·236	40·0	0·734		
340	0·990	0·358	0·242	41·0	0·748		
418	1·170	0·460	0·259	44·0	0·961		
565	1·370	0·678	0·268	45·5	1·102		
650	1·520	0·838	0·280	47·5	1·240		
800	1·830	1·076	0·292	49·5	1·538		
920	2·050	1·310	0·300	51·0	1·750		
1,060	2·230	1·650	0·312	53·0	1·918		
1,200	2·460	1·950	0·324	55·0	2·136		
1,250	2·520	2·050	0·327	55·5	2·193		
1,400	2·830	2·550	0·348	59·0	2·482		
1,520	3·100	3·020	0·362	61·5	2·738		
1,760	3·530	4·050	0·406	69·0	3·124		

Table VI.

Conditions.—Varying excitations provided by H.T. winding. Machine independently driven at constant speed.

I.	II.	III.	IV.	V.
Speed.	H.T. Exciting Current.	H.T. Applied Volts.	L.T. Armature Volts.	Ampere-turns on H.T. Windings.
740 r.p.m.	3·6	2·71	0·290	80
	7·2	4·50	0·401	161
	9·1	5·40	0·462	203
	11·8	6·67	0·552	263
	16·3	8·65	0·714	363
	19·3	9·92	0·812	430
	22·3	11·42	0·936	497
	31·3	16·40	1·270	697
	33·4	17·50	1·342	745
	35·0	18·20	1·393	782
	37·0	19·20	1·455	825

NOTE.—In this test there is a very wide range of H.T. currents extending from zero to practically twice the full-load current, and since it is on the H.T. side that carbon brushes have been used, it cannot be assumed that the H.T. resistance is constant.

Seeing that the difference between applied volts and C.R. drop is very small and also unimportant (since it is due either to errors in resistance, calculation, or to dissymmetry of the field flux), the calculation of this voltage and the C.R. drop have in this case been neglected

TABLE VII.

Conditions.—Excitation provided by main field winding. Machine driven independently at constant speed.

I.	II.	III.	IV.
Speed.	L.T. Armature Volts.	Field Current.	Ampere-turns on Field.
740 r.p.m.	0·252	0·0212	87·0
	0·340	0·0396	162·0
	0·445	0·0606	249·0
	0·563	0·0796	327·0
	0·685	0·1000	410·0
	0·785	0·1170	470·0
	0·910	0·1370	562·0
	1·075	0·1600	657·0
	1·210	0·1780	731·0
	1·300	0·1960	805·0

It was thought that the change referred to was really unreasonably small and the ratio $\frac{2}{\pi}$ fell under suspicion. To check this Mr. Garner took for me an oscillogram of the flux distribution when exciting from the main fields; and as it was found impossible to obtain a reliable oscillogram when exciting from the armature, the curve for the latter was deduced from the former by the method shown in Fig. 8, where the bars 1, 2, 3, etc., represent the positions of the armature conductor groups. This shows that the ratio $\frac{2}{\pi}$ (0·637) was too large, and that 0·579 is really much nearer the probable truth. Allowing for this the field ampere-turns should have been decreased in this ratio, so causing the leakage factor (and hence the output) to be changed by 21 per cent. instead of 12 per cent. That is, by putting the exciting coils on the armature the output would be increased by these amounts. As a matter of fact, however, this is only a portion of the advantage to be gained, for the weight of the field could then be reduced from 6 cwt. to about 2 cwt.

There are many points of interest about this research which was so well carried out for me. One of the most interesting is the following:—

In Test I., where the low-tension armature current is considerable, an induced voltage naturally makes its appearance at the field brushes. The direction of the voltage is such as to reduce apparently the C.R. drop at the H.T. brush terminals; or, in other words, *the armature cross field becomes in these machines useful leakage* and tends to provide the field current. This is well shown in the decrease of H.T. applied volts with constant current in Column 2, Table III. But it was also observed that when the effect of change of speed is eliminated the *voltage is found to be constant and independent of the armature ampere-turns.*

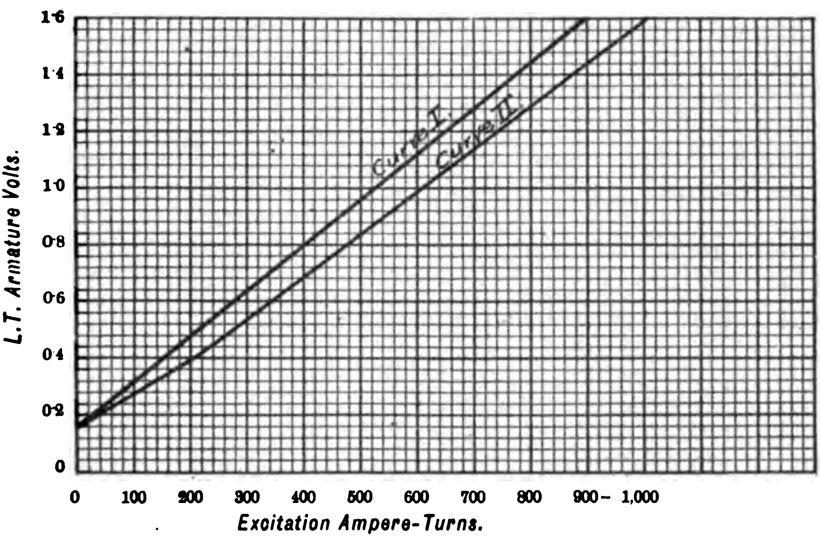
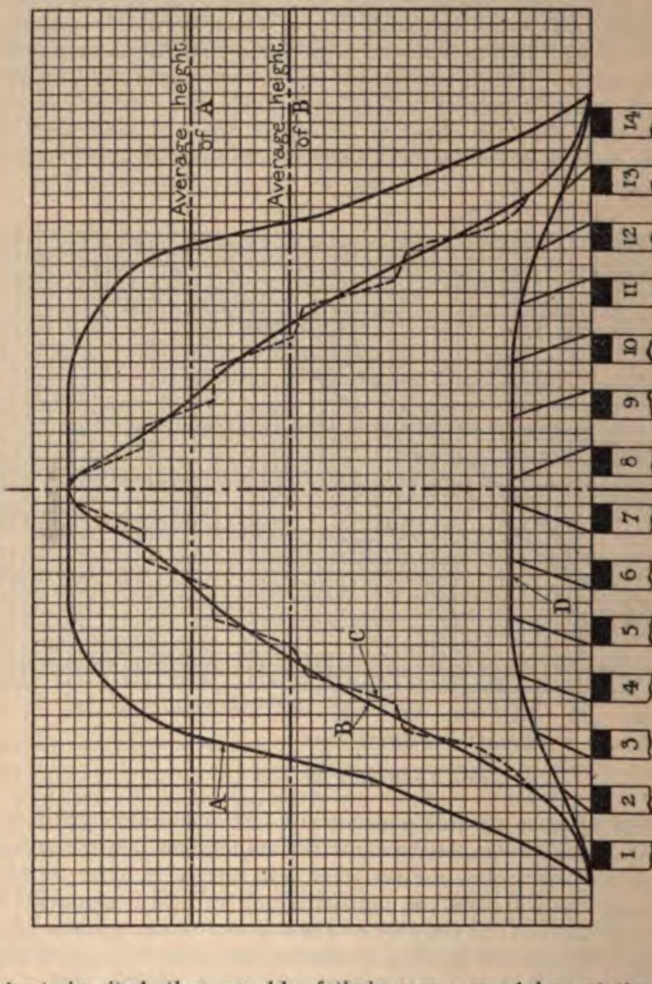


FIG. 7.—Results of Test II.

An exact reason for this phenomenon I am unable to furnish, but I think it can only be explained upon the assumption that increase in armature reaction causes the resultant field always to move through such an angle as will cause the horizontal and vertical components to remain constant. Thus increase of the armature current reacts upon the field ampere-turns tending to distort the main field. And this distortion is such that the changed reluctance of the field path changes also the resultant field till the armature component reaches a fixed value, which depends only upon the field frame shape. That the same conditions do not obtain when excitation is provided by the field is already shown by Fig. 9, where, as would be expected, increase of armature L.T. reaction increases H.T. volts per revolution per minute.

Another point which is full of suggestive interest is the fact that rotation of the armature in the field set up by the armature current produces an E.M.F. at the field brushes *assisting* the field current. This is, of course, equivalent to saying that, if the field brushes were

Flux Distribution Curves.



D

Ratio Average Maximum of B is 0·579.
Ratio Average Maximum of Sine is 0·637.

A.—Field Coil Excitation.
B.—H.T. Armature Excitation.

FIG. 8.

short-circuited, they would of their own accord by rotation of the armature in the armature field set up a field-current magnetising in the proper direction. But this is the very essence of modern compensated alternating-current motors, and to students and teachers alike forms a most valuable starting-point for the study of such machines.

The foregoing tests, though conclusive evidence that in the machine actually tested the saving everywhere by adopting armature and field coils placed on one member is enormous, form hardly a just

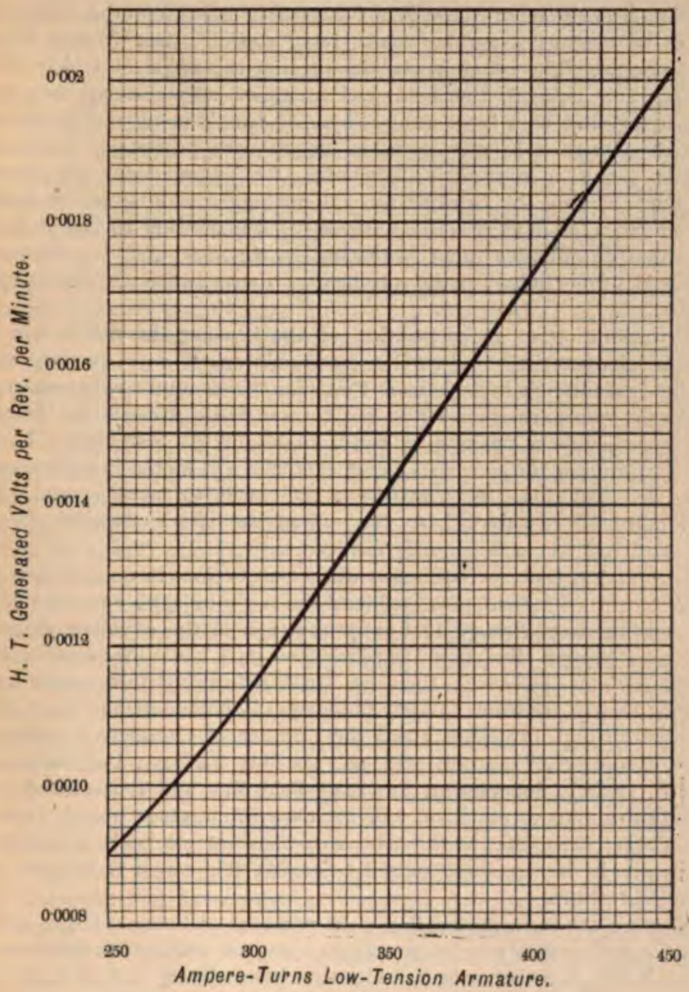


FIG. 9.—Armature Reaction.

criterion. For in fairness it should be stated that the type of machine is old, and though the saving in the case of modern machines might be considerable, it could not be of the same magnitude as in this case. To take, therefore, a more up-to-date instance we may use the machine already shown in Fig. 3 I. If this machine, with a standard armature

winding, be used as a series motor without field coils, then when its brushes are inclined at an angle of 45° to the neutral line, its output is less than one-half of its previous power. There are two points, however, that must not be overlooked; one is the fact that if the machine is designed on these lines for use as a series motor, a different length of armature can be used giving rather better results than those quoted. The other is, that by altering the angle at which the brushes are inclined, the field and armature ampere-turns can be adjusted, giving us a large control by very simple means. The effect of such change is to give variable torque with practically constant speed; for if a sinusoidal distribution be assumed for field and armature, moving the brushes through an angle of $\theta°$ from the field axis changes the field effective ampere-turns according to cos $\theta°$ and the armature effective turns according to sin $\theta°$, so that the torque, which is proportional to the product of these two, is a maximum at $\theta° = 45°$.

Corresponding to every position of the brushes the motor has a definite characteristic; when the brush axis nearly corresponds with the field axis the characteristic is that of a series motor with a strong field and weak armature, while the reverse is the case if the brush axis approaches a position at right angles to the field axis. Thus while maximum torque exists when θ equals 45°, increasing θ beyond this value will cause the armature active turns to increase and the field ampere-turns to fall, so that when the speed remains about constant, the torque is slightly reduced.

In practice, however, such sinusoidal distributions never are found. If, as is usual, the field poles subtend an angle of 135°, upon moving the brushes from $\theta = 45°$ to $\theta = 67°$ the area of the effective air-gap increases though the field ampere-turns fall, and meanwhile the armature-turns increase; thus the total field over this range will remain nearly constant so that the torque (which depends upon the average air-gap flux density and the number of conductors carrying current immersed therein) will not sensibly increase, but the back E.M.F. (which depends on the total field flux and the number of armature active conductors) will increase for a given speed. Over this range, then, the motor acts as a shunt-motor with a variable resistance in its armature circuit. Outside this range it behaves as described above.

It must not be forgotten that such a motor will need a larger commutator and smaller reactance voltage than the ordinary series motor, and this fact, together with the characteristics already given, combine to show that only in special cases will it be of commercial value.

These cases are such as would necessitate a machine having extremely small external dimensions with no room for series rheostat or similar means of control, and an example is the small electric runabout. Even here its use is a little doubtful, because with cells, variable voltage control can be so easily and cheaply arranged. Indeed, for small motors it is hard, in my opinion, to beat Fig. 3 II But very

long motors of very small diameter might be better, if arranged without field coils, for example, tube cleaner motors, etc.

Alternators.—In approaching the question of leakage in alternators, I wish it to be understood from the outset that I intend to confine myself almost entirely to useful leakage, or rather, to showing how use may be made of that leakage which must exist.

Already to some extent this has been done, as, for instance, in the case of the constant-current alternator; which is so constructed that beyond certain points every tendency of the current to increase is checked by that increase itself producing (through leakage flux) a voltage in the armature-turns. Now, in ordinary alternators, the leakage which exists is of two kinds, namely, field leakage and armature leakage. To the former every word that has been said as to the advantage of obviating leakage in continuous-current fields strictly applies. The latter sets up what is called armature reactance, and except in such special cases as the constant-current machine above referred to, must be reduced as far as possible. In setting out to to calculate the reactance of alternator armatures, recourse may be had the formula given in the *Electrician*, vol. 52, p. 1029, 1904, which not only gives good results, but shows clearly what points should receive attention if the leakage is to be reduced to the minimum. When, however, we come to make use of it for calculating the armature details of a machine to give (let us say) constant current, it is found wanting since it only applies to ordinary slot proportions. It is always found difficult to calculate exactly the proper proportions of slot to give a certain reactance, and this is partly due to the fact that the exact effect of the resultant lagging armature current upon the field cannot always be predicted, especially in the case of single-phase machines.

A distinction has been drawn above between field leakage and armature leakage, but in practice these two are very hard to separate. As load comes on a single-phase alternator, armature reactance causes a fall in pressure at the alternator terminals, but it also causes the cyclic variation of armature current to lag in time, so that the relative position of armature and field corresponding to maximum current is changed; with the result that weakening of the field takes place, which causes a further drop in the armature P.D. Thus the action of a constant-current alternator may be just as much due to distortion and destruction of the main field as to armature reactance; but since the path of a continuous magnetic field is a more easy matter to deal with in calculation than armature reactance, I think a better constant-current machine may be designed by providing a definite bye-pass (so to speak) for the field flux, than by attempting to provide leakage paths for the armature flux.

I suggest, therefore, that armature reactance should in every case be kept as small as possible, while reactance of the armature upon the field should be allowed to cause the latter to leak, and thus produce the constant-current effect.

As I shall show presently, the same idea may be adopted to cause

the alternator to be self-compounding, *i.e.*, it may be used either to cause an excessive fall in volts, or to keep them constant, or to cause them to rise. These principles I first made use of in alternators designed in 1904, and from that date to this I have sent out many successful machines constructed upon these lines.

I call attention to this fact because it appears that only in 1906 Mr. Alexander Heyland * has rediscovered and made use of the same idea. The method which I employ is specially applicable to alternators of the inductor type, and may be explained as follows: Fig. 10 is a section

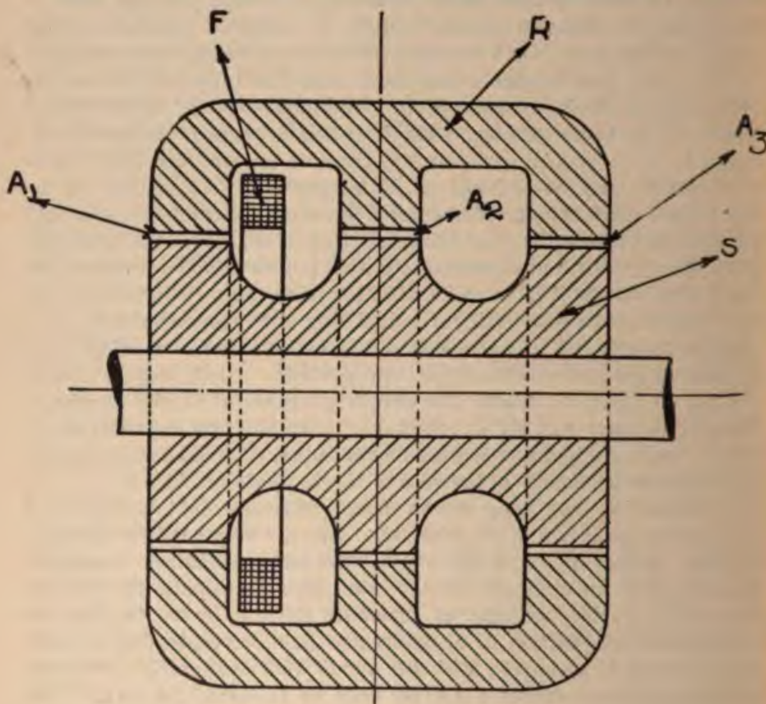


FIG. 10.

through a special inductor alternator. R is the stator or stationary armature forming also part of the magnetic circuit. S is the rotor, and would carry normally the rotating inductors to produce the necessary variations of flux. A_1, A_2, A_3, are air-gaps, and it is evident that if F is the only coil producing the magnetic field, then A_1 and A_2 are magnetically in series, as also are A_1, A_3, whilst A_2 and A_3 are magnetically in parallel with respect to A_1.

Suppose now that the armature coils are wound upon the stator at

* *Elektrotechnische Zeitschrift*, vol. 27, 1906, p. 1,011; *Electrician*, vol. 58, 1906, p. 42.

A_2, while the other two air-gaps remain as simple smooth-bore flux paths. If F be excited by a constant current the coils A_2 will generate a certain open circuit E.M.F. when R rotates at a certain speed, depending upon the relative areas of A_2 and A_3. Directly any current is taken from the coils at A_2, armature reaction is set up which immediately increases the reluctance of the path of the field flux through A_2. In consequence, more of this flux tends to leak by A_3, producing thereby a very large fall of voltage, which will tend to keep the current at A_2 constant. This is particularly the case if the air-gap at A_2 be small and the density high with respect to A_3; so that by carefully adjusting the areas of the gaps and the densities, an alternator may be so constructed that a very small change of current. through the coils at A_2 only takes place, even if the resistance in circuit with these coils is changed from a large value to short circuit. Similarly if the armature coils be placed at A_3, a still more pronounced effect is obtained. Complete regulation and control of this effect is obtained if a second field coil be placed in the recess between A_2 and A_3 and connected in parallel with F. A resistance and reversing switch in series with this second coil will enable all the changes to be produced from an alternator having practically a constant current characteristic to an alternator having the characteristic of a machine intended for ordinary constant potential supply. If the two field coils are adjusted so that both tend to make A_1, A_3 the path of their flux, the effect of the second field coil is equivalent to a great reduction of the reluctance at A_3, while when the two are in opposition they both send their flux through A_2, and tend to prevent any possibility of leakage. Thus in either case the armature at A_2 may be wound as if for constant potential. It will be seen that since the reluctance of each circuit is nearly inversely proportional to the areas of the respective gaps, the calculation for any particular characteristic is extremely simple compared with that required for the ordinary constant-current alternator; and, further, ample adjustment for errors in calculation may easily be provided by means of the second field coil above referred to.

In applying these principles to obtain a machine which shall with varying currents give constant potential difference, we should have to provide at A_3 a steadily increasing magnetomotive force of such sense that it would oppose the leakage from F. This is equivalent to compounding in the ordinary way, and by consequence involves a continuous current increasing with the load. In order to compass this and also to make use of the air-gap A_3, I have placed on the rotor at A_3 a continuous-current armature and commutator, and have broken up A_3 into salient poles.

If the brushes on the commutator are connected through suitable rheostats to the field coil F, we at once have a machine which is self-exciting. Not only this, but the increase of alternating current at A_2 by tending to increase the reluctance of that path, tends also to shunt more flux *viâ* A_3, whereby the voltage at the commutator is increased

and with it the exciting current, so that the voltage at A_2 returns to its original value. In this way the machine becomes both *self-exciting* and

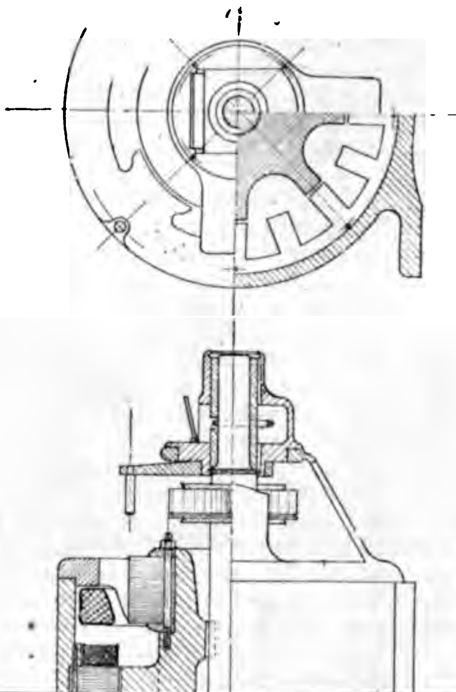


Fig. 11.

factor of the A.C. circuit be reduced, the armature reaction at A_2 is increased, which normally causes a fall of P.D., but in this case it automatically compounds itself by increasing the exciting current. Moreover, the machine may actually be made so that it over compounds, the question of the amount of compounding depending simply upon the saturation of the iron paths at A_2 relatively to those of A_3.

Fig. 11 shows a section through an actual machine constructed upon these principles.

It should be pointed out that when constant current is required, instead of reducing the reluctance at A_3, Fig. 10, we may, if we please, increase the reluctance at A_1. This is very easily carried out if the exciter armature be placed at A_1, for then any increase of current at A_2 increases the reluctance of the whole magnetic circuit sufficiently to

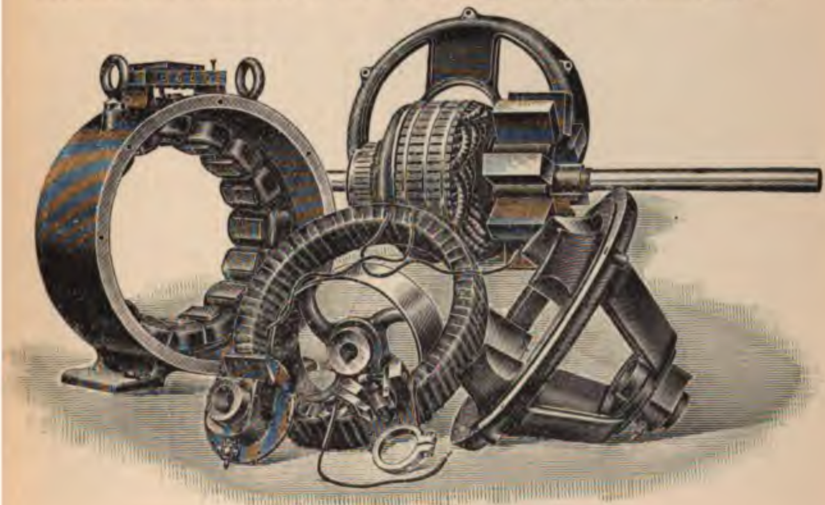


FIG. 12.

drop perceptibly the exciting E.M.F. In this case, then, the air-gap A_3 is not required, but it may be of use in order to give (by allowing a second field coil between A_2 and A_3) a better magnetic distribution at A_2.

These considerations led to the design of the machine depicted in Fig. 12.

Both these types of machine, Figs. 11 and 12, have been made by Messrs. The Crypto Electrical Company, of London; Messrs. Bertram Thomas, of Manchester; and Messrs. Brown-Boveri, of Baden, and about 100 are now in use.

Some curves (Figs. 13, 14, 15, 16) showing how the double-coil type behaves are given here; but since these machines were intended to have a large fall of potential upon load (that is, more like constant-current than constant-potential machines), the exciting armature was placed at

A_{11}, and the principle field coil at F. Thus, while the characteristic from this point of view is good, the magnetic proportions are wrong if constant potential is desired; so that the curves while illustrating the correctness of the principle do not give at all a fair idea as to how far the construction is practicable. Nevertheless the curves, Figs. 14 and 16, show how large is the range of compensation, and it would be a very easy matter to design a constant-potential machine to give still better results. When these machines have been working upon a load possessing considerable capacity, I have seen the exciting watts as low as 30 or about 1 per cent. of the output. It will be observed in Fig. 11

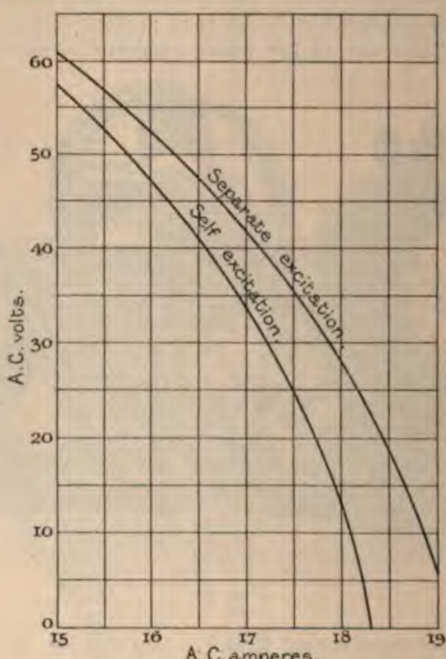


FIG. 13.—Compensating for Current.

that the exciter armature forms rather a large percentage of the total machine. This is partly on account of the fact that, since these machines were constructed to give a falling characteristic, nearly the whole of the flux passes through the exciting armature, and partly because the exciting armature being surrounded by poles which are all of one polarity, proportions have to be adopted different from those which are customary in ordinary D.C. machines. In criticising the use to which these machines may be put, I should be inclined to say that, as constant-current machines self exciting, the size of the exciter armature would limit their adoption to comparatively small units; but

where separate excitation is adopted, there is no limit to the output for which they are suitable.

In considering in the same way the constant-potential type, I should be inclined to think that in such sizes as will entail less than a 2 per cent. loss for excitation, the exciter armature and its magnetic circuit would be too small properly to allow of the regulation being obtained. This, however, needs more practical demonstration. I should like to point out that the *self*-exciting constant-potential characteristic which corresponds very nearly to that of a shunt D.C. machine, is obtained by having two armatures magnetically in parallel; while the constant-current characteristic, which corresponds to the series D.C. machine on heavy loads, is obtained by means of two

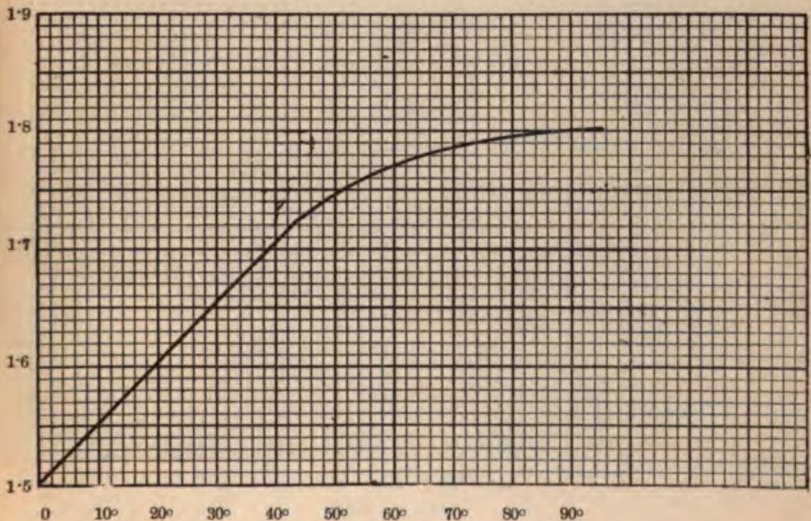


FIG. 14.—Voltage Compensation.

armatures magnetically in series. This is a good example of the converse of the axiom which I put forward in the *Electrical Review*, vol. 58, 1906, p. 668, which ran as follows:—

"Two or more electric circuits excited by being placed magnetically in series (that is, as secondaries upon the same limb of a constant-potential transformer) behave as electric circuits in parallel; and similarly two or more electric circuits excited by being placed magnetically in parallel, behave almost as electric circuits in series."

One use further may be suggested for machines of this type, *i.e.*, as a new form of *motor converter*. If the alternator armature of the machine shown in Fig. 12 be supplied with an alternating current while the rotor is run up to speed, the machine will run as a synchronous motor because

the D.C. armature will excite and set up a field current. When in synchronism, increase of load will tend to strengthen the field magnetism by reducing the reluctance upon the synchronous motor side, so that the machine will automatically compound for fall of voltage upon the D.C. side. Thus we have a motor converter with its D.C. voltage completely under control and tending automatically to compound, while economy and efficiency are obtained by the use of one field coil for the two armatures.

Alternating-Current Motors.

In alternating-current motors the great importance of the dispersion or leakage factor in determining the characteristics of the various

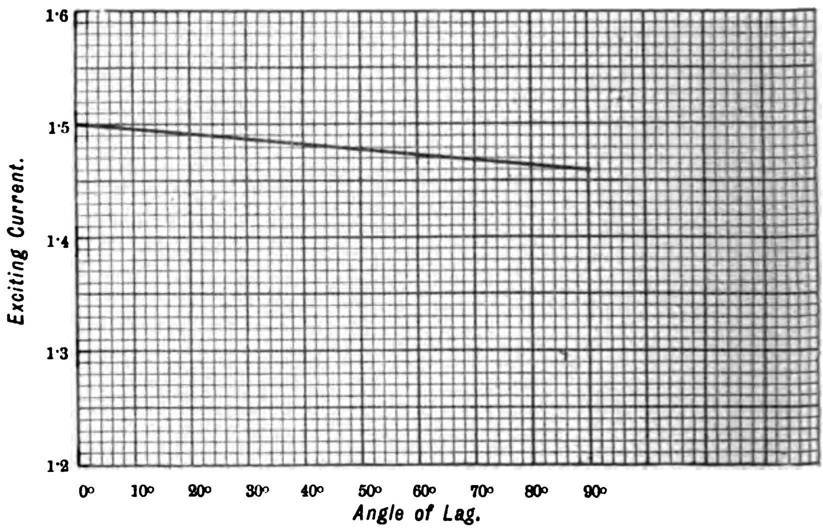


FIG. 15.—Voltage Compensation.

motors as exemplified in the semicircle diagram, has led to a great deal of practical research, most of which was brought together at the historic meeting of this Institution in London when Dr. Behn-Eschenburg read his paper upon the subject. Since that date, however, the great advance which has been made in the direction of alternate-current commutator motors necessitates the division of the subject here into (1) motors without salient poles, and (2) motors with salient poles. To the former class belong all induction motors and, indeed, all those motors which depend upon a rotating field for their performance, among which must be reckoned the ordinary repulsion motor with its various modifications. The results in the paper referred to give for the most part an excellent approximation for the designer's guide in estimating the leakage factor, and it is only necessary for me

here to draw attention to one or two points which appear to some extent to have been overlooked.

The first of these is the fact that no distinction is drawn in the paper between ordinary and hemitropic windings. Now, since the theoretical short-circuit current (in Fig. 1, O B) is the terminal voltage divided by the calculated reactance of the windings, whilst the theoretical no-load current (in Fig 1, O A) is the calculated magnetising current, it follows that the latter is the same whether ordinary or hemitropic windings are used, while the former is smaller with the hemitropic winding than with the ordinary winding. This is easily proved as follows: If f be the number of C.G.S. leakage lines produced by unit (C.G.S.) current in one turn of a coil, and t be the

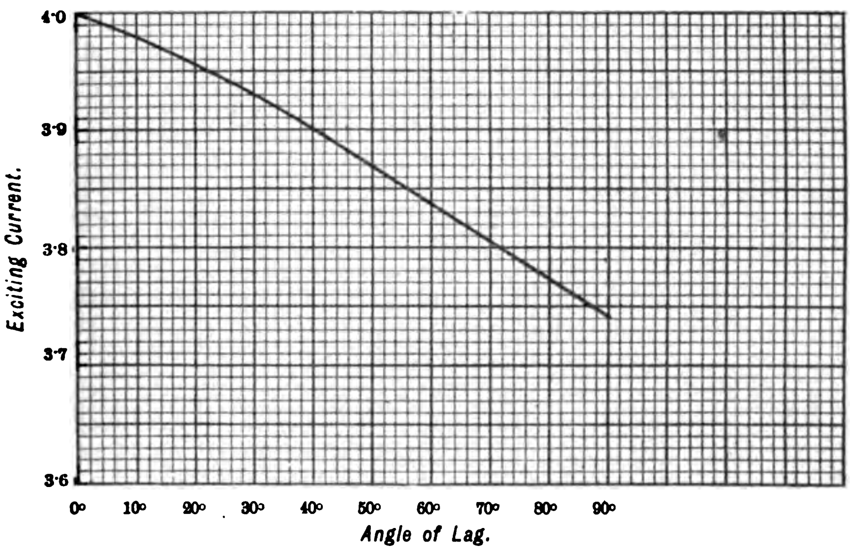


FIG. 16.—Current Compensation.

number of turns in the coil, then the coefficient of self-induction for that coil is—

$$\text{L (henries)} = \frac{t^2 f}{10^9},$$

thus the reactance per coil—

$$= 2\pi \times \sim \times \frac{t^2 f}{10^9} \text{ ohms.}$$

In a hemitropic winding we have C coils in series per phase and t turns per coil, so that reactance per phase (hemitropic)—

$$= 2\pi \times \sim \times \frac{t^2 f}{10^9} \times \text{C} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

In an ordinary winding we have two C coils in series per phase and $\frac{t}{2}$ turns per coil—

Reactance per phase (ordinary)—

$$= 2\pi \times \sim \times \frac{t^2 f}{4\ 10^9} \times 2\,\mathrm{C} \quad \ldots \ldots \ldots \quad (2)$$

and obviously (1) is twice (2).

In practice the hemitropic winding is not found to have a reactance equal to twice that of an ordinary winding, probably because the above calculation assumes that in each case the leakage lines of each coil only link with the turns of that coil, which is most unlikely; and also because the fraction of stator face devoted to the wires of one side of a coil is usually greater in the hemitropic form. But there is in practice a difference, though it is not so great as 2 : 1; and this is almost certain to be the case, because the inductance of the end connections of a hemitropic winding is twice as great as the inductance of the end connections of an ordinary winding, and also because fewer slots are used in the hemitropic case. In my opinion the difference between hemitropic and ordinary reactance per phase is in the proportion of about 5 : 4.

Another point to which I wish to draw attention is the discrepancy in Dr. Behn-Eschenburg's formula pointed out by Professor S. P. Thompson, *i.e.*, the fact that in the second term the factor which takes into consideration the bridge of the slot has been omitted, so that instead of—

$$v = \frac{3}{\mathrm{N}^2} + \frac{\Delta}{\mathrm{X\,N\,T}} + \frac{6\,\Delta}{b}$$

we have—

$$v = \frac{3}{\mathrm{N}^2} + \frac{10\,\Delta\,e}{\mathrm{X\,N\,T}} + \frac{6\,\Delta}{b}$$

where—

N = Mean value of slots per pole in rotor and stator.

Δ = Air-gap length in mm.

b = Gross axial core length in mm.

$e = \left\{\begin{array}{l}\text{Mean edge thickness of tooth for open slots} \\ = \text{Mean of edge thickness of stator teeth and of thick-} \\ \quad \text{ness of thinnest part of iron bridge over rotor slots} \\ \quad \text{when closed.}\end{array}\right\}$ in mm.

$\mathrm{X} = \left\{\begin{array}{l}\text{Average slot opening for open slots in rotor and stator} \\ = \text{Average of slot opening in stator and length of} \\ \quad \text{thinnest portion of iron bridge over closed rotor} \\ \quad \text{slots.}\end{array}\right\}$ in mm.

T = Pole pitch measured upon stator face reduced by ($\mathrm{X} \times \mathrm{N}$) in mm.

This change renders the formula a simple numeric (*i.e.*, without dimensions) and results in a better approximation for the value of v.

A third point omitted by Dr. Behn-Eschenburg is the effect of "squirrel cage" against "wound" rotors. Naturally for the former v

will be less than for the latter, and this change is, I think, about in the proportion of 0·9 : 1.

The following are some calculations which show the sort of approximation obtained with this expression :—

TABLE VIII.

VALUES OF v IN INDUCTION MOTORS.

Motor Number.	1	2	3	4
Maker ...	Electrical Co.	Oerlikon	Brown Boveri	Siemens Bros.
Phases ...	3	3	2	3
Output ...	5 H.P.	5	2	5
Poles	4	4	6	6
Speed (sync.)	1,500	1,500	2,000	1,000
Type winding	Star hemitropic	Star hemitropic	Ordinary	Star or Δ ordinary
$\sim$	50	50	100	50
v calculated ...	0·0563	0·048	0·087	0·071
v from test ...	0·054	0·048	0·077	0·067
Rotor	Wound	Wound but short-circuited	Squirrel-cage	Wound

No allowance has been made for hemitropic windings or squirrel-cage rotors in the above, and my belief is that for hemitropic windings and wound rotors the formula is right. For ordinary windings it must be multiplied by 0·9, and for squirrel-cage rotors by 0·9.

These results on four motors chosen at random show that there is little fault to find with the calculation of v by Dr. Behn-Eschenburg's formula, and when the value of the leakage factor in such machines as those given above is compared with that of similar sized continuous-current motors, one is struck with the effect which a small air-gap and a coil close to the armature have upon the machine.

The 5-H.P. motor in column 1 has a leakage factor 1·054, whilst the 5-H.P. D.C. motor we were considering at the beginning of this paper has a leakage factor as large as 1·35.

Well may induction motors be smaller than continuous-current machines!

Having thus shown that little improvement is to be looked for in the calculation of the leakage factor of rotating field motors, I pass on to the machines with salient poles. That the principles laid down as affecting continuous field design are much better recognised in the world of alternating currents is sufficiently apparent. Nearly all salient pole A.C. motors are fitted with field coils spreading almost over the pole pitch and the pole is kept short, while neutralising windings are used to reduce the armature leakage. These devices (on account of the mischievous effect of magnetic leakage in A.C. work) have had to be adopted by A.C. designers, and those engaged in the design of continuous-current machines would do well to profit by them. Harmful

leakage in salient pole alternating-current motors is reduced to a minimum. Useful leakage, on the other hand, has never been made much of. In the starting up of single-phase motors it has been utilised, as also it is the principle really involved in the repulsion motor. I wish to show that it can be quite as well employed in that modification of the series motor which I first suggested at a meeting of this Institution about twelve months ago. It will be remembered that after that meeting my suggestion was much criticised in the electrical press; statements being made to the effect that such a motor would have low-power factor, practically no torque, low efficiency, not really a series characteristic, etc.

It was my original intention not only to give a full theoretical explanation, but also to show a full practical test of such a motor, but, unfortunately, owing to the fact that the makers delivered the machine

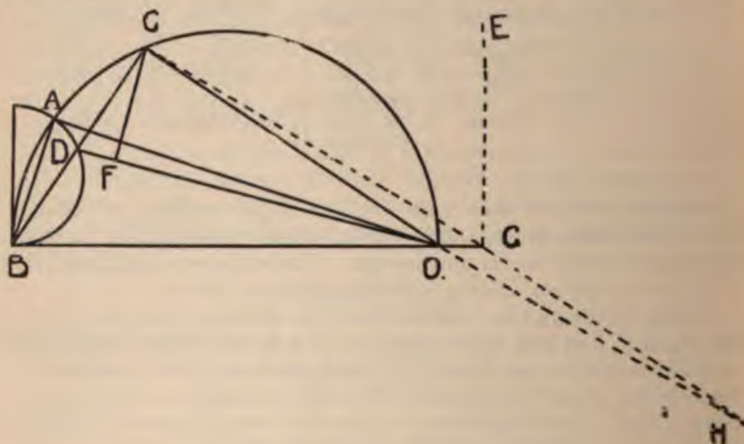


FIG. 17.

a month late and with a serious short on it my test must be delayed for a few weeks. In the meantime I give the theoretical figures and advantages; which is most easily done by commencing with the semi-circle diagram for a series motor.

When the armature is locked, a plain series motor behaves exactly as a choking coil, *i.e.*, if in Fig. 17 O A represents the locked current for full terminal P D, then it also represents to a voltage scale the reactance drop in the motor, and A B at right angles to O A represents the resistance drop. O B is therefore the voltage to drive the current O A against the motor impedance, that is, the full motor P D; whilst O B A is the angle of lag of the machine current behind the applied P D. Releasing the armature and allowing it to run (still keeping the terminal P D constant) we have an E.M.F. set up in the armature windings practically in phase with the main field flux, but, like the resistance drop, opposing

the armature current. This has the effect of reducing the current O A through the addition of an E.M.F. in phase with the armature-resistance drop B A. Thus B A is increased to B C, and B C O still remaining a right angle, the current line O A has for its locus a semicircle upon O B as base.

Reduction of O A to O C, of course, proportionally reduces the resistance drop, so that A moves on the semicircle A D B, whilst C traverses the semicircle B C O. Thus for any position C, to the *current scale* (fixed by O A as short-circuit current A B O as angle of lag given by the wattmeter)—

O C = current taken.

To the *voltage scale* (fixed by O B = full terminal volts)—

O C = motor-reactance drop,
B D = motor-resistance drop,
C D = back E.M.F.

Also since output = back E.M.F. × current, it is = C D × C O, or proportional to the height of the perpendicular from C upon O D, *i.e.*, to C F.

The torque is proportional to OC^2.

Now the above diagram has to be modified in practice to take account of saturation and iron losses, but it is near enough to prove substantially that if—

1. The resistance voltage be low (B D), and,

2. The back E.M.F. be high (D C) compared with the reactance drop (C O).

Then the motor will be good both as to power factor and efficiency.

1 and 2 above are easily attained in practice by well-known means. Now if having designed an ordinary series bipolar motor such as that shown in Fig. 18, I apply to its top another limb like Fig. 18*a*, properly wound, and attached to the mains; and if then I join the free end of the present field to the free brush, I shall have a motor whose main and leakage magnetic paths are in parallel *exactly as they are in an ordinary induction motor;* with these differences: (1) that in an ordinary induction motor the torque is produced by the main field and the rotor current, while here it is by leakage field and rotor current; and (2) that in an ordinary induction motor all the B.H.P. has to cross an air-gap, whilst here only part need cross it.

These two points are well brought out in the change in the semicircle diagram.

The new primary coil will have a very small resistance, and consequently at constant terminal volts the flux through it will be practically constant and the magnetising current practically constant also. If the joints in the circuit be good, there is no reason why the magnetising current should be large, and we may draw it as O G, Fig. 17; so that the total primary current is now OH (= G C).

The flux across the air-gap will now be slightly increased, but also slightly shifted in phase away from O C towards G C, and so the torque still remains practically $\propto OC^2$.

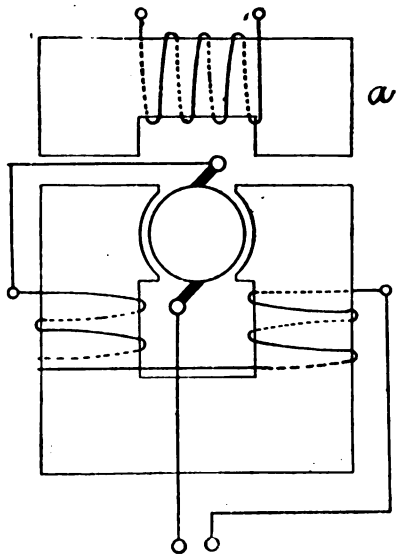


FIG. 18.

The power factor is changed from cos C B O to cos C G E, and the [redacted]

factor and of immensely improving the commutation by altering the number of armature and leakage limb turns respectively. The armature of self-induction is always a problem in H.T. motors, and it is this problem which affects both power factor and commutation.

Now the self-induction of the armature depends upon

$$(\text{the armature turns per pole})^2,$$

thus a small decrease of the turns produces a large change in the coefficient of self-induction, and hence also a large change in power factor and commutation conditions. Or, to put this matter in another light, these motors may be built with fewer poles than ordinary series motors for the same power factor. Of course there is no difficulty about multipolar designs for this type of machine.

The nearest parallel to the above motor in use at the present day is the repulsion motor represented diagrammatically in Fig. 19. In this motor the short-circuited armature is fed through induction by coils A, while coils B provide the field flux. A and B are placed in series across the mains. When the locked test is applied to such a motor, it is seen that the coil A is relatively non-inductive compared with the coil B hence a phase displacement of flux takes place, setting up a rotating field somewhat similar to the starting system of an ordinary single-phase induction motor; and in order to take full advantage of this, a stator without salient poles, like that of an ordinary induction motor, must be provided.

A comparison of Figs. 18 and 18 *a* reveals the fact that for electrical circuits in series magnetic circuits in parallel are used, which is an additional confirmation of the principle just laid down (page 575). Also we note that in Fig. 19 not only has a flux at B to cross the air-gap, but also a flux at A, so that the whole energy imparted to the rotor has to cross the air-gap. This is the secret of the low-power factor obtained with repulsion motors. For the sake of clearness, we may compare four motors with similar characteristics, as follows :—

TABLE IX.

	Series Motor Simple.	Series Motor with Transformers.	Repulsion Motor.	Cramp Motor.
Suitable to Voltage ...	Low only	Any	Any	Any
Power factor... ...	Good	Good	Fair	Good
Commutation...	Depending on voltage	Very good	Only good at sync.	Very good
Iron weight	1	1·75	1·1	1·5
Copper weight ...	1	1·75	1·3	1·35
Easy to wind... ...	Very	Very	No	Very

It is thus seen that for low voltages the simple series motor is best, while for high voltages my motor is best; a verdict which I hope shortly to be able to endorse by definite tests.

I have now shown, I think, that the subject of magnetic leakage is one deserving of great attention, and I trust that the original work here set out may prove productive of much more on the same lines. It only remains for me to thank those who have helped me in this work; foremost among these stands Mr. Garner, whose patience and capability have contributed largely to whatever of interest the paper exhibits. My thanks are also due to Principal Reynolds, the Technical School Committee, and the Electrical Department of the School. Also to Messrs. Wood and Browning, students of the School, who have willingly given me help; and to Messrs. Henry Simon, Ltd., who have lent me the machines for exhibition.

Discussion.

Mr. Frith.

Mr. J. Frith: The author begins by a campaign against the standard shape and form of continuous-current motors. We have always been used to continuous-current machinery with shaped poles and a yoke, and we are told that the machine on the table, which is like four hat-boxes, is a very good form of continuous-current machine. I must say the arguments brought forward to support it are all very good, but I do not like the look of it, and there is no doubt that small motors, at any rate, are sold more or less by their looks. In the comparison of costs the author does not say on what basis the copper and steel costs are worked out. With the metal market going up as it is, one form of motor may be the cheapest to-day and another form to-morrow. The long coils do not appear to me quite the right thing. The next thing in the paper is a method of magnetising from the armature. That opens out a great deal of very interesting work. I wonder something has not been said in connection with the continuous-current dynamo introduced recently in this country for train lighting. It is a very good example indeed of that class of machine. The machine mentioned in the paper has a double-wound armature; this, instead of having to use another winding and another commutator, has twice as many pairs of brushes as poles. A two-pole machine would have two sets of brushes with one set short-circuited, and in that way the one armature carries both the magnetising and the main current. That the magnetising current in one half of the armature is helped by the armature current in the other, and is also independent of that current, is not fully explained. We are given a set of phenomena where the effect is apparently independent of the cause, and I am sorry that in the Table IV., which demonstrates that point, the main current is only given between the limits of 158 amperes and 226 amperes. I should like to know when that effect begins; it cannot be there on quite open circuit because there would not be then any main current. It would be rather interesting to study what happens between the time when this begins and the time when it becomes independent of its cause. The author has brought out another interesting question as to what is really the magnetising power of a current in an armature, *i.e.*, a current that goes

all round the circular armature instead of being wound round the fields. The curves in Fig. 8 are not entirely conclusive. There is an element of assumption and calculation about them, but certainly a better result is arrived at than $\frac{2}{\pi}$. The real magnetising value of a current carried by an armature is a question which would pay for a little research. Of course it is quite plain that the fact that the magnetising current is helped by its armature current is only true when the machine is being used as a motor. Turning to the section on alternators: an exceedingly interesting form of inductor alternator is described, which can be used both for constant current and constant potential. I am sorry that it is an inductor alternator, because I think that form of alternator is played out, and it is rather a pity that such a lot of ingenuity is more or less wasted by its connection with an inductor alternator, unless perhaps the new method will bring the inductors into favour again. As a constant current machine it does not interest me much. It is very easy to build an alternator with very nearly constant current. The interesting point is when the same construction is used to make a constant potential alternator, which is quite another thing. I think it is a very ingenious method which makes the stray field work the exciter, and the author has passed by it with too much modesty. I think the principle of making the armature stray field magnetise the exciter is a very pretty one indeed, because it not only compensates for the amount of external current, but also for its phase, and that is a very difficult thing to do in compound alternators, but the method in question very ingeniously uses the waste field, the thing that is doing most of the harm, to get over that difficulty. Coming now to the section dealing with induction motors, we are all glad to hear more of the curious "Cramp" motor. The limit of a series motor is the voltage that it can be wound for, and the new motor is a series motor with its own transformer on the top of it. Suppose a series motor with its transformer were put in a box we should get a diagram exactly like that shown in Fig. 17. There would be the magnetising current of the transformer coming in, and it would be absolutely indistinguishable from the motor shown on the table. Mr. Frith.

Dr. C. C. GARRARD: On page 581 it is stated that the advantage of the Cramp motor over the ordinary induction motor is that only a certain proportion of the B.H.P. has to cross the air-gap. I cannot understand that. It seems to me the energy must go across the air-gap, just the same as in other motors. Dr. Garrard.

With regard to Table III., the author shows an advantage in the cost of the two machines of about 4 per cent., but he is comparing a 5-k.w. with a 6-k.w. machine. If machines of the same output had been compared, the advantage as regards cost per k.w. would be the other way.

Mr. C. F. SMITH: In Table IV., column 8 amounts practically to the fact that the field which gives the voltage measured at the Mr. Smith.

Mr. Smith. high-tension terminals is practically constant, no matter what the low-tension current supplied to the armature to run it as a motor. The low tension ampere turns appear to be without any effect on the flux which produces the voltage at the high-tension terminals, although the flux is really produced by the low-tension ampere-turns. The author states that he is not satisfied as to the explanation of this apparently contradictory result. As helping towards a possible explanation, it may be worth pointing out that the back voltage at the low-tension terminals does not increase in the same proportion as the speed, which is evident by comparing column 9 with column 1. Supposing that the flux producing the low-tension voltage remained constant, the voltage at the low-tension terminals would increase in the same proportion as the speed of the motor, which is an increase of practically 1 to 10, whereas the actual increase is only in the ratio of 1 to 6·74 instead of 1 to 10, so that the flux produced by the constant excitation applied through the high-tension brushes appears to diminish to the fraction 0·67 of its original value as the speed increases through the range shown in the table. That means that at the highest speed the apparent flux producing the low-tension volts is only 0·67 of the flux which exists at the lowest of the speeds. Supposing that the high-tension flux increased in the same proportion as the ampere-turns supplied to the low-tension brushes, the flux would increase in the proportion of 158 ampere-turns to 226 ampere-turns, since the flux may be supposed to increase in the same proportion as the current producing the flux. If the effect of leakage which the armature current seems to induce is the same on the circuit between the high-tension brushes as it is on the circuit between the low-tension brushes, the effectiveness of the actual flux will be reduced as before by the fraction 0·67. By actual multiplication it appears that if we multiply 0·67, the apparent diminution of the flux on the low-tension brushes, by the ratio of the currents 226/158, the value of

$$\frac{226}{158} \times 0{\cdot}67 = 1{\cdot}01$$

obtained will be seen to be the actual ratio between the flux for the low speed and the flux for the high speed, making the flux an apparently constant quantity for the machine used. It would thus be to some extent an explanation to say that the leakage effect produced by the increased armature current acts on the circuit between the high-tension brushes in exactly the same way as it acts on the circuit between the low-tension brushes, and would account for the fact that the field appears to remain constant. Possibly the action may be found to be something in the nature of a general change of the axis of the total field.

Dr. Dr. F. H. BOWMAN : We all know how the tendency is to distribute power in small units, and for that purpose we require small motors, which, as we know, have been very inefficient. I am not particular about the shape, and when I saw the motor on the table I thought we

had something new. I am glad to see that by the alteration to the form, and putting the coils inside instead of outside, and doing away with the yokes, we get a machine that gives a larger output for the same size, and is apparently more economical. But when I see that there was 52s. worth of copper instead of 36s., and I know the price of copper, I think it will have to be deferred until another time. There is one thing which strikes me in the opening pages. The author divides the leakage into that which opposes the attainment of the ideal characteristic at which a designer aims in any particular machine, and that which assists the designer to attain the end at which he aims. I think that is exactly the way in which the biologist talks about microbes; some are inimical to life and some advantageous, and the author seems to be doing in his apparatus exactly what they profess to do in medicine; while he cannot decrease the quantity of the inimical microbes, he increases the quantity of those that are amiable to utilise them to advantage. Dr. Bowman.

Mr. H. W. WILSON: In Figs. 3 and 4 there are very pretty illustrations of machines which the author considers would have a very much better leakage coefficient than the ordinary standard motor we are generally acquainted with. I have seen one or two machines built after that plan; there was one designed for motor-car work, which was like Fig. 4. He does not claim that those machines have a higher efficiency than the ordinary motor, but only that they would be more economical to construct, and that they would be of smaller size and weight for any given output. On that basis I do not think that the increase of efficiency that Dr. Bowman is hoping for is very probable. The author states that his new machine will have a cylindrical cooling surface distinctly larger than that of the ordinary motor; but my experience of those machines where the field magnet coils are brought down close to the armature is that they have a very greatly increased temperature rise as a rule. Mr. Cramp speaks rather disparagingly of the Lundell motor, and my opinion about the Lundell motor is that it has missed its vocation, and is meant for a piece of heating apparatus, and not for a motor. Some of the designs present would also have the same effect, because the machines do not appear to be as well ventilated as the ordinary small motor, and I think it will be necessary to build machines and carefully test them to find out whether there would be any advantage. Theoretically, it ought to be possible to reduce the dimensions; practically, I doubt whether it can be done very materially. In another part of the paper, where the author speaks about the possibility of doing away with windings on the field magnet circuit altogether, and having the magnetic winding and the armature winding both on the armature, I gather in one place that he seems to have abolished armature reaction altogether, or rather that the armature reaction keeps constant at all loads. I should like to know if he can give any explanation of that. Like Mr. Frith, I am anxious to know what curve D in Fig. 8 means. Mr. Cramp said that the machine on the table is a purely Mr. Wilson.

Mr. Wilson. experimental one, and that the shape of the coil as shown in the machine on the table does not represent anything definite, but I must confess that I am still in the dark as to what the coil is for. I can see that it has a sort of transformer action. I should also be glad to know whether that machine can be built other than as a bipolar machine.

Mr. Goldschmidt. Mr. R. GOLDSCHMIDT: The author gives the coefficients of leakage of the machines shown in Figs. 3 and 4 as 1·4 and 1·1 respectively, that is, the leakage of the ordinary machine is four times as much as his special design. That certainly does not look quite correct, as the leakage lines passing through the field winding itself from pole to pole and from the poles to the end brackets are very considerable in the latter case. Further, I wonder if he can with many designs afford such big holes in the middle of the poles as are shown in Fig. 4. I find that one usually is glad with most machines to accommodate the necessary field winding and the section of the poles necessary to carry the flux without any allowance for air. The author states that he gains in ventilation. Is not the armature very much more boxed in than with the ordinary design? With regard to the leakage coefficient of induction motors, I agree with his statement that the leakage of hemitropic windings is certainly more than half that of ordinary windings, and that both are more nearly equal. In the author's alternating commutator motor the ampere-turns of the coil on the top of the machine are practically in phase with those of the proper field coils, provided that a good joint exists between the top yoke and the horseshoe portion, just as in a series transformer. All exciting ampere-turns having equal phase, the whole character of the machine must be that of an ordinary series motor. The top yoke might be left off, and the coil on it put in series with the ordinary field coils on the horseshoe, and naturally the motor would in that form be a low volt motor only. It would be interesting if the author would give a sketch showing how the special yoke and the high volt coil would be accommodated in, say, a four-polar railway motor. He remarks at the end of the induction motor portion of his paper that induction motors are smaller than continuous-current machines, because the leakage factor of the former is smaller. Surely the leakage factor is only of secondary importance with regard to the size of machine. Its influence on the behaviour of the machines themselves is entirely different in both cases; an induction motor with a leakage factor of 1·15 is bad, a continuous-current machine with 15 per cent. leakage is very good.

Mr. Cramp. Mr. W. CRAMP (*in reply*): I must agree with Mr. Frith in that some apology is due for the arrangement of the paper. If I had to re-write it I certainly would not arrange it in quite the same way. As to Mr. Frith's remarks on the appearance of machines, I cordially agree that for selling purposes it is most important that a machine should have a pleasing appearance, and so convinced am I of the truth of this that I lay great emphasis on it each session at the School of Technology.

But though in section my machine may appear extraordinary, it certainly is not necessary that its appearance should in reality be at all uncommon. I think that Mr. Frith made too much of the question of field coil dimensions, as many firms with ordinary pattern motors do not attempt to adopt the most economical coil shape. With respect to the tables of costs given in the paper, all the figures were based on the nominal prices of copper at 1s. per lb., cast iron at 1½d. per lb., and cast steel at 2½d. per lb. I am glad to endorse Mr. Frith's views as to the recent train-lighting dynamos, which are undoubtedly good examples of magnetisation from the armature. Mr. Cramp.

With regard to Mr. Frith's remarks on inductor alternators, there is no reason why the same principle which is involved in the self-exciting inductor machine should not be applied to alternators of ordinary form. In explanation of Fig. 15, I would say that Figs. 14 and 16 were taken for one frequency, Fig. 15 for another, and it is clear that there is a certain frequency at which the machine behaved best. Further, Figs. 14, 15, and 16 were all taken for a constant current of 10 amperes. In considering that the alternating-current motor shown is essentially the same as a series motor with separate transformer Mr. Frith is quite in error, for the economy obtained over the latter combination is very marked both in copper and iron, as shown by the comparisons given in Table IX.

Coming next to Dr. Garrard's remarks, as no air-gap is necessary between the motor proper and the limb in Fig. 18, there is no necessity for the power appearing at the brushes to cross any air-gap. With regard to Table III., I feel sure that Dr. Garrard is in error in thinking that the difference in specific cost for 5- and 6-k.w. machines respectively is as much as 4 per cent.

I thank Mr. C. F. Smith for his suggested explanation of the phenomenon referred to on page 565, and it seems to me that that explanation is right.

Referring to Dr. Bowman's remarks, my object was for a given efficiency to reduce the size and cost of the machine rather than to raise the efficiency of existing sizes.

In answer to Mr. Wilson, Lundell machines undoubtedly do get very hot indeed, but the designs shown in the paper can be ventilated very easily and efficiently by a simple slot in the middle of the thin part of the pole.

In reply to Mr. Goldschmidt, the coefficients of leakage given in the paper were estimated from actual machines of similar type. I am glad to have Mr. Goldschmidt's confirmation of my statements concerning hemitropic windings; but in his criticisms of the new A.C. motor Mr. Goldschmidt is, I think, entirely wrong. The effect of placing the upper coil around the lower limbs, and doing away with the upper limb, would be to change a motor with a good series characteristic into a bad shunt motor, and to convert a machine which depends for its action on magnetic leakage into one which is practically independent of this principle.

CAPE TOWN LOCAL SECTION

MODERN TRANSFORMER DESIGN.

By Professor H. BOHLE, Member.

(*Abstract of Paper read June* 14, 1906.)

EXPLANATION OF SYMBOLS AND EXPRESSIONS.

B_m = flux density (amplitude).
c, C = coefficients.
D = density of copper.
D_i = density of iron.
d = diameter of circle circumscribing core.
d_1 = distance between two such circles.
E_1 = primary E.M.F.
f = frequency.
f_i = space factor of iron core.
f_c = space factor of copper windings.
f = form factor of E.M.F. wave.
f, F = functions.
h = height of core.

[redacted]

The degree of efficiency of a well-designed transformer is largely a matter of cost, and in determining the most economical efficiency for any particular working conditions the relation between capital cost of plant and cost of energy must be taken into account. In all cases the efficiency of a transformer should be such that the rentability of the plant is a maximum. We find this efficiency for known working conditions as follows:—

The cost of the active material of a transformer P is a function of the efficiency η, so that—

$$P = f(\eta).$$

The cost of the inactive material, such as cases, cooling oil—if any —and the expenditure caused by wages, power required for machine tools, etc., are partly proportional to P, partly constant, so that the total price of the transformer P_t is expressed by—

$$P_t = C + c\,P \text{ shillings.}$$

Where C and c are coefficients, the former oscillating between 1·5 P and 0·5 P, the latter between 2·0 and 1·2, according to the size of transformer, the mode of construction, and the locality of the manufacturing works, being constant for a given size and known manufacturing conditions.

The expenditure caused by interest and amortisation, at p per cent., is—

$$p\,C + p\,c\,P = p\,C + p\,c\,f(\eta).$$

The cost for current consumption is also a function of the efficiency, and equal to $F(\eta)$, following a straight line for a given price per kilowatt-hour, so that the total working costs are equal to—

$$p\,C + p\,c\,f(\eta) + F(\eta).$$

When this is a minimum—

$$f'(\eta) = -\frac{1}{p\,c} \times F'(\eta),$$

and since—

$$F'(\eta) = \tan \alpha,$$

and—

$$f'(\eta) = \tan \beta,$$

$$\frac{\tan \beta}{\tan \alpha} = -\frac{1}{p\,c},$$

i.e., the most favourable point in the curve $P = f(\eta)$ is that point the tangent of which forms with the axis of abscissæ an angle β, the — sign denoting that the angles α and β are to be plotted in opposite directions with regard to the axis of abscissæ. All we require to know are the values of p and c, the cost per kilowatt-hour, and the average or annual load.

When a definite copper loss has to be assumed on account of the

"drop," which may have been specified, we obtain the most favourable efficiency without requiring to know the annual load by plotting as abscissæ the iron losses $p_i P_l$, and as ordinates the price of the active material $P = f(p_i P_l)$, where P_l denotes the total losses, plus the cost of the kilowatt-hours to cover the iron losses, the latter being equal to $F(p_i P_l)$.

For unknown working conditions the efficiency has to be assumed, for which purpose the curves in Fig. 1, *a* and *b*, may be employed. As the temperature rise is roughly inversely proportional to the efficiency, the latter must be high enough to prevent excessive heating. Curve *a* in Fig. 1 gives the efficiencies of transformers with natural draught cooling, the temperature rise of which does not exceed 50° C. (90° F.).

The regulation at various power factors depends upon the E.M.F.'s of resistance and leakage reactance. The latter is proportional to the

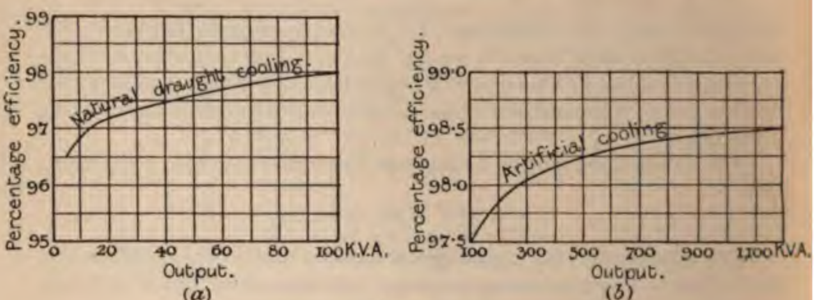


FIG. 1.

square of the number of turns, and depends further upon the disposition of the coils. By employing long and thin concentric windings, or highly subdivided sandwiched coils, the pressure drop may always be kept within reasonable limits.

The method of cooling depends chiefly on the efficiency of the transformer. From a commercial standpoint it pays to accelerate the dissipation of heat by means of oil for all sizes above 50 to 80 K.V.A., and by mechanical cooling (forced draught or combined oil and water) above 250 K.V.A. The dissipation of heat depends slightly upon the copper space factor f_c, that is, the ratio—

$$f_c = \frac{\text{space absorbed by the copper of the winding}}{\text{total available winding space}}.$$

If f_c be large, we obtain a comparatively small transformer, but the chances are that little space is left for properly insulating the coils, and ventilating ducts are out of the question. Fig. 2 represents the space factors of transformers for outputs varying from 10 to 1,000 K.V.A., for P.D.'s up to 40,000 volts. Oil-cooled transformers may be given a

longer space factor than blast-cooled designs, since oil itself is an insulator.

The magnetising current which influences the power factor of the plant on account of being chiefly wattless, may be made small by

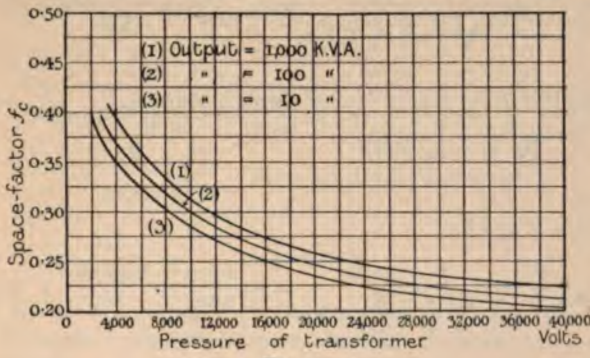


FIG. 2.

employing a short magnetic circuit, a small flux density, iron of a high permeability, and properly constructed joints.

The subject of the calculation of the main dimensions has been fully treated in a previous article by R. Pohl and the author.* The formulæ were deduced for a single-phase core transformer of the cruciform type, and the principal equations are as follows:—

$$I'' B''' h\, d_1\, d^2 = \frac{5{\cdot}7 \times \text{K.V.A.} \times 10^9}{f f_i f_c} = K_5 = \frac{\text{amps.}}{\text{mm.}^2} \times \frac{\text{kilolines}}{\text{cm.}^2} \times \text{mm.}^4$$

$$= \frac{5{\cdot}7 \times \text{K.V.A.} \times 10^7}{f f_i f_c} = K_5 = \frac{\text{amps.}}{\text{inch}^2} \times \frac{\text{kilolines}}{\text{inch}^2} \times \text{inch}^4 \quad . \quad . \quad (1)$$

$$M_i = 12 \times 10^{-6} f_i\, d^2 (2\, d + d_1 + h) \frac{\text{kilograms}}{\text{mm.}^3}$$

$$= 0{\cdot}44 f_i\, d^2 (2\, d + d_1 + h) \frac{\text{lbs.}}{\text{inch}^3} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$M_c = 28 \times 10^{-6} f_c\, h \left(d\, d_1 + \frac{d_1^2}{2}\right) \frac{\text{kilograms}}{\text{mm.}^3}$$

$$= f_c\, h \left(d\, d_1 + \frac{d_1^2}{2}\right) \frac{\text{lbs.}}{\text{inch}^3} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$M_i = K_M \frac{P_l}{I'' B'''} \sqrt{\frac{p_c\, p_i}{\frac{\rho}{D} \times \frac{K_i}{D_i} \times K_M}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$M_c = \frac{P_l}{I'' B'''} \sqrt{\frac{p_c\, p_i}{\frac{\rho}{D} \times \frac{K}{D_i} \times K_M}} = \frac{M_i}{K_M} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

* *Electrical Engineer*, vol. 36, 1905, pp. 446–449 and pp. 519–521. *Elektrotechnische Zeitschrift*, vol. 26, pp. 897–900.

By combining these equations we obtain—

$$I'' B^m d^2 (2d + d_1 + h)$$

$$= \frac{K_M P_l}{12 \times 10^{-6} f_l} \sqrt{\frac{p_c p_i}{\frac{\rho}{D} \times \frac{K_i}{D_i} \times K_M}} = K_6 \frac{\text{amps.}}{\text{mm.}^2} \times \frac{\text{kilolines}}{\text{cm.}^2} \times \text{mm.}^3$$

$$= \frac{K_M P}{0 \cdot 44 f_l} \sqrt{\frac{p_c p_i}{\frac{\rho}{D} \times \frac{K_i}{D_i} \times K_M}} = K_6 \frac{\text{amps.}}{\text{inch}^2} \times \frac{\text{kilolines}}{\text{inch}^2} \times \text{inch}^3 \qquad (6)$$

$$I'' B^m \left(d d_1 + \frac{d_1^2}{2}\right) h$$

$$= \frac{P_l}{28 \times 10^{-6} f_c} \sqrt{\frac{p_c p_i}{\frac{\rho}{D} \times \frac{K_i}{D_i} \times K_M}} = K_7 \frac{\text{amps.}}{\text{mm.}^2} \times \frac{\text{kilolines}}{\text{cm.}^2} \times \text{mm.}^3$$

$$= \frac{P_l}{f_c} \sqrt{\frac{p_c p_i}{\frac{\rho}{D} \times \frac{K_i}{D_i} \times K_M}} = K_7 \frac{\text{amps.}}{\text{inch}^2} \times \frac{\text{kilolines}}{\text{inch}^2} \times \text{inch}^3 \qquad (7$$

From these equations we obtain—

$$d_1 = 2d\left(\frac{K_7}{K_5} d - 1\right) \qquad (8)$$

[redacted]

We obtain further—

$$\phi^m = f_i \frac{d^2\pi}{4} B^m$$

$$N_1 = \frac{E_1 \times 10^7}{4 f \mathbf{f} f_i \frac{d^2\pi}{4} B^m} \quad \frac{\text{volts}}{\text{mm.}^2 \times \text{kilolines per cm.}^2}$$

$$= \frac{V_1 \times 10^7}{4 f \mathbf{f} f_i \frac{d^2\pi}{4} B^m} \quad \frac{\text{volts}}{\text{mm.}^2 \times \text{kilolines per cm.}^2} \text{ nearly}$$

$$= \frac{V_1 \times 10^5}{4 f \mathbf{f} f_i \frac{d^2\pi}{4} B^m} \quad \frac{\text{volts}}{\text{inch}^2 \times \text{kilolines per inch}^2}$$

and—

$$N_2 = \frac{V_2}{V_1} \times N_1$$

For 3-phase transformers, of the unsymmetrical core type, all cores being arranged in one line, the equations read as follows :—

$$h\, d_1\, d^2\, I''\, B^m$$
$$= \frac{3{\cdot}82 \times 10^9 \times \text{K.V.A.}}{f f_i f_c} = K_5 \frac{\text{amps.}}{\text{mm.}^2} \times \frac{\text{kilolines}}{\text{cm.}^2} \times \text{mm.}^4$$
$$= \frac{3{\cdot}82 \times 10^7 \times \text{K.V.A.}}{f f_i f_c} = K_5 \frac{\text{amps.}}{\text{inch}^2} \times \frac{\text{kilolines}}{\text{inch}^2} \times \text{inch}^4$$

$$M_i = K_M \frac{P_l}{I''\, B^m} \sqrt{\frac{p_c\, p_i}{\frac{\rho}{D} \times \frac{K_i}{D_i} \times K_M}} \quad ; \quad M_c = \frac{M_i}{K_M}$$

$$I''\, B^m\, d^2 (3\, h + 4\, d_1 + 6\, d)$$
$$= \frac{K_M\, P_l}{6 \times 10^{-6} f_i} \sqrt{\frac{p_c\, p_i}{\frac{\rho}{D} \times \frac{K_i}{D_i} \times K_M}} = K_6 \frac{\text{amps.}}{\text{mm.}^2} \times \frac{\text{kilolines}}{\text{cm.}^2} \times \text{mm.}^3$$
$$= \frac{K_M P_l}{0{\cdot}22 f_i} \sqrt{\frac{p_c\, p_i}{\frac{\rho}{D} \times \frac{K_i}{D_i} \times K_M}} = K_6 \frac{\text{amps.}}{\text{inch}^2} \times \frac{\text{kilolines}}{\text{inch}^2} \times \text{inch}^3$$

$$I''\, B^m\, h \left(d\, d_1 + \frac{d_1^2}{2}\right)$$
$$= \frac{P_l}{42 \times 10^{-6} f_c} \sqrt{\frac{p_c\, p_i}{\frac{\rho}{D} \times \frac{K_i}{D_i} \times K_M}} = K_7 \frac{\text{amps.}}{\text{mm.}^2} \times \frac{\text{kilolines}}{\text{cm.}^2} \times \text{mm.}^3$$
$$= \frac{P_l}{1{\cdot}5 f_c} \sqrt{\frac{p_c\, p_i}{\frac{\rho}{D} \times \frac{K_i}{D_i} \times K_M}} = K_7 \frac{\text{amps.}}{\text{inch}^2} \times \frac{\text{kilolines}}{\text{inch}^2} \times \text{inch}^3$$

$$d_1 = \frac{2\, d\, (K_7\, d - K_5)}{K_5}$$

$$I''\, B^m = \frac{K_6\, d_1 - 3\, K_5}{d^2\, d_1 (4\, d_1 + 6\, d)}$$

$$h = \frac{K_5}{I''\, B^m\, d_1\, d^2}$$

For shell transformers the equations are also easily deduced.

For the deductions of the above formulæ it has been assumed that the total losses are a minimum when the flux density is constant along the whole magnetic path. This is, however, not the case, and it may be proved that better results are obtained by making the flux density in the core approximately twice as high as that in the yokes. This modification may easily be introduced when the principal dimensions have been determined. The saving of active material thereby is often

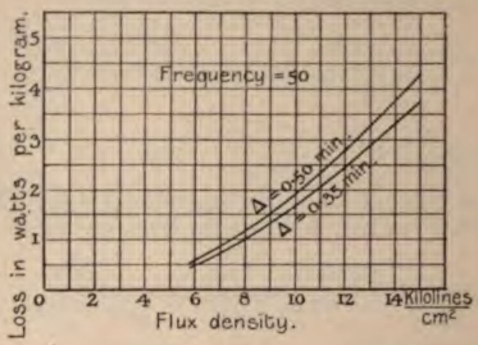


FIG. 3.

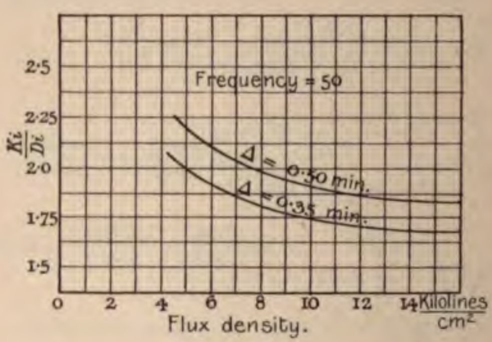


FIG. 4.

considerable, especially in the case of shell transformers, but the cooling area is reduced and the magnetising current increases.

Example.—It is required to design a 5,500-watt lighting transformer with a maximum pressure drop of 2 per cent. at full load, transforming from 2,000 volts to 220 volts at 50 periods. The transformer is only occasionally fully loaded, so that the iron losses should be small. The alloyed iron, as manufactured by Kapito and Klein, Benrath, is suitable for such a design. The loss curves for this iron are illustrated in Fig. 3, and the factor $\frac{K_i}{D_i}$ in Fig. 4.

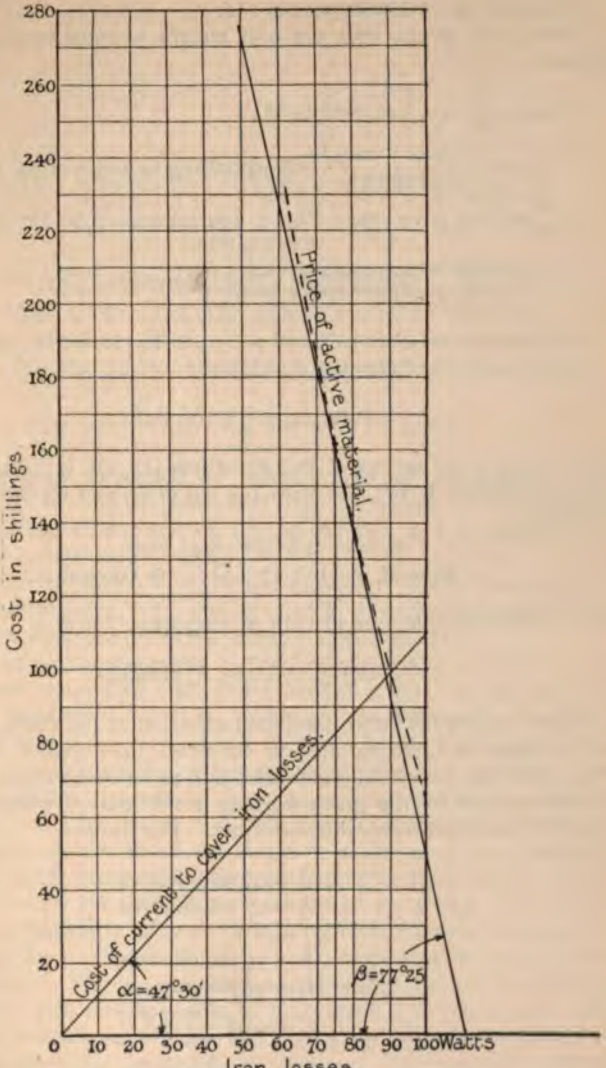


FIG. 5.

The resistance E.M.F. is here almost identical with the percentage opper losses, since the load is non-inductive, so that the copper losses ust be limited to 2 per cent.

We procure the most favourable efficiency by plotting the curve $=f(p_i P_i)$, obtained by assuming various iron losses, the primary of e transformer running for 24×365 hours annually.

The price per kilowatt-hour is 1½d.

The price of the iron per unit weight is equal to that of copper, so that—

$$K_M = \frac{M_i}{M_c} = 1, \text{ hence } M_i = M_c.$$

$$\frac{\rho}{D} = 2.5 \frac{\text{ohms} \times \text{mm.}^4}{\text{kilograms}} \text{ corresponding to a rise of } 50^\circ \text{ C.}$$

$$\frac{K_i}{D_i} = 1.94 \times 10^{-2} \text{ (see Fig. 4, corresponding to } B^m = 10{,}000 \text{ and } \Delta = 0.5 \text{ mm.)}$$

$$\left.\begin{array}{l} f_i = 0.68 = \text{core space factor} \\ f_c = 0.35 = \text{copper space factor} \end{array}\right\} \text{assumed.}$$

The copper losses at full load are equal to 110 watts (2 per cent.).

We assume the following iron losses:—

$$P_i = 40,\ 55,\ 75,\ 100 \text{ watts,}$$

and calculate by means of the equations (1), (6), (7), (8), and (9) the most favourable I″ B^m, and with the aid of (4) and (5) the weights M_i and M_c—

$$\begin{array}{l} I'' B^m = \ 2.8,\ 4.6;\ 8.9;\ 26.6 \\ M_i = M_c = 110;\ 77;\ 46;\ \ 18 \text{ kilograms.} \end{array}$$

At 21 pence per kilogram of copper, respectively iron—

$$P = 385,\ 270,\ 163,\ 63 \text{ shillings.}$$

If we reckon for interest and amortisation 12 per cent., and assume for c a value of 2, we are able to construct the tangent to the curve [redacted]

CAPE TOWN LOCAL SECTION.

THREE-PHASE ELECTRIC POWER TRANSMISSION AT THE CAPE EXPLOSIVE WORKS.

By R. S. Mansel, Associate.

(*Abstract of Paper read November* 12, 1906.)

Electric energy generated by steam power has been adopted in many works for driving purposes, and it is the object of this paper to describe what may be considered a good example of a modern factory electric power transmission plant.

The generating station at the Cape Explosive Works was designed for an output of about 1,000 k.w. in electric power, and also power in the form of compressed air to the extent of some 240 H.P.

The power station contains, in the boiler-room, four Babcock & Wilcox pattern water-tube boilers of American make. These are each of a normal capacity of 300 H.P., which can be increased to 400 H.P. by forcing, if necessary. The grates are hand-fired. Each grate is of about 50 sq. ft. area, and each furnace is fitted with a side door, by means of which the back part of the grate may be very readily cleaned. The furnace fronts are extended in the form of a firebrick arch about 5 ft. in length, against which the flame from the coal first impinges before reaching the water-tubes, thus preventing the direct impact of cold air on them when the furnace door is opened for firing. The steam from the four boilers is passed through two superheaters designed to raise the temperature by 50° F., these being heated by the waste gases from the boilers. The superheaters are of cast iron with vertical tubes, and are fitted with scraping gear to keep them free from soot. The boilers work at a pressure of 150 lbs. per sq. in. Fuel economisers of the Green pattern, but of American make, are also installed, with the object of utilising as much of the heat in the waste gases as possible. Induced draught is used, the fans for the draught being driven by small vertical steam engines directly coupled to the shafts. They deliver the waste gases into an iron chimney stack 6 ft. in diameter and 60 ft. high. Two fans are provided, but one fan is found to be sufficient to produce all the necessary draught.

The engine-room contains three Parsons' turbo-electric generators for the supply of 3-phase alternating current at a voltage of 440 to 460

and at a periodicity of 50 ∿ per second. The exhaust steam from all these turbines passes into a 24-in. diameter main, and thence to an Edwards' evaporative condenser capable of dealing with 24,000 lbs. of steam per hour. An Edwards' 3-throw air-pump is provided, this being driven by a 3-phase induction motor. The condensing water is provided from a lake in the neighbourhood, and is circulated by means of centrifugal pumps, also driven by induction motors.

As compressed air is very much used in various processes about the works, three Riedler air compressors have been installed at the power station to supply this. One of these is of the steam-driven duplex compound type, for double-stage compression, and is of some 110 H.P. The other two compressors are of 90 H.P. and 60 H.P. respectively, and are of the duplex type for single-stage compression. The two latter are driven by 2-speed induction motors, by means of belts and countershafting. These compressors all deliver air at a pressure of 80 lbs. per sq. in.

The switchboard for the control of the electric power was built by the Westinghouse Company, of Pittsburg, U.S.A., and is entirely of white marble, with black oxidised apparatus mounted thereon. The board consists of twelve panels, each 24 in. wide. There are three generator panels, two exciter panels, one station panel, and six feeder panels. An indicating wattmeter reading to 1,600 k.w. is mounted on the station panel, also an integrating wattmeter of the same capacity. Integrating wattmeters are provided for all the feeder panels, and both generator and feeder panels are fitted with automatic circuit-breakers. The feeder cables are carried in a concrete culvert to a skeleton steel "cable tower" 35 ft. in height, from whence the transmission lines radiate in various directions to the points where the power is utilised.

The transmission lines are nearly all overhead, of bare copper wire carried on porcelain insulators. The poles are of steel, of tapered shape, having cast-iron bases with buckled footplates. Channel-steel cross-arms are used, the sizes varying with the size of line to be carried, the largest section used being 4 in. × 2 in. × ¼ in. Sections 3 in. × 2 in. × ¼ in. and smaller are also in use. Where the largest cables are shackled to the poles, small I-beams of section 4 in. × 2 in. × ¼ in. are used as cross-arms, being better suited to the strain than channel iron. The lines are placed side by side on the cross-arms. A preferable method of arranging these, as far as minimising line induction is concerned, would have been to have placed them equidistantly at the angles of a triangle, but this was not considered necessary owing to the short length of the lines and the increased cost this construction would have entailed. The maximum distance from the power station to any point where the power is used is about 1½ miles, and the lines are of sufficient size to prevent any serious drop in voltage. At the points where branches are taken off the main lines, "flying fuses" of a special type are inserted, both as a safeguard in the case of a short-circuit in the branch, and to isolate the branch if necessary.

As some trouble with lightning was anticipated, arresters of a very simple type have been fixed at numerous points on the overhead system. The arrester consists of a small cast-iron case containing a porcelain block, which has two small cylinders of non-arcing metal secured thereto. These are set at about $\frac{1}{32}$ inch apart. One of these cylinders is connected to the line wire and the other to an earth plate buried close to the point where the arrester is fixed.

Only on one occasion, however, has any appreciable damage been done by lightning, and that was before the arresters had been connected. Two recording instruments in the power station and one motor in a building at a little distance were severely damaged. As a protection to the main switchboard instruments, a more sensitive type of arrester is used, consisting of carbon rods mounted horizontally in pairs on a marble slab, at a few inches distance. One rod of each pair is connected to one of the bus bars ; the other, through a suitable water-resistance to earth. Small hooks of No. 36 S.W.G. silk-covered copper wire hang over the rods, being held in contact by small leaden weights pinched on to the ends of the wires. A very small electrostatic potential on the line is sufficient to spark through the silk insulation of the wire, discharging the line to earth. This type of arrester has been found perfectly reliable in parts of the Transvaal where lightning is dreaded by the electrical engineer.

The motors installed on the works are all of the Westinghouse induction type, having squirrel-cage rotors. In all there are installed some ninety motors, of many different sizes, from 1 H.P. to 90 H.P. With the exception of four of the largest size, these are all of the constant-speed type. The construction of the motor admits of the rotor being utilised as a fan, producing an excellent effect in keeping the conductor cool.

The polyphase induction motor may be started by connecting it directly to the circuit with an ordinary switch. Those in use at the works, up to 3 H.P., are so started in practice. The larger motors are started on a reduced voltage, the full E.M.F. of the circuit not being applied until the motors have approached their running speed.

The lower E.M.F. is obtained by the auto-starter, which consists of a double-throw switch mounted in a cast-iron case in which are also two auto-transformers. The case is filled with a special oil, which serves both to prevent sparking at the switch contacts, and to insulate the auto-transformers. When the switch is thrown in one position, the auto-transformers are connected across the circuits and deliver a low E.M.F. to the motor. When the switch is thrown in the opposite direction the auto-transformers are cut out and the motor is connected directly to the circuit. The wires from the power circuit are connected through suitable fuses and an ammeter, and, in many cases, an integrating wattmeter, to the auto-starter, and the same number of wires connect the auto-starter to the motor. The auto-transformers are arranged with loops so that one of several voltages may be applied for starting, thus adjusting the starting torque to the nature of the load

on the motor. In cases, the auto-starters may be placed at points more or less distant from the motors. This is a valuable feature, and is taken full advantage of when the motors have to be fixed in places not easily accessible.

Certain of the largest motors, namely, one of 60 H.P., two of 75 H.P., and one of 90 H.P., are of the two-speed type, the connections between the auto-starters and motors being so arranged that the number of poles in the stator may be changed.

The power used is measured by Thomson integrating wattmeters suitable for 3-phase balanced loads. One of these instruments is provided for each motor installation. They are provided with an artificial neutral point, the pressure coil of the meter being connected between this point and one of the lines, which is connected through the current coil of the meter.

Where motors are exposed to acid fumes, condensation of the acid on the coils is likely to occur which may eventually lead to breakdown of the insulation. To prevent this, in a part of the works where two 40-H.P. motors are together driving a large Roots blower and where they are much exposed, the expedient has been adopted of making the end casings of the motors air-tight and connecting them to a large air-trunk communicating with fresh air, by means of sheet-iron pipes. The air is drawn into the motors through the pipes by the fan-action of the rotor, and is expelled through holes in the periphery of the motor frame. This arrangement effectually keeps the motor cool, and, since fitted, no trouble has occurred. In certain sections of the works, a higher line voltage than 110 is prohibited, and some twenty-four of the motors and a large portion of the lighting are operated at this pressure. The remainder of the motors are operated at 440 volts. Much of the lighting is operated at 220 volts.

With the object of keeping up the voltage on the longest feeder, three "booster" transformers are inserted in that circuit at the power station, a double-throw switch being provided so that they may be switched in or out as necessary. The secondary windings are in series with the lines, and the primaries may be connected in either star or mesh combination, thus giving two different additional voltages to the circuit.

DUBLIN LOCAL SECTION.

SOME NOTES ON THE BREAKING OF TROLLEY WIRES.

By P. S. SHEARDOWN, Member.

(*Abstract of Paper read December* 6, 1906.)

With the overhead system of electric traction, there is always present the possibility of a trolley wire breaking and coming to the ground, which usually means a short circuit on the section, the temporary stoppage of the car service, to say nothing of the danger of electric shock to persons and animals, particularly to horses.

In the case of trolley wires erected with the ordinary soldered on ears, the wear is usually fairly uniform along the span up to the suspension where the wire close up to the ear will be found to be still more worn. This is due to the fact that the trolley wheel strikes the end of the ear a blow, the blow being caused partly by the ear more or less enveloping the wire, thus creating a greater diameter in the horizontal direction, and very often a slight unevenness in the under-running or path of the trolley wheel, and partly by the fact that the wheel after running on a comparatively flexible or yielding surface suddenly meets a rigid section in the ear. When once the trolley wire begins to wear or burr up at the ear, the blow becomes gradually more severe, owing to the formation of a slight hollow in the path of the wheel into which it runs, and in bad cases rebounds and leaves the wire, causing an open circuit spark. It then, with a certain amount of momentum, strikes the ear further on, often causing a second upward indentation at this place.

For more than ten years the author has given attention to the wear of trolley wire, and his experience in this direction is that there is far more potent action at work in weakening trolley wire than is accounted for by the reduction of cross-section due to wear, this weakness being undoubtedly due to molecular change in the copper at the point of suspension. This molecular change or crystallisation which takes place in the wire close up to the ear, or other point of suspension, is due to the fact that the trolley wire hanging between two suspensions is usually in a state of vibration. This vibration or ripple travelling along the wire is damped out or rebounds at the points of rigid suspension; consequently there is, just at the place where the trolley wire is

soldered to the ear, a continual stressing of the molecules due to one piece of wire being held rigid while the adjoining length of wire is free to move.

There is also a third action at work tending to weaken the wire, which, though akin to the trouble due to what the author would call crystallisation of the material, is not quite the same, and may be described as the hinging action which occurs at the place where the wire joins the ear, due to the upward pressure of the trolley wheel which is continually bending up the wire, this bending being most severe just at the butt of the ear.

We have therefore four actions taking place just at the critical spot where the wire joins the rigid suspension, all tending to weaken or deteriorate the material in the wire at this place. These are :—

1. The blow of the trolley wheel against the butt of the ear, part of which comes against the wire.
2. The effect of the sparking which occurs at the same place, due to the trolley wheel losing contact with the wire.
3. The molecular change or crystallising action in the wire, due to what the author has termed the damping out of vibration in the suspended wire.
4. The bending or hinging action due to the upward pressure of the passing trolley wheels (and possibly a fifth, namely, overheating of the wire when being soldered).

The actions taking place in the wire under 3 and 4 must be much the same as the action which takes place when a piece of wire is clamped in the jaws of a vice, and the wire is bent to and fro. After a certain amount of bending, depending upon the material in the wire, and the original physical conditions as to hardness, etc., the wire becomes short and breaks off; and this action explains why, in practically all cases of broken trolley wire, the fracture takes place close up to the point of suspension, or close up to a splicing tube, where No. 4, the bending action, is very severe on the wire. After wire has been up a certain time, it usually breaks first at the section insulators (where it is held most rigid), then at frogs and crossings, and finally at ordinary ears and at splicing tubes.

The author has seen many trolley wire breakages which have occurred at a full-size section, and he can only recall perhaps two cases in which the wire appeared to have broken from a pure tensile stress. In these cases the sections had pulled out thin before fracturing.

CONCLUSIONS.

First, in drawing up a specification for trolley wire, there is not very much to be gained in asking for a very high tensile strength only. What is required is some guarantee that it will be able to withstand in practice the bending and vibration stresses it will have to meet.

In connection with this matter it would not surprise the author if

the experience of different engineers varied considerably on this matter of trolley wire breakages. It is, for instance, quite conceivable that difference in pressure exerted on wires when being drawn through the die, might have a considerable effect on their power to withstand bending. The presence of an alloy has a marked effect, and in this respect the author would like to refer to a composite wire known as phono-electric trolley wire, which gives remarkably good results. On the system with which he is connected, the trolley wire was on certain routes subjected to very hard wear, and within two years of erection breakages so often occurred that it was replaced by phono-electric wire. This latter wire has now been up about six years and only two fractures are on record. There are only two disadvantages in this make of wire; first, it is somewhat dearer than hard drawn copper, and secondly, its electric conductivity is less than half that of copper.

The second conclusion is that trolley wire should be so erected that there is smooth under-running for the trolleys, and, if possible, it should be so suspended that vibration in the wire should be free to travel the whole length of the wire, and not hung so that as far as vibrations are concerned each span is insulated from the next, the vibration in each span of trolley wire being damped out or caused to rebound at the adjacent ears. What would appear to be a capital method on which to erect trolley wire would be first to erect a stranded steel suspension wire right along the route, and from this at frequent intervals suspend a grooved trolley wire by mechanical clips. The author made some experiments in this direction, though there is considerable difficulty to be overcome in ordinary street work where there are many curves, but this method has been developed in the United States under the name of the catenary suspension, which, besides giving smooth under-running, will probably much reduce the tendency to fracture.

The third conclusion to be drawn is that cars should be run with as little upward pressure of the trolley wheel against the wire as possible, in order to reduce the hinging action at the ears. With this action in mind, the author is of opinion that trolley wire, especially when suspended from span wires, should be erected fairly taut and the span wires left on the slack side, but he does not think the short so-called flexible suspensions on bracket arms are of much assistance, except that they make it possible to have better secondary insulation.

The fourth conclusion is that, other things being equal, the life of heavy wire in comparison with that of light wires, say, 0000 compared with 0, is not likely to be very much longer before breaking.

Lastly, as the fracturing referred to gives practically no external evidence that it is going to occur—indeed, in many cases takes place just inside the butt of the ear or under the frog clamps—careful inspection is of very little use, and the question to be asked on existing systems is what is best to be done, as naturally no one wants to renew the wire until wear makes it absolutely necessary.

The best way out of the difficulty in the author's opinion, and the one which he has adopted throughout the Dublin system, is to anchor the wire at all suspensions after it has been up two years, but at section insulators it is safest to anchor them at once. Anchoring the wire at section insulators, frogs, and crossings is a comparatively simple matter, but anchoring at the ordinary ear suspension is not as simple, if ordinary anchor ears are not put up in the first instance. One method would be to anchor the span wire or bracket arm, but this would mean the cost of nearly double the number of insulators throughout the system, to say nothing of the fact that the more insulators there are on a line the greater the chance of trouble with them. The method which the author has adopted is known as the K.Q. Patent Anchoring device. It was designed by two of his assistants, and is at once both cheap in first cost and simple to put up. It consists essentially of a stamped steel plate with a hole in it through which the threaded portion of the insulator bolt passes. The plate to which the anchor wires are fastened is thus securely held in position on top of the ear, but the strain of a broken wire comes direct on the bolt. The cost of this arrangement of anchoring erected complete costs about £14 per mile of double track.

It might be claimed that the wire would be liable to fracture at the end of the half anchor ear in the same way that it does at the ordinary ear, but the half anchor ear if properly designed will have a sufficient grip of the wire and yet give smooth under-running. Moreover, it is much shorter than an ordinary ear, and is free to move with the wire instead of itself being fastened to a support. In Dublin, anchoring has been employed at section insulators for about eight years, and at the present time every suspension is anchored except where the wire has been recently renewed. There are therefore about 8,000 of these half anchor ears in use, but on only two occasions has the wire failed outside a half-anchor ear.

When the wire breaks at a suspension, the anchoring device holds it in position, and as soon as it is noticed the emergency wagon is sent for and the men quickly make a temporary repair, and beyond the fact that a few trolley wheels may leave the wire, no inconvenience or stoppage to traffic is experienced. Although as many as twenty broken wires per month have been reported during the past twelve months, only three have come to the ground. In one case the suspension was not anchored; in another the wire was held in the anchor all right, but was afterwards pulled down by a trolley head getting caught; and in the third case the wire broke outside the anchoring device.

DUBLIN LOCAL SECTION.

NOTE ON SUCTION PRODUCER PLANT.

By A. E. PORTE, Member.

(*Paper read at Meeting of Section, January* 10, 1907.)

Simple producer gas is made using air only with incandescent coke or coal, and is sometimes called air gas. Water gas is made by driving steam or water vapour over incandescent coke or coal. Semi-water gas is the ideal producer gas, and consists of air and water gas in the proportions of air gas 2·88 volumes, and water gas 1·48 volumes. Taking simple producer gas, using air only, we have air consisting of :—

	Volume	Weight.
O	21 per cent....	20 per cent.
N	79 "	77 "

Carbon burned to CO occupies twice the volume of O which it contains; 100 volumes of air will yield 42 volumes CO, having a composition of CO $42 = 34·7$ per cent.; N $79 = 65·3$ per cent. The two gases have the same specific gravity, and hence the proportions of volume and weight will be the same. One thousand cubic feet of this gas will weigh 78 lbs., and one ton of C would give 190,000 cubic feet; assuming the coke to be 90 per cent. C, one ton will give 171,000 cubic feet of gas. The calorific power is $0·347 \times 4{,}320 = 1{,}499$ B.Th.U. per pound, and since 1,000 cubic feet = 78 lbs., this will evolve 116,925 B.Th.U. Again, 1 lb. of C combined with 1·33 lbs. of O = 2·33 lbs. CO + N for air = 6·71 lbs. of simple producer gas. Now, 1 lb. of C burned to CO_2 gives 14,500 B.Th.U.; 6·7 lbs. simple producer gas burned to CO_2 gives 10,058 B.Th.U.; the difference is 4,442 B.Th.U., or, say, 31 per cent. of the total heat energy of the fuel is lost in conversion into gas. The loss of heat is not rendered latent as in steam-raising, and it cannot be recovered on reversing the process; it is evolved in the producer, and is nearly all lost. The simple producer gas is practically never used with gas-engines, but is always mixed with water gas. If steam be driven over incandescent coal or coke it is decomposed, the reaction being $C + H_2O = CO + 2H$, which means that the steam yields its own volume of CO and of H. Every 1 lb. of C reduced to CO by steam yields the same volume as if reduced by air, but the CO, instead of being mixed with twice its own volume of N, which is a diluent, is mixed with its own volume of H, which is combustible, and of very high calorific power; the gas is therefore enriched by the added H, and at the same

time by the reduction of N. The decomposition of steam by the carbon consists of two separate operations, each having a separate thermal value; first, the formation of a molecule of CO; second, the decomposition of a molecule of H_2O. The first evolves and the second absorbs heat. For every pound of H liberated 6 lbs. of C will be consumed, and will produce 14 lbs. of CO. The thermal changes may be roughly represented as :—

Heat absorbed in dissociation of 1 lb. H from O of water	52,200 B.Th.U.
Heat evolved in combustion of 6 lbs. of C to CO ...	25,920 „
Heat absorbed by reaction for each 6 lbs. of C oxidised	26,280 „
Heat absorbed per pound of carbon oxidised by steam	4,380 „

Hence the quantity of steam which can be used with advantage in a producer is limited. The usual practice now is to draw in the air and water vapour together through the same pipe or pipes. The combustion of the carbon by air will supply the necessary heat to keep up the combustion, and since the combustion of C by air takes place at a much lower temperature than that required for the decomposition of steam, if excess of steam be used it will not stop the combustion altogether, but by lowering the temperature will cause the production of a large amount of CO_2, and at the same time the excess of steam will pass through without being decomposed. That a fairly high temperature is necessary for the production of good gas is evident from the following table, taken from Dr. Buntes' experiments :—

EFFECTS OF TEMPERATURE ON ACTION OF STEAM.

Temperature. C.	Temperature. F.	Per Cent. of Steam Decomposed.	Comparison of Gas—Volumes.		
			H.	CO.	CO_2
674	1,245	8·8	65·2	4·9	29·8
758	1,396	25·3	65·2	7·8	27·0
838	1,540	41·0	61·9	15·1	22·9
954	1,749	70·2	53·3	39·3	6·8
1,010	1,850	94·0	48·8	49·7	1·5
1,125	2,057	99·4	50·9	48·5	0·6

One lb. of C burned by steam absorbs 4,380 B.Th.U.; 1 lb. of C burned by air evolves 4,320 B.Th.U.; therefore, if there were no heat losses in the producer through radiation, hot gases, etc., a mixture of

about half and half would fill the bill. But we cannot avoid losses, and we must, therefore, use more air and burn extra carbon to keep up the balances. In practice it is found that the best proportion is about 4 of air to 1 of steam; this, however, varies considerably in different plants, and from this we get a gas of average quality which runs to, say:—

	Volume.	Weight.		B.Th.U. per lb.	B.Th.U.
CO ...	37·0 per cent.	= 39·73 per cent.	= 0·3973 ×	4,320 =	1,716·3
H ...	7·4 "	= 0·57 "	= 0·0057 ×	53,000 =	302·1
N ...	55·6 "	= 59·70 "	—	—	—
	100·0	100·00			2,018·4

1,000 cub. ft. of this gas will weigh 72·9 lbs., its calorific power = 147·141 B.Th.U. per cubic foot, or 2,018 B.Th.U. per pound. Again, 1 lb. of this gas will contain 0·1705 lb. of C, which, if completely burned to CO_2, will give 0·17 × 14,544 = 2,473 B.Th.U.; 1 lb. of this gas = 2,018·4 B.Th.U., the loss being 454·6 B.Th.U., which is about 20 per cent. of the value of the solid carbon, whereas if air only were used the loss would be 31 per cent., or showing a saving of 11 per cent. by using steam.

Generally speaking, the more steam that can be used the better as long as the temperature can be kept up, and hence it is of importance to economise all the possible heat and to return as much of the waste heat as possible to the producer in the form of preheating of air and vapour; at the same time an excess of steam must be carefully guarded against, since if the steam be decomposed it tends to lower the temperature, and if passed in the form of steam will decrease the calorific power of the resultant gas, as will be seen from the following table.*

EFFECT OF STEAM ON GAS.

	Moderate Excess of Steam.	Great Excess of Steam.	Maximum Quantity of Steam.
CO_2	5·30	8·90	15·0
CO	23·50	16·40	11·5
CH_4	3·30	2·55	1·9
H	13·14	18·60	24·6
C.P.	1,343	1,202	1,150·5
Temp. C.	800	700	500
Temp. F.	1,472	1,292	932

* *Minutes of Proceedings of the Institution of Civil Engineers*, vol. cxxiii. p. 328.

Steam has another advantage which is of importance with producers having grates or grids in the bottom through which air or steam must pass, and that is, the steam will keep the clinkers soft and porous, giving a more even distribution of heat and preventing chimneys (or blowholes) forming; it also prevents the fine ashes being blown up into the combustion zone, and the fusion of them on to the sides of the producer.

In summarising this question, I will quote an abstract from Raymond's paper, *Trans., A. Inst. M.E.*, vol. xx. p. 465: "(1) No possible use of steam can cause a gain of heat; if steam be introduced into a bed of incandescent carbon, it is decomposed into H and O. (2) The heat absorbed by the reduction of 1 lb. of steam to H and O is much greater in amount than the heat generated by the union of the O with C to form CO_2; hence the effect of steam upon a bed of incandescent fuel is to chill it. (3) The loss may be recovered if the H of the steam is subsequently burned to form steam again. Such a combustion of H is contemplated in the case of fuel gas as secured in the subsequent use of the gas. (4) The advantages to be secured consist principally in the transfer of heat from the lower side of the fire, where it is not wanted. The decomposition of steam below cools the fuel and grates, whereas a blast of air alone would produce at that point intense combustion (forming CO_2 at first), to the injury of the grate and fusing of the fuel, etc. (5) The proportion of steam most economical is not easily determined; the temperature of the steam itself, the nature of the fuel, air supply, and form of bottom affect the problem."

We will now glance at the question of the presence of CO_2 in the gas; it is considered as an indication of bad gas if we have more than 3 per cent., due either to defective design or operation. Either the CO_2 produced in the combustion zone is not reduced to CO in the decomposition zone, or the CO has been burned. The presence of CO_2 simply represents its own volume of CO uselessly burned in the producer, causing serious loss of heat and diluting the gas with a quite worthless constituent. Further, the extra O required to form CO_2 means so much more N present, so the gas suffers both ways.

1 lb. C burned to	CO_2	evolves	14,500	B.Th.U.
1 lb. „ „	CO	„	4,450	„
			10,050	„

Hence every pound of C represented by CO_2 in the gas means a loss of 10,050 units in the producer and a reduction in calorific power of the gas by approximately the same amount. Roughly speaking, we may assume that, if the producer gas contains 5·2 per cent. of CO_2, out of every pound of carbon is gasified 0·167 lbs. is absolutely wasted ($10{,}050 \times 0{\cdot}167 = 1{,}678$ B.Th.U., or 11 per cent.).

The heat losses in the producer must now be examined, and for

the purpose let us see what they are: (1) In ashes, if the ashpit be full of water, the heat loss on this may be very small, as the water is vaporised by the hot ash falling. (2) Losses in unburnt C falling through with ashes. (3) Loss in heating air and gases from 32° F. to temperature of delivery pipe, reducible by preheating air by waste heat and reducing quantity of air used to a minimum. (4) In latent heat of hydro-carbons; this loss does not affect us very much in the discussion of suction producers. (5) Loss in sensible heat of evolved gas. The gas leaves the producer at about 1,000° F., and must, therefore, carry over a large amount of heat; for our purpose this is all waste heat, as it has to be taken out by means of water washing. It will be realised from the following what this amounts to: 1 lb. of C will yield 6·7 lbs. of simple gas whose specific heat is $0·245 \times 6·7 = 1·641$ B.Th.U. per pound of fuel per 1° F. rise in temperature $\times$ 1,000° F. $= 1,641$ units, or 11 per cent. of the total calorific power of fuel. (6) Loss due to vaporisation of moisture in coal and formation of useless steam. (7) Loss in heating the undecomposed steam which passed through the fire. (8) Loss of heat in decomposition of steam. (9) Loss of heat in formation of CO_2, the most serious loss of all. (10) Loss of heat, solid carbon losses in soot and tar (practically nothing). (11) Loss of heat by radiation. We, therefore, have our heat balance:—

Dr.			Cr.
To heat per pound of coal—		*By heat per pound of coal—*	
1. From calorific power of fuel	A	Evolved in forming CO_2 ...	D
2. From heated air blast ...	B	Evolved in forming CO ...	E
3. From steam blast	C	In calorific power of gas ...	F
		Absorbed in decomposition of steam	G
		Lost in ash	H
		Lost in unburnt carbon ...	I
		Lost in tar and soot	J
		Lost in volatilising of hydro-carbons	K
		Lost in sensible heat of gas ...	L
		Lost in heating of undecomposed steam	M
		Lost in evaporating moisture in coal	N
		Lost in radiation	P

$$(A + B + C) = (D + E + F + G + H + I + J + K + L + M + N + P)$$

CALCULATION OF FIGURE OF MERIT.

	Cub. ft. of Gas.	B.Th.U. per cub. ft. of each Constituent.	B.Th.U. per cub. ft. of Mixture.	Carbon per cub. ft. in Constituent.	Lb. of C in 1 cub. ft. of Resultant.
CO_2	0·040	... ...	... ...	× 0·0335	= 0·00134
CO	0·254	× 342	= 86·86	× 0·0334	= 0·00748
CH_4	0·015	× 1,070	= 16·05	× 0·0337	= 0·00505
H	0·111	× 346	= 38·40	—	—
N	0·580	—	—	—	—
	1·000		141·31		0·01387

From this it is obvious that not more than about 80 cub. ft. of gas per pound of coal can be obtained.

Following from the consideration of the heat losses, we may now deal with the fuel itself. Broadly, any fuel will do for making gas. It is only a question of designing a special producer to suit it, but as we generally use coal, let us devote our attention to this. Coal should be purchased from analysis which should represent the bulk. For engine and metallurgical work absence of sulphur is important. The coal should be kept dry and clean—dry to save the cost of evaporating useless moisture, and clean to prevent foreign bodies being gasified and possibly spoiling the mixture.

A great deal has been said and written about the size of coal to be used in a producer, and its regularity. The size should be fairly even. If large pieces are in the body of the coal it will certainly feed irregularly, and the smaller pieces burning more rapidly may tend to cause blowholes, and even let the steam and air rush through cavities and burn up both coal and gas. At the same time, we must not run away with the idea that an odd chip will do any harm, or that the coal must be graded to a fraction of an inch. If the producer be small, it is important to keep the coal regular and small in size, but in a large producer it is not so very important. If the average run of beans be, say, ⅜ in. to ¾ in., pieces up to 2 in. by 1 in. will not do any serious damage. The effect of the size of fuel on the reduction of CO_2 to CO was fully investigated by Boudouard.* I may quote a few of his results which are of interest to us. A known quantity of CO_2 was enclosed in a porcelain tube in the presence of C, and the tube raised to a temperature 800° C. for eight minutes (1,470° F.):—

	Volumes per cent. CO_2		CO
Finely divided carbon	13·6	...	86·4
Wood charcoal pieces, 2 mm. to 5 mm. cube ...	39·9	...	60·1
Coke " " " ...	79·1	...	20·9
Gas carbon . " " " ...	80·1	...	19·9

From this it is evident that the more porous the carbon the easier

* *Annales de Chimie et de Physique*, vol. 24, 1901, Series vii., pp. 5–85.

is the reduction of CO_2 to CO; the gas carbon and coke being very dense, the CO formed only amounts to 19·9 and 20·9 per cent. respectively. Since the gases move rapidly in a producer, it is important that the fuel should be as porous as possible. The exact size is a matter of experiment, but Mr. Boudouard experimented on the subject, and I give the results :—

	Volume per cent. CO_2	CO
Wood charcoal 2 mm. to 5 mm.	39·9 ..	60·1
" " size of a nut (hazel) ...	17·1 ...	82·9
Coke, 2 mm. to 5 mm....	79·1 ...	20·9
" (hazel nut)	83·6 ...	16·4
Gas carbon, 2 mm. to 5 mm....	80·1 ...	19·9
" " (hazel nut)	86·7 ...	13·3

From these experiments we conclude that, other things being equal, the smaller the size of the fuel the better the result. One of the main determining factors in the size of fuel is the suction pressure required to work the producer, which acts the same as a back pressure on the engine. Also, it must be noted that if the gas be drawn rapidly through the producer, the percentage of CO_2 is always higher than if the movement be slow; this, of course, means increasing the area and diminishing the height of the producer.

The actual composition of gas produced by the action of steam on carbon at different temperatures is shown by Boudouard to be :—

Temp. C.	Per cent. of Steam Decomposed.	Composition in Vols.		
		H	CO	CO_2
674	8·8	65·2	4·9	29·8
758	25·3	65·2	7·8	27·0
838	41·0	61·9	15·1	22·9
954	70·2	63·3	39·3	6·8
1,010	94·0	48·8	49·7	1·5
1,060	93·0	50·7	48·0	1·3
1,125	99·4	50·9	48·5	0·6

From this it is evident that any temperature less than 1,000° C. is insufficient to produce gas of high calorific value; it will, further, allow the reaction $CO_2 + C = 2CO$, and will prevent $CO + H_2O = CO_2 + H_2$. In selecting a coal, having examined the analysis and calorific value, ascertain the percentage of ash and clinker. It is well to make sure

that the clinker is not of such a nature that it is likely to cling to the brick lining, and will be soft and easily dislodged when cleaning becomes necessary. Sometimes the clinker refuses to leave the bricks, and pieces of the brick come away in cleaning. Otherwise good coal should be rejected for this fault, as the relining of a producer is expensive (£25 for a 50-B.H.P. producer is about the cost), to say nothing of throwing the plant out of service. Of course, the final criterion of a good coal is good clean gas and little attention.

Producer gas appears to have been first used in connection with the gas engine in 1879. The thermo-dynamic theory of the engine was not then fully appreciated; whereas according to D. K. Clark's tests at the smoke abatement exhibition, 1882, 110·34 cubic feet of gas was consumed per indicated horse-power, and 149·34 cubic feet per brake horse-power, to-day we can easily get 1 I.H.P. for 65 cubic feet, and 1 B.H.P. for 75 cubic feet. To Mr. Dugald Clark we are largely indebted for careful researches into the problems involved. Of the total heat energy given to the gas engine in 1882, 16 per cent. was represented on the indicator card, and in 1900 30 per cent. was indicated. In the best engines to-day 30 per cent. of the heat energy is taken up in the cooling water, 40 per cent. is lost on the exhaust, the remaining 30 per cent. being useful work. There is nothing more striking than the change in the compression. While in 1881 35 lbs. was considered a good compression, we see compressions to-day ranging up to 170 lbs. per sq. in. on engines run with producer gas. The products of combustion which remain in the cylinder after exhaust should, if possible, be expelled from the cylinder. The entering producer gas, being of low thermal value, loses by being further diluted with the exhaust gas; while if fresh air were admitted it would contain oxygen, which would be of value in helping the explosion. Again, the remaining exhaust gases are very hot, and to clear them out tends to cool the cylinder. The entering gas would be of greater density, and consequently of greater thermal value per cubic foot, so that greater power might be got out of the engine for a given size of cylinder. This cleaning out of exhaust gases, called scavenging, is, however, not universally adopted. The amount of water supplied to the vaporiser is, as we have seen, of the greatest importance. This should be in proportion to the load at any time, and is very difficult of arrangement. It is almost impossible to govern the water supply. In some plants the water trickles on to a hot-plate, where it flashes into steam, and the amount of water can be governed in such a case. Many schemes have been devised to regulate the supply of water to the vaporiser other than the flashing system referred to. I will quote two: First, the Watt system, which has a diaphragm of indiarubber on top of the scrubber, and from this by a system of levers the admission of water to the vaporiser is regulated, each suck of the engine drawing down the regulating diaphragm and opening a small tap, which allows a certain amount of water to be delivered. With such a system water can only be delivered at each suction stroke of engine, the cock being shut off

meantime. Another arrangement, which I have seen described, consists of a regulator worked by the air suction. There are, no doubt, many other systems of regulation, but the couple we have considered will be sufficient for our purpose.

Efficiency.—We may now turn our attention to some tests on suction producer plants. Proceeding scientifically, we should first ascertain the heat efficiency of the producer, which is a fairly simple process. It is not, however, generally necessary to know the heat efficiency of a producer. The composition of the gas in producer plants is said to vary constantly during the run, but that this may be kept fairly regular is shown by a diagram in the book by Dowson and Larter,* giving the results of a test on a 30-B.H.P. suction plant. In considering the efficiency of a suction producer plant it must be remembered that, owing to the piston of the engine having to act as a suction pump, the gas and air, when drawn into the cylinder, are at a pressure below atmospheric; the mixed gases are consequently at a less density than gas supplied on pressure system or from the town mains, and, therefore, other things being equal, of less thermo-dynamic value. This is a matter of some importance, for we must face the following losses: (*a*) extra work on engine sucking in charge (this we can estimate exactly from a card taken off engine); (*b*) reduced power of gas (this can be determined by an analysis of the gas itself). This means that the engine must be made larger than for town gas; how much larger we shall presently see. Let us first examine a card taken off an engine working on suction. Here the suction is equal to 3 lbs. below atmosphere, which is the same as taking 3 lbs. off the average pressure. Since the suction of the engine keeps the fire in the producer going, it may be asked what will happen when the engine is working at low loads. Take a case in point. An engine of 30 H.P., having a rated speed of 250 r.p.m., will take something in the order of 120 sucks of gas per minute at full load, or two per second. If we have a large expansion box and, say, 12 ft. to 20 ft. of gaspipe, for all practical purposes there will be a continuous draught on the fire. Now, supposing we take the same engine running light, it would probably take 18 sucks per minute, or, roughly, one-seventh of the full-load draught. With this diminished draught we must have a falling temperature, and with a falling temperature, as we have previously seen, we must expect an increase of CO_2, with a corresponding decrease of CO and H. I give an actual test quoted by Mr. Dowson:—

	Full Load, per cent.	No Load, per cent.
CO	27·65	22·4
H	9·85	7·0
CO_2	3·80	4·9
O	0·30	0·5
N	58·40	55·2
	100·00	100·0 vols.
	Calorific Power.	
B.Th.U.	128·9	101·0

* "Producer Gas," p. 131.

44

45

46

47

[redacted]

It will thus be evident that there is a loss in calorific power of almost 28 per cent., which will at once upset the balance of the mixture, and the engine might and probably would shut down if not attended to by regulating the air supply to it, and also possibly by reducing the water-vapour supply to the producer. If the air supply be heated, say, by the exhaust, it helps to prevent the fire temperature falling very much. The falling-off of power is shown on the accompanying cards (Fig. 1), taken on a no-load run :—

Card No.	Time.	Mean Pressure.
—	2.50 p.m., load off	—
44	3.5 " "	76
45	3.15 " "	74
46	3.25 " "	74
47	3.45 " "	43·5
48	4.00 " full load	Decreasing
49	—	Increasing
50	—	84·5

48, 49, 50 were taken immediately the load was full on. In 48 the card shows weakening explosions, 49 strengthening explosions, and 50 shows

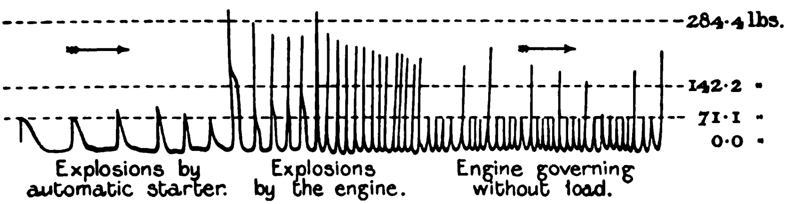


FIG. 2.—Mathot Explosion Record.

a normal full-load card. 48 is particularly interesting, as showing that the first effect of the increased draught was the introduction of too much steam into a cool fire, and a consequent falling of power, to be soon recovered, however, as the air draught being four times as great as the steam caused the fire to burn out. The case I have cited is, of course, a very extreme case—that is, throwing full load on to an engine which had been running on no load for some time, and it seldom happens in practice that the full load is thrown on all at once ; it is usually put on in parts and gives the producer a chance to recover itself.

Mathot's Explosion Recorder.—Mr. Mathot, a Belgian engineer, is a recognised authority on all matters connected with internal-combustion engines. We have seen that the explosions immediately following one another are of different value ; it therefore becomes necessary to take several diagrams overlapping on each card so as to arrive at an average. There are indicators made which do this by means of a clockwork drum in place of the usual drum on which the record paper is fixed ; such an apparatus is invaluable if a careful analysis of the cards must be made for any purpose. Each diagram follows its neighbour, leaving a space of probably $\frac{1}{16}$ in., so the records can be easily followed. These apparatus are, however, limited in use, as the

drum must be very large to take many records, and this means that there would be considerable weight to be moved, and the inertia of the system is an objection. If continuous records are required, the Mathot explosion recorder is very useful, and can be attached to any indicator. All that is necessary is to have a clockwork drum attached to the indicator by a swing arm, which enables the explosion recorder to be thrown in when required without interfering with the ordinary indications. The explosion record tells us the value of the suction, compression, and explosion, and, what is also very important, the accuracy or otherwise of the governing. Presuming we start up with our recorder on the engine, we learn what is happening. Take, for example, Fig. 2, which is taken from an engine with an automatic starter supplied with mixed gases at atmospheric pressure. The value of these explosions, and the rate of acceleration can be observed at once. Six charges were fired by the automatic starter before the engine took the business in hand, and it will be seen that the first explosions are much more powerful than those following. In this case the engine took 19 explosions before attaining full speed, and the compression was thrown in. The speed of rotation of the drum can be altered so as to pull out the record as much as desired, but this will give fewer records on the drum. A continuous band of paper is sometimes used, but this means three drums and the apparatus becomes cumbersome. The drum usually makes one complete revolution in two minutes. We might study many of these records and glean information, but time will not permit our following out the most interesting subject any further now.

Stand-by losses are always a matter of much interest to engineers, and there is scarcely any part of the day's work that there is such doubt about. Mr. Dowson cites a case of 250-H.P. plant standing for nine hours and only consuming 46 lbs. of coal in nine hours. This appears very low, but it can be understood if the whole plant were allowed to cool considerably during stand-by; in other words, if the draught was stopped and the fire probably just kept alive. This is about one-tenth of the coal required to keep a steam boiler of the same power banked. I have made several tests on the stand-by losses in suction producers, and have found them to vary so much that no rule can be laid down; probably 0·5 lb. per brake horse-power for 10 hours would be a safe average. In one case I had returns kept of stand-by losses on a 100-H.P. plant, and by constant attention got the loss on a 14-hour stand-by down from 120 lbs. to about 50 lbs., or $=35·7$ lbs. per 10 hours on 100-H.P. plant $= 0·357$ per horse-power for 10 hours, 0·035 per horse-power hour. In this case the blow-off or escape chimney is about 40 ft. high, so the tendency is naturally to keep up a good draught, if the air and escape valves are not kept pretty tightly closed. In another case, a 70-H.P. plant, the records show only 17 lbs. stand-by on 10 hours, or 0·024 lbs. per horse-power hour. In this case the chimney is only about 12 ft. high, so the draught is not so great; still, the figure is very low indeed, and requires verifying. When blowing up a

producer plant, care must be taken that the engine crank is in such a position that the air and gas valves are closed, or otherwise there is a risk of blowing the gas into the engine-room from the air valve and pipe. The presence of a high percentage of hydrogen in producer gas tends to cause pre-ignition, and it has been found that under those conditions the compression cannot be carried safely beyond 120 lbs., and requires about 11,500 B.Th.U. per B.H.P., while gas which is rich in CO and poor in H will not be liable to pre-ignition, and the compression may be carried up to 170 lbs. or over safely, requiring only 9,500 B.Th.U. per brake horse-power. Hence it is well to keep the percentage of H fairly low.

We will now look at some tests taken here last year on a suction pressure plant. The plant consists of an Industrial suction producer, Stockport engine, and Industrial dynamo direct-coupled; plant about 70 H.P. The tests ran two days. The first test was to determine the friction load of engine with dynamo not excited, and this worked out at 18·9 I.H.P. This looks a little high, but the engine had three bearings and the dynamo two. The dynamo was also a large machine for its output. The dynamo was rated at 33·75 k.w. at 220 r.p.m. The next test was at 33 k.w. The indicator card worked out at 66·2 H.P., and the gas consumption tested by using one of our holders gave 46 cub. ft. per indicated horse-power and 92 per kilowatt delivered at switchboard. This was satisfactory so far. A second gas consumption test was taken with the other gasholder, and in this case we got 28·05 k.w., 58·5 I.H.P., 60 cub. ft. per indicated horse-power 115 per kilowatt. The average of these will be 53 cub. ft. per indicated horse-power and 103 cub. ft. per kilowatt at switchboard. Our next test was taken at 29·9 k.w. to determine the coal consumption, which worked out to 0·8 lb. per indicated horse-power and 1·4 lbs. per kilowatt delivered at switchboard. Some alterations were made in the plant, and an 8 hours' non-stop run was made on another occasion, and for 4 hours the engine was taking every explosion, so was at full load. This test was particularly to determine the coal consumption under new conditions, which worked out as follows: 5.55 p.m.—hopper filled up, 31½ lbs. put in, load 32·55 k.w., speed 220 r.p.m.; 6.50 p.m.—charged hopper, 31½ lbs. coal (55 minutes' interval), same load; 7.45—charged hopper, 31½ lbs. coal (55 minutes' interval). It will be noticed that exactly the same intervals of time show that the producer was working quite regularly. This works out at 0·68 lb. of coal per brake horse-power (a figure that is exceedingly low) and 1·06 per kilowatt delivered at switchboard. The producer plant in this case, although well able to keep the engine going at normal load, was not quite up to the overload test, and had to be replaced by a larger one, which gave between 25 and 30 per cent. spare power. The exact figures are: the plant was specified to make 4,000 cub. ft. of gas per hour, and it made sufficient gas to keep the plant running for 4 hours, developing, roughly, 39 k.w. to 40 k.w. at switchboard, say 35·9 × 103 = 4,068 cub. ft. per hour, and, at the same time, raised the large holder 24¼ ins., representing 1,104 cub. ft.,

showing a liberal margin in hand. The analysis of the gas was not quite as good as one might expect with such low consumption, and was from an average of four samples (see Table below).

Having now considered in a somewhat incomplete manner the fundamental principles on which the suction producer operates, let us look at its infirmities. In the first place, there is often great difficulty in starting the engine. Having made sure that everything about the gas plant is in order, and that the gas being given off is of proper quality, we proceed to start our engine. This is usually done with a compression starter of some sort—either a hand-pump which forces the gas and air into the cylinder under pressure, or a pump which fills a large receiver with gas and air at atmospheric pressure, or some other device. The piston having been brought to the proper position for starting, the magneto is operated by hand, and the charge is fired. Should the magneto fail to fire the charge, usually the contact in the

	Vol.	Sp. Gr.	W. Ft.	Cal. Power.
CO_2	6	—	—	—
CO	19·0	0·0181	1·5839	6,842
H	17·1	0·0056	0·9957	5,075
N	57·9	—	—	—
	100·0	—	—	11,917

$$\frac{11{,}917}{100} = 119{\cdot}1 \text{ B.Th.U. per cubic foot.}$$

explosion chamber will be found wet, forming a partial short-circuit. This fault is very common after the engine has been laid aside for a day or two, and the moisture of the last explosion is precipitated on the inside of the cylinder. This defect is easily remedied by drying the contacts. Sometimes, however, the charge fires the first shot, but the engine fails to pick up. This may be due to many causes; the damp on the contacts may again be the cause. The gas and air mixture may be wrong, or the gas may be weak. If the mixture be wrong, experiment is the only cure; if the gas be weak, the producer must be further blown up till the gas is right. In any case the trouble is very annoying, and oftentimes very difficult to get over. Once the engine gets away, the compression is put on. There is usually no further trouble, and if an engine is systematically difficult to start, I would recommend a petrol starter, for when once the engine starts, it will itself draw up the fire pretty quickly and settle down to work.

Having adjusted the air and gas for our load, the plant will take care of itself for a couple of hours, provided the load is not very variable; if it be, we may get into trouble. If the load falls off, the draught on the producer also falls off and the temperature of the fire goes down, with the result, as we should expect, of inferior gas, and the engine may fail to fire charges at proper time, or may back fire, and shut down. This is probably the most annoying and most difficult trick to overcome that gas plants are subject to. First let us assume that the plant is likely to be run at very light load for any length of time. In order to keep up the draught on the producer, the governor of the engine may be fitted with a step die, which will cause the engine to take a small charge of gas every cycle, but not enough to make an explosive mixture with the full supply of air. This arrangement generally meets the case—of course, if the plant be only running light for short periods, this will not be necessary. I need scarcely say the use of a step die does not help the efficiency of the plant in coal consumption.

The next difficulty (and it has its origin in a similar cause—namely, the variation in quality of gas) is if the gas has too much hydrogen. Let us say the average quality of gas was CO 24 per cent., H 8 per cent., and that from an excess of steam we get CO 15 per cent., H 17 per cent.—which is not a bad gas at all. Pre-ignition might occur if the compression were fairly high and the cylinder hot. As H is very likely to pre-ignite if the conditions are favourable, the result is just the same—the engine will slow down if not relieved of some load. When the load is taken off, generally speaking, air and gas adjustment set things right, but if at an increase of load, and nearly at the limit of power, it is almost impossible to stop the pre-ignition, and, therefore, we must try and guard against it by keeping the cylinder as cool as possible. Whereas with town gas the circulation water may safely attain a temperature of 120° F. on the hot side, with producer gas the hot circulation water should not exceed 110° F. We must also keep the quality of the gas as constant as possible by effective stoking. Most producer makers tell us that a producer requires no attention. That may be true within certain limits, but for very variable loads a producer requires attention every couple of hours at the very outside. We have seen that it is possible to get blowholes or cavities up through the combustion zone. This can be prevented by stoking fairly often and keeping the level of coal in the producer constant by filling in coal, say, every two hours. This keeps the pressure of material fairly constant, and tends to prevent cavities forming and irregular gas being generated. Also, the engine attendant knows from his governor what work the engine is doing, and he can regulate the water supply accordingly—that is, in the absence of any reliable automatic device. Another cause of pre-ignition is supposed to be due to particles of free carbon in the cylinder, or a deposit of fine soot on the back of the piston; for example, the slightest little excrescence will form a very suitable point for firing, as it must be remembered that the piston is about the hottest part of the engine, and a slight excess of heat due to compression will

set one of these spots of carbon alight and fire the charge. This fault is not common if perfectly pure anthracite coal be used. Sometimes the magneto for some reason fails, and we get imperfect ignition. This manifests itself by irregular running, and the simple test is to try the magneto by detaching the connection from the sparking plug and fire by hand if it is all right. Next try the plug, and make sure it is sparking in the cylinder. All engines should be fitted with a peep-hole for this purpose. It may also happen that the spark requires advancing or retarding; most engines have an adjustment for this purpose.

The next cause of trouble, and I am afraid there is no cure for it, is that almost every manufacturer overrates the power of his engines. If you ask the maker for 100-B.H.P. engine on suction gas, he will probably quote you for an engine that will give 100 B.H.P. at his own works and not at yours—that is to say, the engine will undoubtedly give the power under test conditions, but not under ordinary commercial conditions. My idea is that at normal full load the engine should miss 25 per cent. and fire 75 per cent. of the charges. This leaves a fair margin to govern on, and if the engine be running at rated full load a slight variation in quality of gas will not mean a stop; for if the gas be rushed through the producer, it is very likely, unless most carefully handled, to come over very hot, and may even heat up the scrubber to such an extent that the gas cannot be cooled to atmospheric temperature, and, as a consequence, full power will not be obtained. Suppose the temperature of the air be 60° F., and the scrubber is passing gas at 400° F., the calorific value of the gas will only be six-tenths of its value at temperature of air; consequently the engine could not possibly give its power, also the gas would not be clean, so it is of the first importance to have the plant and scrubber big enough and an ample supply of clean water. The amount of water required for scrubbing varies very much with different makers, from 1½ gallons per brake horse-power to 3 gallons—probably 2 gallons per brake horse-power would be a fair average. I have recently tested a plant taking just 1¼ gallons per brake horse-power hour, and the plant was delivering quite cold gas. As I am speaking of cooling water, let me say just a word about cylinder cooling water. This should be free from lime if possible, otherwise the jacket will soon get blocked up and the cylinder cannot be kept cool. I have now enumerated some of the troubles which I have met myself; of course, other users have met other troubles, which I hope they will tell us all about.

I wish to acknowledge my indebtedness to Mr. Emerson Dowson who kindly lent me the original drawings of the apparatus.

Discussion.

The Chairman.

The CHAIRMAN (Mr. T. Tomlinson): I have found, generally speaking, that the chief trouble experienced with plants of this nature is due

to irregularities in the quality and thermal value of the gas generated. I remember particularly one producer, of the Dowson type, which the expert left without regulating the water supply to producer. I tried every means within my knowledge to get the plant to work satisfactorily, but without success; finally I had to send for the expert, who, by simply adjusting the height of water in producer, removed all source of complaint. A trouble often experienced with this type of plant is owing to the water nozzles which feed the producer becoming blocked, and this is a matter which requires frequent attention. I think the success of the Dowson plant is largely due to the simple method of water adjustment. The effect of suddenly changing from a small load to a large one was at one time very real, but that has now been overcome to a very large extent. There is absolutely no doubt as to fuel economy as compared with steam plant, although the plant is not quite so reliable as one could wish. I think that such a plant installed in an electric lighting station for the purposes of taking the "peak load" would result in considerable economy, as the "stand-by" losses are insignificant compared with steam plant. I am of opinion that means of storing the gas should be provided, and taking a hypothetical case—say a small lighting station—and working out the size of the plant necessary to make and store sufficient gas in the course of, say, twenty-four hours, it is suprising how small a plant will do the work; but how far one can go in that direction, of course, is a purely financial question, depending upon the cost of the gasholders, etc. Producer gas, although not of high calorific value, is more economical for use in gas engines, owing to the fact that very much higher compression can be obtained with it in gas engines, and consequently increased thermal efficiency, than would be the case with, say, town gas. In conclusion, I am not so sure that gas-engine makers do overrate the power of the engines they sell. I think that very often the user underestimates what he needs, and then condemns the enginemaker. The Chairman.

Mr. Wm. Tatlow: My experience of suction plants is that they are likely to fail when momentarily overloaded, owing to the reduction in the calorific power of the gas. In order to overcome this difficulty, I have thought of trying an arrangement whereby, in the event of the engine slowing down unduly, the engine governor would cause to be injected into the expansion box a few drops of some volatile hydrocarbon, thus enriching the poor gas and keeping the engine going to tide over the interval of bad gas until the producer fires recover. Such a contrivance would be particularly useful where the plant is used for driving circular saws, and in any case where the conditions are severe. I think that producers are being steadily improved, and will be more reliable in the future. Another point worthy of consideration is that some of the cheaper qualities of Irish coal contain a considerable proportion of sulphur, and cannot be used in producers owing to the sulphur condensing and forming sulphurous acid, which attacks the pistons and cylinders. To get over this difficulty I suggest the use of Mr. Tatlow.

Mr. Tatlow. lime water in the scrubber. Sulphur-bearing coals can be got cheap, because people will not use them on account of the difficulty referred to. I should like to ask Mr. Porte whether in his experience a gas plant uses less water than an equivalent non-condensing steam plant? As regards the simplicity of working producer plant, I know of a plant near Wicklow which is run solely by two women.

Mr. Sowter. Mr. W. J. U. SOWTER: As a central station engineer the subject has a particular fascination for me. I have looked into the matter pretty closely, but think it is not politic to put down a plant which can be used with practically only one particular class of coal, for obvious reasons. I suggest that the combination of steam and gas plant in a central station would be economical, because much of the heat wasted in the gas blast could be utilised on the steam side, and there would be very little waste heat. There are large "stand-by" losses in steam-driven stations, and these can be greatly reduced by the use of gas plant. Gas plant, however, must be absolutely reliable before it can be installed to any extent in public supply works. Another important consideration, too, is the capital cost of the plant.

Mr. Sheardown. Mr. P. S. SHEARDOWN: I think that the difficulty of varying the water and air supply to the producer can be easily got over by controlling the admission by the engine governor, and I really cannot see where the great difficulties come in, although I have never run a producer myself. Personally I should feel nervous if I had to start up a lot of gas engines with the load coming on rapidly. Will the author kindly say why it is that very large gas engines are reputed to give so much more trouble from back-firing than small engines? It has been stated that very large gas engines are less efficient than small ones. Is this due to the fact that the large engines cannot work at so high a compression owing to the back-firing difficulty?

Mr. Taylor. Mr. J. TAYLOR: Are any of the responsible engineers present prepared to consider seriously the installing of large gas plants? I notice that most of the people who use gas plants are not engineers at all, which in itself is suspicious. I recently inspected a plant having six 500-H.P. engines in Scotland, and the experience was sufficient to last me a lifetime—the noise, heat, smell, and chattering were appalling. I am told that a 1,000 H.P. Koerting engine—another plant I have inspected—works fairly satisfactorily at half to three-quarter load, but if it were run at full load for any length of time it would be speedily knocked to pieces! I think no sane engineer would go in for gas driving on a large scale at the present time.

Mr. Kettle. Mr. L. KETTLE: Are the figures distributed broadcast by the engine makers founded on facts or only tests? I anticipate considerable trouble if, say, three or four 1,000-H.P. gas-driven alternators are running in parallel and one or more starts back-firing.

Mr. Porte. Mr. A. E. PORTE (*in reply*): Referring to Mr. Tomlinson's remarks, the stopping up of the water nozzles is one of the most serious defects in the type of plant alluded to; many other methods of regulating the water supply are in vogue, some of which are fairly successful.

Regarding the use of gasholders, I have laid down one plant where there were two old existing gasholders which were utilised, and proved valuable additions to the plant in various ways. Referring to Mr. Tatlow's suggestion to inject a small quantity of hydro-carbon to act as a "pick-me-up" to a failing gas engine, I have thought of trying that method, but the gas makers seem to object, so that I have not so far tried the experiment. I think the addition of lime water to the purifiers to get over the sulphur difficulty well worth trying. The usual quantity of water consumed by a gas plant is 15 lbs. per brake horse-power hour. In reply to Mr. Sowter's remarks, peat can be used in producers, but as peat usually contains some 90 per cent. of water, means would have to be taken to reduce the water to 40 or 50 per cent. before the peat could be used for gas making. Mr. Taylor is somewhat pessimistic regarding gas plants. I have had a good deal to do with gas engines, and certainly my experience does not coincide with his. I could mention a mill where the steam plant has been taken out and two 100-H.P. gas engines substituted. The engines are looked after by the man who formerly ran the steam plant, and no stoppages have occurred. A saving of £400 per annum is effected by the change. Regarding the statement that engine makers do overrate their plant, I am sure that is so. A short time since I purchased a 100-B.H.P. engine, and the most I can get out of it is 100·3 B.H.P. I consider there should be a margin of not less than 25 per cent. Mr. Porte.

NEWCASTLE LOCAL SECTION.

THE ELECTRIC POWER INSTALLATION AT GRANGESBERG IRON MINES, SWEDEN.

By G. Ralph, Member.

(Abstract of Paper read at Meeting, February 4, 1907.)

The Grangesberg Iron Mines belong to the largest iron mining company in Sweden, and are situated some 200 miles north-west of Stockholm. Many thousands of tons of ore have been obtained from the surface workings, which have now reached a depth of over 300 ft. Shafts have been sunk at the foot of the hill to a depth of about 600 ft., and will be deepened gradually as the vein is worked out and new cross roads are required to meet it at lower levels, till a final depth of about 1,800 ft. is reached.

The power-station is situated at a waterfall about 18 miles away, and three-phase current is transmitted to the mine at 8,500 volts pressure by an overhead line. (It may be mentioned that in all the electrical transmission of power schemes in the country overhead wires are employed.) The high-tension current is transformed down to 430 volts at the mine sub-station. A synchronous motor coupled to a direct-current generator is installed to provide continuous-current supply for the winding engine. This set is of about 400 H.P., and the reason stated for the employment of a motor-generator instead of a rotary converter is that the comparatively high periodicity of 70 is used. Also, the over-excited synchronous motor is said to improve the power factor somewhat. This motor-generator runs continuously, charging a battery of 243 Tudor cells having a capacity of 600 ampere-hours at a very high discharge rate. They were found on test, however, to be capable of a discharge of 900 ampere-hours. The battery is divided into three sections, arrangements being made to charge two sections only at a time. The battery operates in parallel with the motor-generator when winding is in progress.

The winding machinery is the most interesting part of the installation. The winding drums (Figs. 1 and 1*a*) are of the tapering spiral type, frequently used both here and abroad to compensate for the weight of the ropes and to facilitate acceleration. On arriving at the pit-head gear, the bucket is automatically tipped by curved guides, and the material is delivered to the travelling sorting belts driven by

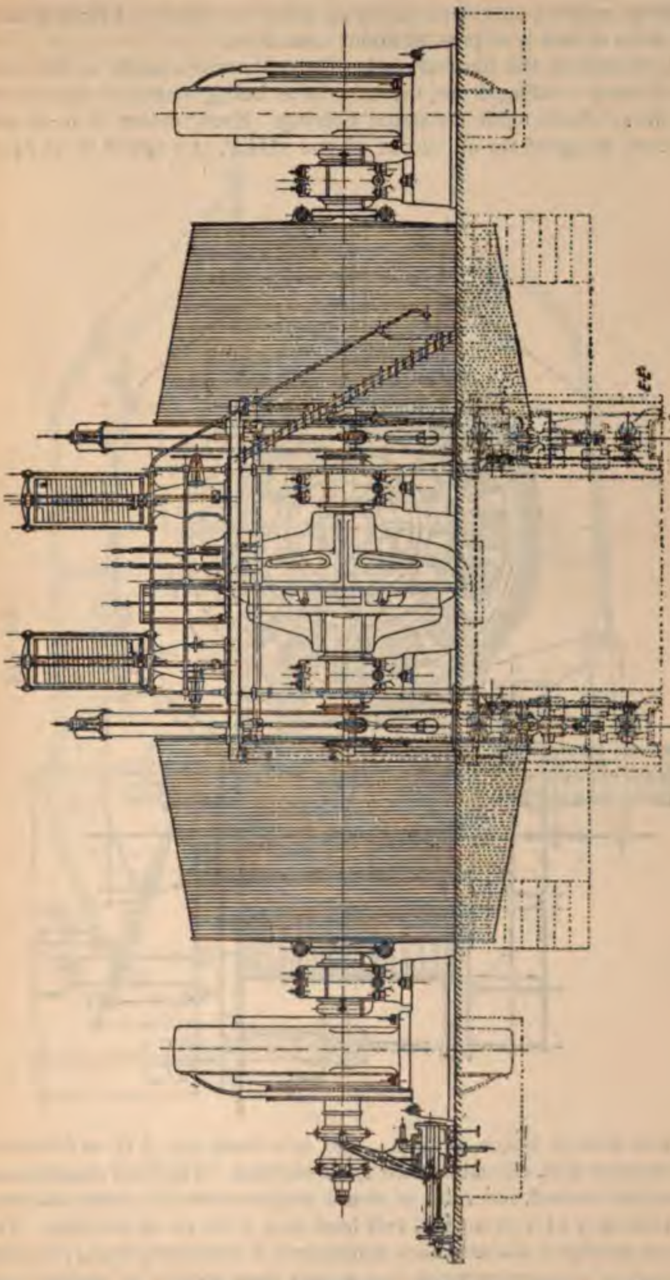


FIG. 1.

induction motors, as is practically all other machinery at the pit-head. The daily output is at present about 1,200 tons.

As shown by the illustration in Fig. 1, there is a motor at the outer end of each winding drum, the armatures being mounted direct upon the drum shafts with no outer bearing. Each motor is a 16-pole machine, designed for an output of 500 B.H.P. at a speed of 38 r.p.m.

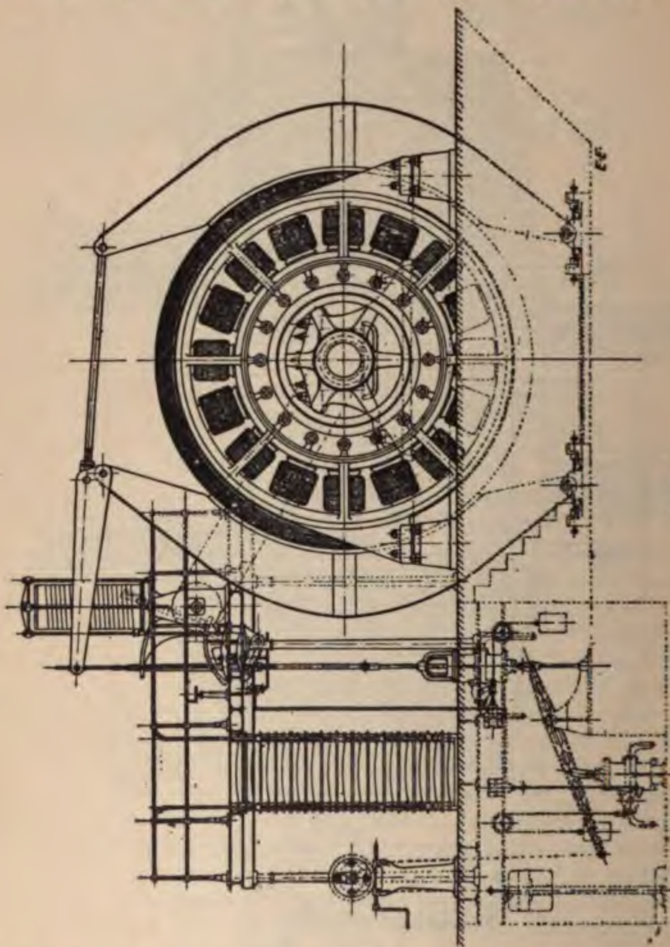


FIG. 1d.

The two motors weigh 45 tons. The armatures are 8 ft. in diameter, commutators 6 ft. diameter, with 1,248 sections. The field magnets are compound wound, the ratio of shunt ampere-turns to series ampere-turns being 3 to 2 at normal full load and 9 to 11 at starting. The current density in the armature conductors is extremely high, reaching the figure of 10,000 amperes per square inch section at starting. At

the present time, now that winding is done from a comparatively shallow shaft, the two machines are coupled in series, giving a speed of only 19 r.p.m. They will be coupled in parallel and the speed doubled when the shaft is deepened. The controlling apparatus, which is shown in Fig. 3, is extremely simple to manipulate, and seems to be

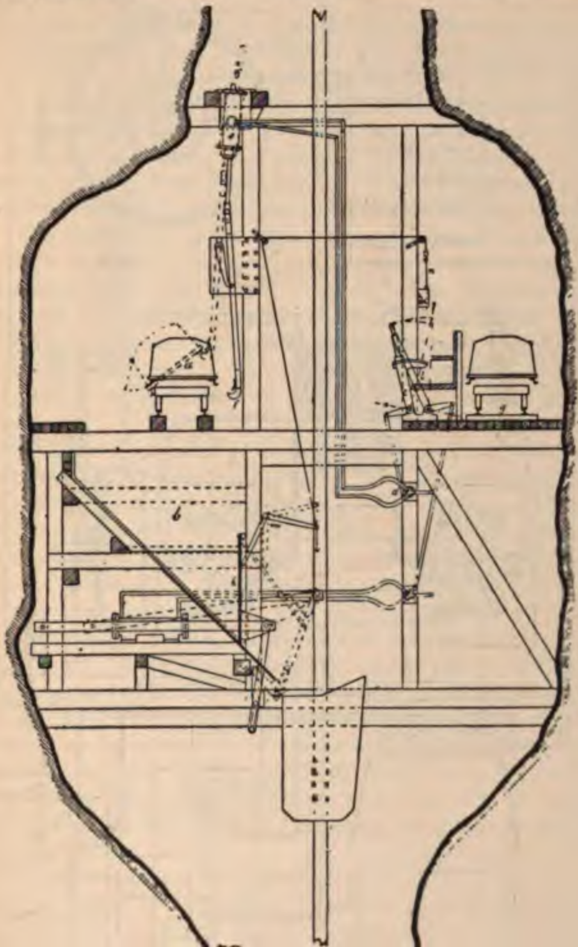
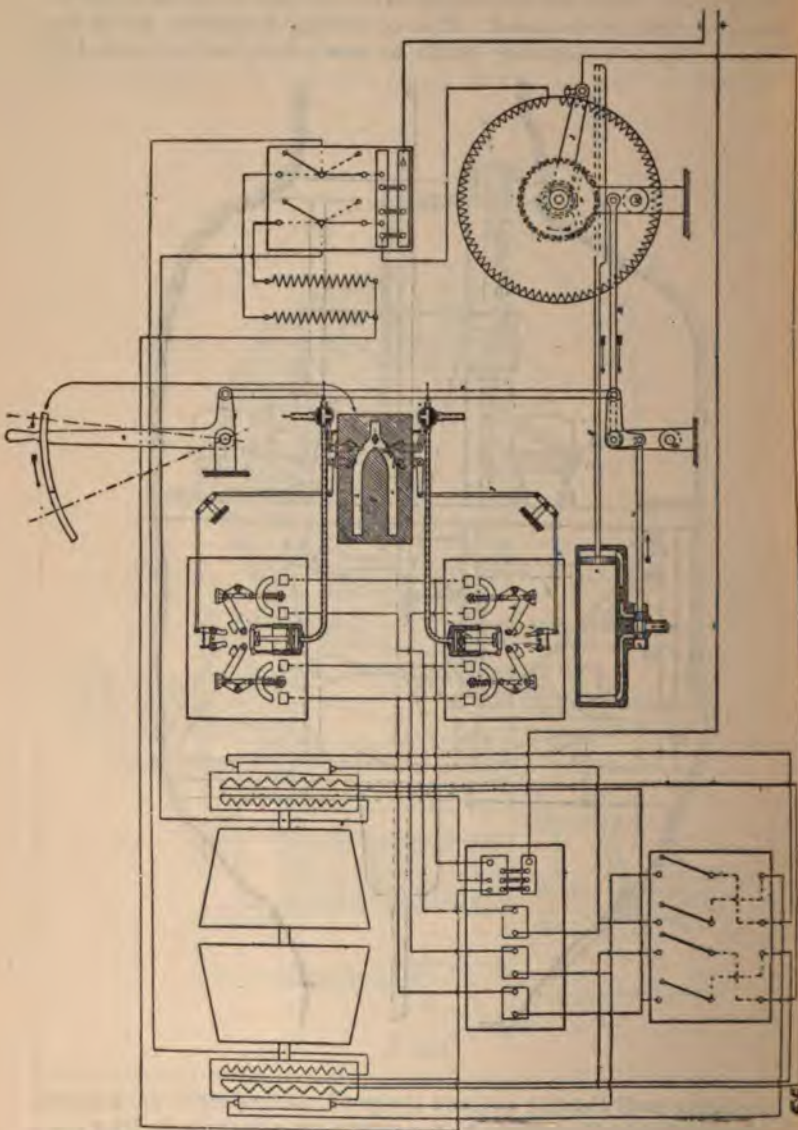


FIG. 2.

thoroughly well thought out and designed, and operates in a most perfect manner. The whole of the operations are controlled by one lever. This lever works in the inverted U-shaped slot in a steel plate, shown in the illustration with cross-sectional lining. The lever is shown *in the "off" position.* The direction of rotation of the motors



and winding drums depends upon which side of the slot the lever is pushed forward in. The movement of the lever from the "off" position into the short tail slot puts on the brake gear. The circuit is always made and broken on two single-pole circuit breakers fitted with magnetic blow-outs.

The rheostat for inserting and cutting out resistance in the armature circuits consists of a large commutator (shown in the top right-hand corner in Fig. 3). This commutator is mounted with the segments vertical in a fixed position; an arm carrying carbon brushes mounted on a vertical axis is moved round the commutator by means of a rack and pinion, and in rotating cuts out the resistance gradually segment by segment. The resistance consists of cast-iron V-shaped strips bolted between the commutator arms, and easily removable should a burn-out occur. The rack, which gears with the pinion keyed on the movable brush arm, is operated by air pressure, the air cylinder being shown on the right-hand side of the circuit breakers below the commutator. A vacuum dash-pot cylinder, not shown in the illustration, is fixed in tandem with the pressure cylinder, and controls the rate of movement of the piston, providing an easy means of adjustment of the length of time which it is desired to elapse for the cutting out of the resistance.

The method of operation is as follows: Forward movement of the lever in either slot causes the lever to catch against a trigger which opens a valve and admits compressed air to a cylinder, which forces the two circuit breakers on that side to the "on" position, thus closing the main circuit. Air pressure is also admitted to the cylinder which operates the rheostat arm by means of a double-piston valve, which is linked up to the starting lever. The rack is then pulled downwards with a slow, uniform motion, and the resistance gradually cut out of the armature circuit. In stopping, the lever on its return movement trips the lower trigger in the slot, and mechanically knocks off the hold-on catches of the two circuit breakers, thus breaking the circuit. It also first moves the bell-crank lever connected to the valve rod of the pressure cylinder and admits the compressed air to the bottom end of the cylinder, which then returns the moving arm of the rheostat quickly to the starting position, inserting all the resistance in the armature circuit.

There is other gearing in connection with this air-pressure cylinder, the action of which is as follows. There is a toothed quadrant also gearing with another pinion on the rheostat arm. This quadrant is connected by a rod to the connecting rod between the starting lever and the bell-crank lever which operates the valve. As the rheostat arm rotates the quadrant is moved in a downward direction, which pushes the connecting-link closer to the fulcrum of the bell-crank lever, which is slotted for this purpose. Having moved the connecting rod closer to the centre, it is evident that a small backward movement of the starting lever will not only move the valve to the "off" position, but will open the valve to admit air pressure to the

bottom side of the piston. In this way the engine-man can contr the speed of winding when approaching the end of each wind, as

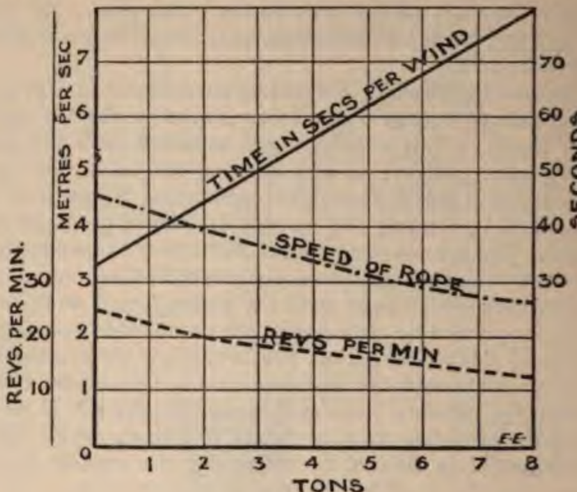


FIG. 4.

can insert as much resistance as required in the armature circu Having moved the starting lever to the "off" position, the operat

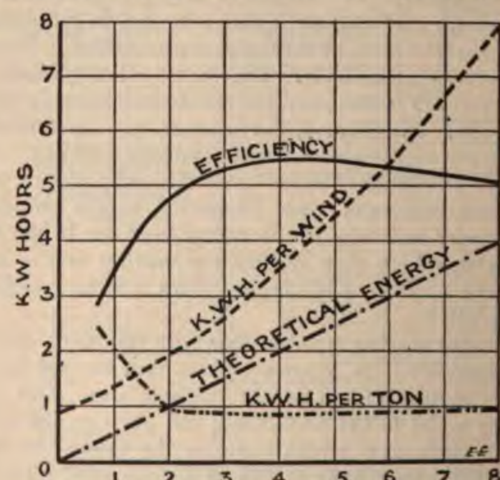


FIG. 5.

can jamb on the brakes by pulling it back into the tail slot, whi opens a valve to admit compressed air to the brake cylinders. Shou

anything fail in this valve gear, a treadle is provided in connection with another valve and air-pressure supply. There are also emergency hand and foot-brakes should the compressed air supply fail. In addition to this, in the event of overwinding from any cause the brakes are put on automatically by the winding drums themselves tripping a heavy-weighted lever.

The air pressure referred to is supplied by a compressor, driven by an induction-motor in the winding-engine house. The compressor is of great size, and supplies compressed air for rock drills and other purposes in the mine at 75 lbs. per square inch. The gear shown in Fig. 1, on the left-hand side of the motor, is for operating the friction-clutch mechanism, when required, through the hollow shaft of the winding-drum. Men are sent down and brought to bank by a

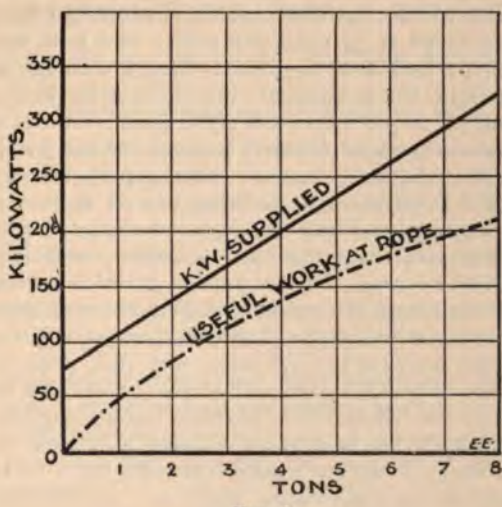


FIG. 6.

separate winding-gear driven by an induction motor. There is also in the winding-engine room a 100-k.w. motor-generator for supplying direct current at 500 volts for the use of the mine locomotives.

The speed of winding is arranged for a mean speed of 10 ft. per second for a shaft depth up to about 900 ft. When the shaft reaches greater depths the motors will be put in parallel and this mean speed doubled. The normal load in the cage bucket is 5 tons of material, but it may be anything up to 7½ tons, owing to the variation in the weight of a given bulk of material, depending on the proportions of iron-ore and stone, etc. The bucket and the unbalanced weight of rope bring this up to a maximum load of 10 tons, upon which calculations have been based. The time for acceleration is about 10 seconds, and the current reaches a maximum of about 1,300 amperes for a few seconds, about one-half of which is supplied by the battery.

Some curves prepared by Mr. Nordenstrom are shown in Figs. 4, 5, and 6, giving—

A. Time in seconds for one wind at various loads.
B. The speed of rope in metres per second at various loads.
C. The revolutions per minute of motors at various loads.
D. The total electrical energy supplied per wind at various loads.
E. The useful work on the ropes at various loads.
F. Kilowatt-hours per ton lifted.
G. Kilowatt-hours per wind supplied to the motors at various loads.
H. Actual kilowatt-hours required at the ropes per wind at various loads.
J. Efficiency curve plotted from G and H.

There are three electric locomotives employed in the mine. Each is equipped with two motors, and capable of exerting a draw-bar pull of 600 lbs. at a speed of 7½ miles per hour. The tubs weigh about 6 cwt., and carry a load of 2½ to 3½ tons. They are usually made up in sets of about eight, but occasionally the locomotives haul sets of 15 tubs. The current at 500 volts is collected from overhead wires supported on insulators spaced about 13 ft. apart, the rails, which are, of course, bonded, serving as the return. Siemens pattern bow collectors are fitted on the locomotives, there being two on each, one for each direction, the leading one being always out of contact. In the limited space in underground roads there is, of course, insufficient room to reverse the collector when the direction of travel is reversed, hence the use of the two bows. There are earthed guard wires run alongside the trolley wires, to minimise the danger of the miners getting a shock from accidentally touching the live wires with rock drills or anything else they may be carrying on their shoulders. In addition to the three locomotives, there are several little portable hauling engines, mounted on trucks, each consisting of a motor coupled to a drum on which a wire rope is coiled. These are used for drawing tubs out of galleries extending beyond the main roads, which are not equipped for the electric locomotives.

I am largely indebted to the engineer of the mine, Mr. Eric Nordenstrom, who designed all the mechanical arrangements, and to Mr. Clayton, the designer of the electrical machinery, for the information contained in this paper.

GLASGOW LOCAL SECTION.

REMOTE CONTROL HIGH-TENSION SWITCHGEAR.

CONTAINING A DESCRIPTION OF THE L.C.C. TRAMWAYS GREENWICH POWER STATION SWITCHGEAR.

By FRANK WALKER, Associate Member.

(*Abstract of Paper read February* 12, 1907.)

General Remarks.—Remote control switchgear for generating stations containing large units, generating extra high-tension current, is of very recent development in this country. A description of the switchgear installed in the new Greenwich generating station of the London County Council may therefore be of interest to many engineers.

Apparatus Controlled.—The principal apparatus for the control of which the switchgear at Greenwich is designed, consists of :—

8 3,500-k.w. generators, four only being now installed. These give 3-phase current, 350 amperes per phase, P.F. 0·9, 6,600 volts between phases, at 25 ∾ per second. The generators are star-connected, the centre point of the star being earthed.

28 6,600-volt feeders, to sub-stations outside generating station.

2 6,600-volt feeders, to sub-stations inside generating station.

2 6,600-volt feeders transformed to 220 volts, for operating induction motors driving station auxiliaries.

2 125 volts D.C. generators, each giving 1,500 amperes.

1 battery giving 550-volt or 125-volt supply.

4 500-k.w. 3-phase induction motors, coupled to D.C. generators, each capable of giving 900 amperes, 550 volts for supplying local tramway service.

General Arrangement of Switchgear.—The switchgear is arranged on two galleries, which run parallel to the engine-room; the centre line of the engine-room being also the centre line of the galleries. Plate No. 1 shows plans of the galleries, and Plate No. 2 shows a section through them, and also elevations of the apparatus arranged on the wall. Plate No. 1, Fig. 1, shows the top gallery which contains all

the E.H.T. busbars and main E.H.T. switches, together with isolating plugs, and spark-gaps for discharging any excess pressure rise which may occur in the cables.

Plate No. 1, Fig. 2, shows the plan of the lower gallery. Against the wall the positions are marked of the E.H.T. generator and feeder cables, instrument transformers, etc., front and sectional elevations of these being given on Plate No. 2. A 46 panel L.T. board is shown, enclosed by a wall which is of glazed brick and is 8 ft. 6 ins. high. Within this enclosing wall, the position is indicated of the 6,250-k.w. transformers, which give 220 volts, 3-phase supply for operating station auxiliaries, and also the L.T. switchboard panels controlling this.

Plate No. 4 shows the general scheme of E.H.T. connections. The current is led from each generator, through its instrument transformer tank, to a main generator switch; from this switch it is led to a section switch. This controls a set of section busbars which supply a group of four feeders. Each feeder is supplied through a main switch and instrument transformer tank. Spark-gaps are connected to each feeder cable, for discharging any excess pressure that may occur. Main busbars are provided in two parts, which can be interconnected by means of a switch.

The normal method of running will be to have one or more generators feeding the main busbars, which will be interconnected, and to which all the feeders will be connected in parallel.

By withdrawing the isolating plugs connecting it to the main busbars, any generator may be isolated therefrom, and remain connected only to one set of section busbars supplying four feeders.

It will be seen that the scheme, generally, is simple and systematic. The switches are arranged with respect to the busbars so that the minimum length of connecting cable is required; and the apparatus is so subdivided and arranged that in case of a breakdown the damage will be confined to a small area, and cannot possibly spread and cripple the whole gear.

Referring again to Plate No. 1, Fig. 1 shows in dotted lines the E.H.T. cables interconnecting the various switch structures, and also the main generator and feeder cables. The arrangement of switches is such that these latter cables run in the most simple and direct manner possible to the chases provided for them in the face of the wall, without any crossing of each other.

Remote Control.—When the magnitude of the work to be done by the main switches is considered, it will be evident that these must be of substantial design. In an emergency, it is possible that one of the switches may be called upon to break the short circuit of the entire station plant. There are 49 switches arranged on the top gallery, which is approximately 120 ft. long by 30 ft. broad.

Electrical control is accomplished by the very easy motion of a 3-in. handle through 60°. This insures rapid closing and opening, with absolute certainty and decision, and the smart response which is neces-

sary when synchronising. The action is a very simple one for the operator to perform, leaving his attention free to observe the instruments.

A comparison on Plate No. 1 of the relative space taken up by the switches on the top gallery and the control desk on the lower gallery, will show the gain made by concentration of controlling gear in case of observation and rapid manipulation. The E.M.F. of the current used for control purposes is 125 volts, and this is obtained from a battery. The switchgear control is thus independent of any machinery breakdown. Only low-tension current is brought to the control desk. Consequently in case of accident to the E.H.T. gear the operator is not liable to get confused, and can disconnect the defective parts without danger.

The entire separation of low-tension from the extra high-tension circuits enables the latter to be properly arranged in fireproof structures.

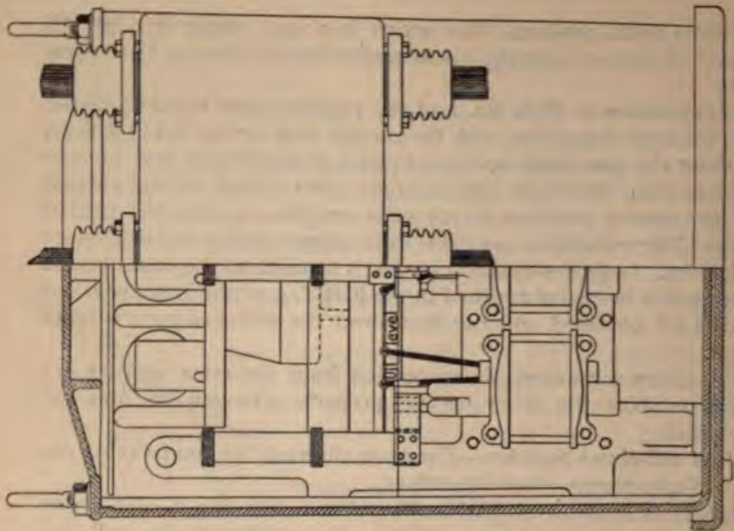
Main Oil-Break Switches.—The main oil-break tension switches are of the Westinghouse standard pattern.

Apparatus Mounted on Wall.—Plate No. 1, Fig. No. 1, also shows the cable runs from the feeder and generator switches to the wall, and indicates that the spark-gaps and instrument transformer tanks, etc., as shown on Plate No. 2, are in direct alignment with these positions.

Commencing at the lower gallery floor level, there is, first, a trifurcating box for every generator cable or outgoing feeder cable ; from these boxes the tails are led through porcelain bushings to isolating plugs.

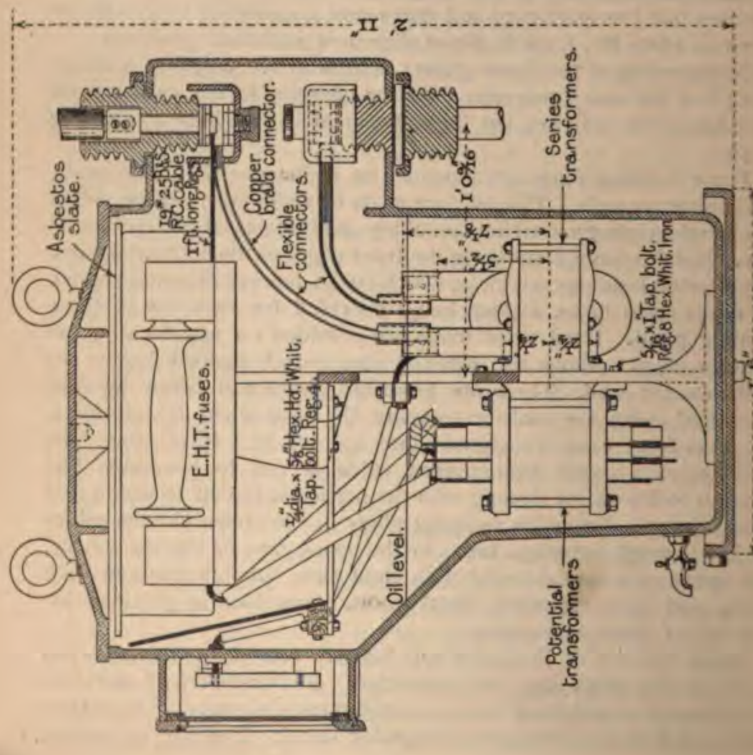
These isolating plugs are mounted on white enamelled slate bases, and in separate cells. The cells are made by sliding vertical enamelled slate barriers into grooved horizontal top and bottom slabs of enamelled slate. Metal beadings run along the front edges of the horizontal slabs. Fitted to the beadings are locks, which fasten doors of expanded metal. All doors have locks, a door being provided for each set of three isolating plugs. Hard wood tongs are provided for withdrawing the isolating plugs. Above the isolating plugs, steel channel frames are built into the wall. These are protected above and below by slate slabs, and carry the tanks containing the instrument transformers. The tails from each transformer tank are led to a trifurcating box, from which a 3-core lead-covered cable is run to the cable box adjacent to the switch dealing with the particular circuit to which that cable belongs. From the isolating plugs in the feeder circuits cables are led through porcelain tubes to the spark-gaps on the top gallery. The spark-gaps are arranged with stout slate barriers between each phase, and have expanded metal doors. One door is provided for each set of three spark-gaps.

Along the top of the spark-gap barriers runs an earthing bar, to which all the spark-gaps are connected. All framework of switches, switchboards, transformer tanks, expanded metal doors, etc., is earthed to this common bar, thus guarding against shock. The bar, in its turn,



REAR ELEVATION.

Fig. 4.



SIDE ELEVATION.

Fig. 3.

Instrument Transformer Tank.

is connected by three separate leads to the main condensing water-pipes.

Instrument Transformer Tanks.—These are shown in Figs. 3 and 4.

Each tank contains three series and two shunt transformers, together with three H.T. fuses for the latter. These transformers all stand in oil.

From their secondary terminals two 3-core cables are taken through steel conduit to the control desk and instrument board. They are there connected to the various ammeters, wattmeters, relays, etc., belonging to the circuit in which that particular tank is placed. By this means all H.T. current is kept away from the control desk.

The arrangement is a very compact one, and as the main H.T. cables are sweated to terminals in the tank, which are in line with each other, no deviation is made in the alignment of the cables.

The transformers are all fixed to a cast-iron frame, which can be readily withdrawn from the tank. The entire tank can very easily be disconnected, and entirely removed or replaced.

Spark-Gaps.—These are connected in each feeder circuit, to discharge any excess pressure rise above a set value.

Their general arrangement is shown in Plate No. 2. The apparatus consists of a porcelain isolating plug, series and shunted spark-gaps, and series and shunt resistances. These are mounted on white marble, which, in turn, is insulated from the wall by porcelain insulators. The gaps are made up of milled or knurled cylinders of non-arcing metal, placed side by side, and separated by air-gaps. They are held in position by porcelain pieces. The operation of the apparatus is as follows :—

If the potential in the cable rise sufficiently high, the series gaps will be sparked across. If the discharge be sufficiently high, it will then meet opposition in the shunt resistance, and pass over the shunted gaps to earth, through the series resistance. The arc, which tends to follow the discharge, is then withdrawn from the shunted gaps by the shunt resistance, and is suppressed by the series gaps aided by both resistances.

E.H.T. Switch Structures.—Plate No. 5 shows in detail one of the E.H.T. switch structures on the top gallery.

A reference to Plate No. 2 will show, in the section through the two galleries, the position of the feeder switch structures, relative to other E.H.T. gear. On the steel joists supporting the floor angle irons are placed across openings left in the floor for the special trifurcating boxes tailing the 3-core cables to the main switches. These 3-core cables enter the box below the floor, whilst the tails are led out within the 6-in. space above it. Moulded bricks serve the purpose of carrying porcelain insulators, through which the tails from the cable boxes are led. These tail cables are heavily insulated with rubber. From the cable box to the moulded bricks, 2½-in. cement barriers are placed between the tails, and beyond the moulded bricks the cables are in cells, separated by 4½-in. brick walls. The cables

pass up the vertical wall of the structure, through porcelain insulators, which are set within separate glazed earthenware ducts. They are thus entirely isolated from each other, and are surrounded by non-inflammable material till close to the switch terminals, thus reducing fire risks to a minimum. The isolating plugs for each phase are separated by 4½-in. walls. They are similar to those already described in all respects. Where necessary, a special union connection is provided at the top terminal, to enable the cable to be readily connected or disconnected. The detail view (Fig. 5) of the busbar connection shows that this is made by means of a terminal, into which the connecting cable is sweated and screwed, and which is fitted into a conical hole in the busbars. Lock nuts are provided to prevent slacking back. Between the terminals the busbars are wrapped with tape, then with insulating cloth to $\frac{1}{16}$ in. radial depth; an outer layer of tape, and then a final wrapping of torpedo twine. The terminal points are guarded by coverings of moulded ebonite. The busbars at these points are supported, as shown, by porcelain insulators; these insulators have ample creeping surface, and are cemented into thick slate slabs, which form the bottom of the busbar chambers. The covering of these chambers is also of slate slabs, all joints between slates being sealed to prevent any dust entering.

The structure proper is of white glazed bricks laid with cement. These will be very easily kept clean and free from dust. Slate slabs are built in wherever necessary to support the porcelain insulators and glazed earthenware ducts. Moulded bricks or stones are built in, having holes provided for rag bolts for fixing isolating plugs. A steel channel frame is also built in to receive the bolts fixing the main switches.

A beading across the top and a steel channel across the bottom of the isolating plug cells are fixed so that expanded metal doors, which are provided for every set of three cells, may be securely locked.

Wherever single-core rubber insulated cables are used for connecting between one E.H.T. switch structure and another, these cables are run in porcelain insulators supported in glazed earthenware ducts, as already shown within the wall of the feeder structure, except that these ducts run horizontally and are placed under false floor, the surrounding spaces being filled in with cement and breeze. It will thus be seen that except at the switch terminals the rubber insulated E.H.T. cables on the top gallery are, throughout their entire lengths, surrounded and isolated from each other by substantial barriers of non-inflammable and insulating material.

Control Desk.—Plate No. 3 shows the general appearance, together with that of the instrument board, and their relative positions. It will be seen that the operator faces the engine-room. The instrument board being fixed in front of, and higher than the control desk, the operator can read his instruments, operate his switches, and see what is going on in the engine-room without turning round.

All the controlling switches, measuring and indicating instruments etc., as well as automatic safety devices, are grouped on these two boards.

The desk is approximately 60 ft. long. The panels are of white marble, fixed to a frame made up of wrought-iron base, vertical castings, and top wrought-iron tie straps. Every panel is distinctly engraved with the name of the circuit or circuits controlled therefrom. No current used about the desk or instrument board has higher voltage than 220. None of the metal work outside the desk is alive, so that it is impossible for the operator to get any shock. There are eight panels each controlling one generator, sixteen panels each controlling two

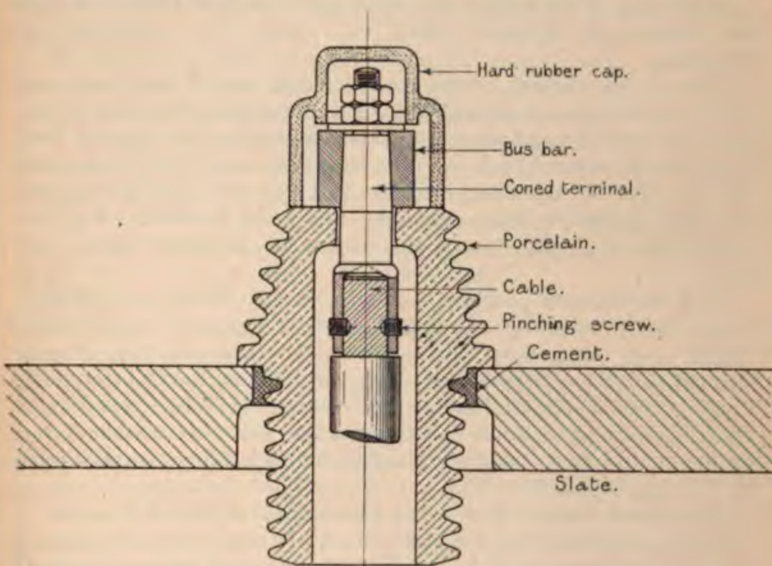


FIG. 5.—High-Tension Busbar Terminal.

feeders, and four panels each controlling two sectional groups of four feeders. Each generator panel has one, and each section and feeder panel two small controlling switches, together with their red and green indicating lamps, as already referred to in connection with the main switches.

Each generator panel has also the following apparatus, namely: A switch which operates the engine governors, a bell-push to operate a signal bell; another bell-push having a thin glass cover which must be smashed before the push can be operated, and then an electro-magnetic device can be actuated which stops the engine; also three sockets for synchronising. Two plugs only are provided for the entire desk, these having prongs at different distances, the sockets being

spaced equally. This arrangement makes it impossible to short-circuit two machines through the synchronising busbars.

A handwheel is provided for operating the resistance in the generator field circuit, with an indicator having sixty divisions, and a pointer, suitably geared to travel over the divisions in correspondence with the motion of the resistance switch contact brush over its sixty contacts. Finally, each generator panel has six switches for signalling. By means of these, various signals:—"Slow," "Run up," "On load," "Reduce," "No. 1," "No. 2," "No. 3," "No. 4," "A," "B," can be illuminated on a telegraph board. This is placed conveniently to be seen from all required parts of the station.

All wiring of the control desk is brought to terminal boards, which are conveniently arranged under each panel for connecting up purposes.

Through the centre of the control desk, one of the main roof stanchions passes, and around this a frame has been built for panels, on which switches and fuses are placed, controlling the 125-volt D.C. supply to the control desk for operating the main E.H.T. switches; also fuses for the signalling circuits; and a control switch to operate the E.H.T. switch interconnecting the main busbars. A glass-fronted cover is placed over this controller to guard against improper use.

The instruments provided for each feeder circuit are: Power-factor meter, indicating wattmeter and A.C. ammeter on instrument board; and integrating wattmeter with overload, inverse time element, relay for tripping the main switch, on the control desk. Each generator circuit has on the instrument board, power-factor meter, indicating wattmeter, A.C. ammeter, and D.C. ammeter, and on the control desk, integrating wattmeter and reverse current relay for tripping the main switch.

Instrument Board.—The instrument board is shown on Plate No. 3, and in the sectional view on Plate No. 2. It consists of steel frames each approximately 29 ft. long. These frames are supported on felt discs at each end and at their centres. On each frame fourteen black enamelled slate panels, each 3 ft. × 2 ft., are mounted, and on these the instruments are fixed.

Conduit System.—All the operating leads from the 49 main switches, and secondary leads from the 44 instrument transformer tanks to the control desk, and the leads from thence to the signalling boards, engine synchroscopes, etc., are run in steel conduit.

The well-being of these operating and transformer leads is essential to the success of the station, and it may be well to state some of the points kept in mind in designing the conduit system, and in its erection. The conduit itself is of extra heavy solid drawn tube, screwed with standard Whitworth gas-pipe threads.

Incidentally I should like to deprecate the practice of some conduit makers, of drawing the conduit from old and more or less rusty tubes taken from locomotives, etc. A slight trace of rust prevents the

enamel properly adhering. The enamel, besides being insulating, and acid and moisture proof, should be really flexible. Out of nine samples tested by the author, only one piece of conduit proved to be coated with enamel worth calling flexible. All conduit and fittings should be stoved after enamelling. The conduit and fittings should all be bought with the threads entirely free from enamel, and care should be taken to have all ends cut square, all burrs removed, and all the threaded parts long enough to allow the ends to be butted inside the couplings.

The conduits are of ample size for the leads they contain, the runs are short, frequent inspection boxes being provided, the majority of these being specially designed. In the boxes barriers are provided, to keep the leads belonging to separate circuits apart from each other as far as possible.

No elbows are used: where possible, the pipes are bent with a long sweep, and where space forbids this, standard bends are used.

The pipes are arranged systematically, and as simply as possible. Each pipe has a number painted on it, and by means of a key plan referring to these numbers the conduit containing the leads belonging to any particular circuit can be readily located.

The use of fire in bending the conduits should be forbidden, as it burns off all the enamel both internally and externally. The runs should be designed to make this unnecessary.

Switchgear for Six 250-k.w. Static Transformers.—Two of the E.H.T. feeders supply current to six 250-k.w. transformers, from which 220-volt, 3-phase current is obtained for operating the station auxiliaries. The arrangement of these transformers and of the switchgear in connection with them will be seen from Plate No. 6. The E.H.T. cables are led to cable boxes, and thence, through isolating plugs, to busbars. Beneath these busbars six openings for the transformers are provided, fitted with spring-locked Bostwick gates.

Heavy copper bars are placed across the front of the transformer openings, and the secondaries are connected to these through fuses. These copper bars terminate at the bottom contact of heavy switches mounted on panels, as shown, the switches having removable blades.

Each panel is provided with a guarded controlling switch and indicating lamp, for tripping the main switch controlling that particular feeder; also with three ammeters, 0–2,000 amperes, and one voltmeter.

Openings, covered with expanded metal, are left in the floor beneath the transformers for ventilation purposes. The transformers are mounted on wheels, and can be wheeled along steel joists let in the floor, from their positions, when required.

Instruments.—The reverse-current relays are similar in appearance to induction integrating wattmeters. They operate on the same principle, and in consequence retain the sensitiveness of those instruments. Adjustable contacts for the tripping circuit are arranged to vary the amount of reverse current at which it is desired to operate.

The overload relays are worked by the series transformers in each phase, the secondary current passing round the windings of a magnet,

between the poles of which is a copper disc, capable of rotating. On the same spindle as this disc is a small drum to which a weight is attached by a fine cord. Normally the disc is stationary, the tendency to rotate being balanced by the moment of the weight. On an overload, the disc rotates and winds up the weight, which closes an auxiliary circuit, tripping the main switch. The ratio of rotation is proportional to the current following in the main circuit, and the time lag may be adjusted by altering the length of the cord.

The arrangement is very sensitive and is found to work well.

Switchgear in Sub-station.—The switchgear for the sub-station located within the main generating station is shown in plan on Plate No. 7.

E.H.T. current is supplied by two of the main feeders through isolating plugs to a set of busbars; from these, through isolating plugs, to oil switches. These are operated from the front of the switchboard, by hand, through rods and bell-crank levers.

From these switches cables run to the instrument transformer tanks, and thence to the E.H.T. induction motors.

The oil switches are of the hand-operated type, and are arranged to be tripped electrically from several positions in the sub-station.

The general arrangement of the E.H.T. apparatus within its masonry is similar to that already described.

The E.H.T. induction motors are coupled to D.C. generators, each capable of giving 900 amperes at 550 volts.

There are nine switchboard panels, four each fitted with E.H.T. switch handle, ammeter, P.F. meter and indicating wattmeter. Two of these panels have guarded control switches with indicating lamps for tripping the main switches of the feeders supplying the sub-station.

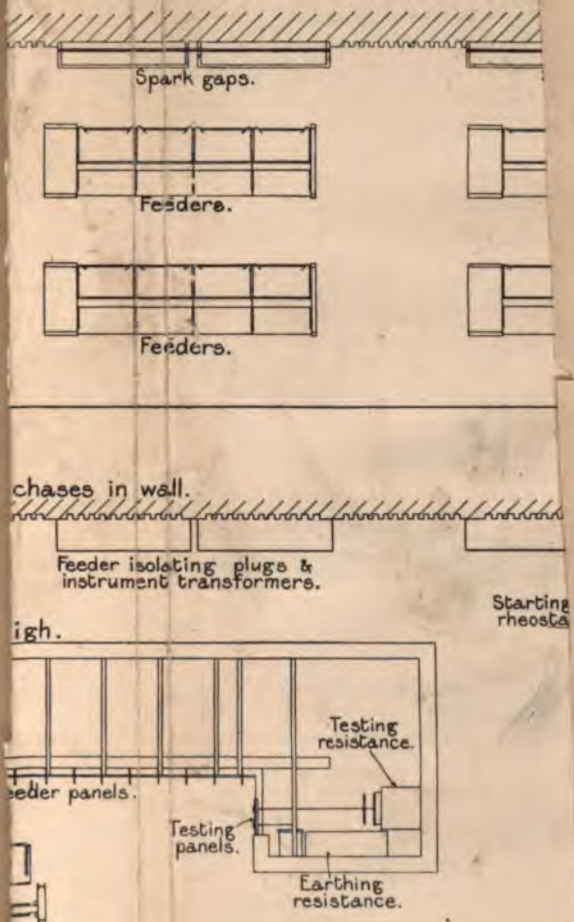
There are four L.T. panels controlling the sub-station D.C. generators. Each has reverse-current, loose-handle circuit-breaker, ammeter, two knife switches, and a circular change-over switch, having a starting and a running position.

The ninth panel is a common starting panel. In front of the switchboard a pillar is erected having a handwheel and shaft, geared to operate a starting faceplate and resistance, which is mounted behind the switchboard.

The connections are arranged so that by putting the change-over switch on its starting position, and closing the switch and circuit-breaker on the common starting panel, any of the sub-station sets can be run up through this common D.C. starting arrangement.

The British Westinghouse Electric and Manufacturing Company, Ltd., were responsible for the whole of the switchgear, which was arranged exactly in accordance with complete and detailed designs prepared by Mr. J. H. Rider, Chief Electrical Engineer to the London County Council Tramways.

6



Spark gaps.
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Feeders.
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Testing panels.
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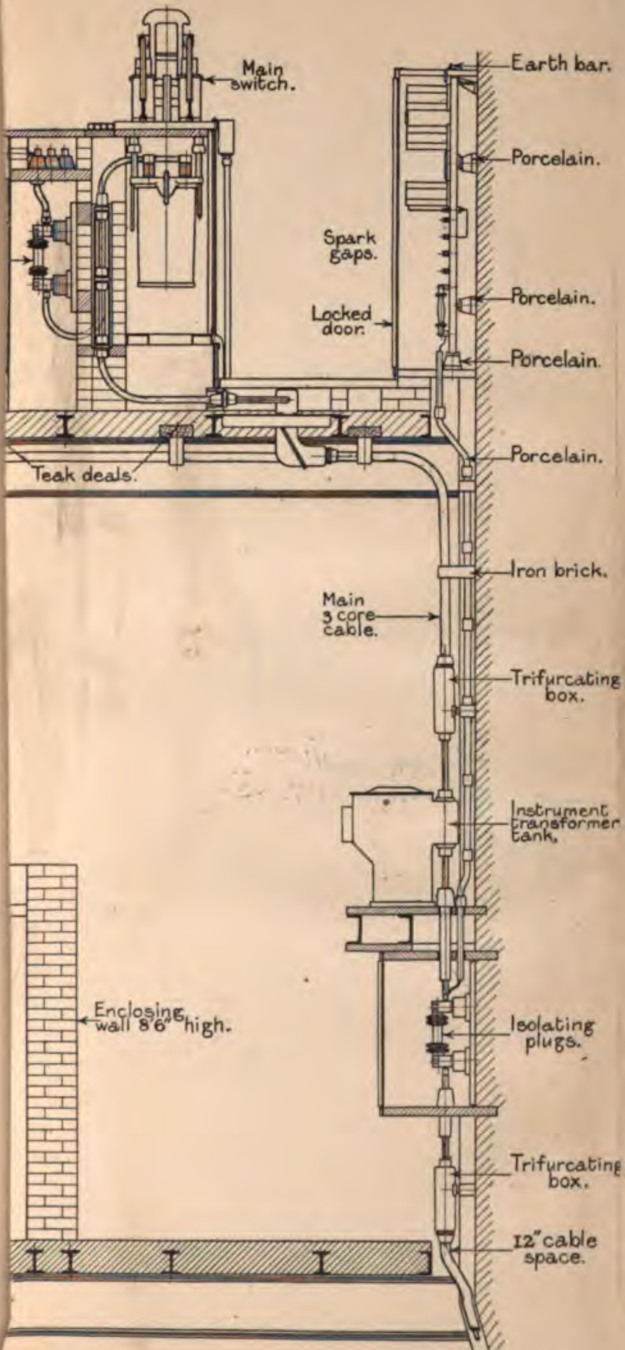
Main switch.
Earth bar.
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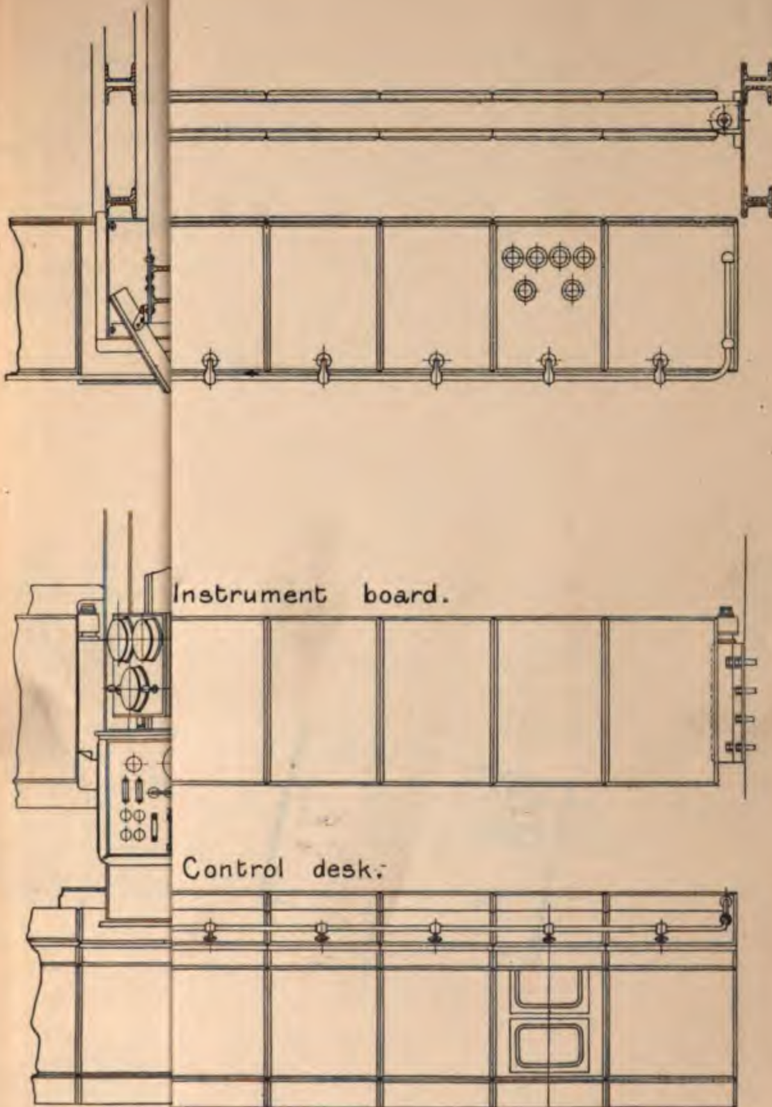
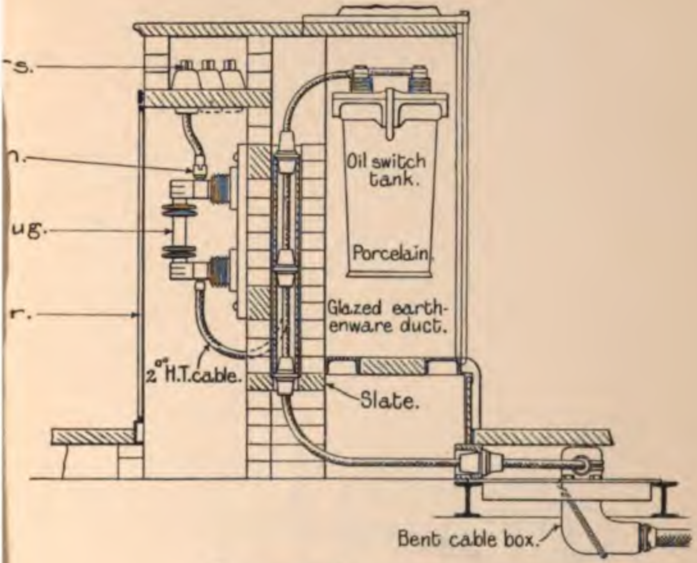
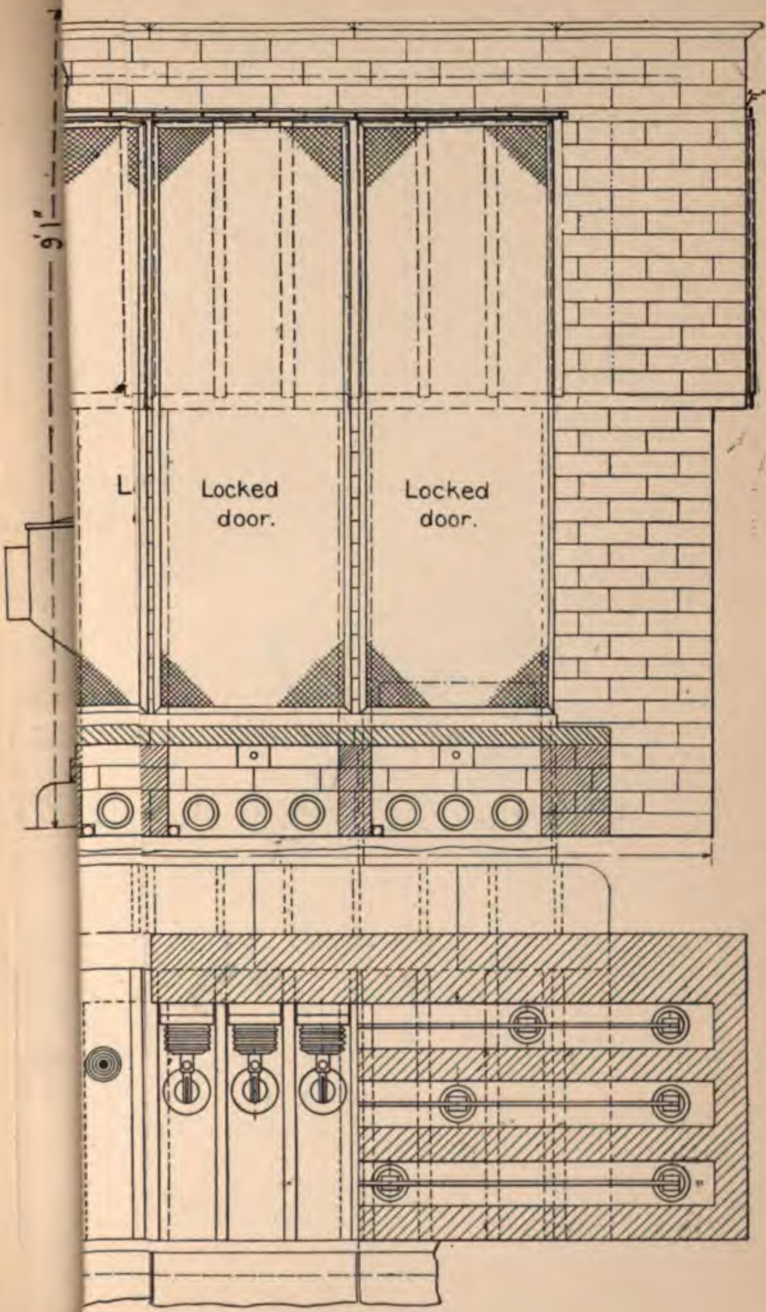
Spark gaps.
Porcelain.
Locked door.
Porcelain.
Teak deals.
Porcelain.
Iron brick.
Main 3 core cable.
Trifurcating box.
Instrument transformer tank.
Enclosing wall 8'6" high.
Isolating plugs.
Trifurcating box.
12" cable space.

H GALLERIES.


Instrument board.
Control desk.



SECTION THRO' FEEDER SWITCH.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3

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1906–1907.

EXPLANATION OF ABBREVIATIONS.

[P] signifies a reference to the general title or subject of a Paper.

[*p*] signifies a reference to a subject incidentally introduced into a Paper.

[D] signifies a reference to remarks made in a Discussion upon a Paper, of which the general title or subject is quoted.

[*d*] signifies a reference to remarks incidentally introduced into a discussion on a Paper.

Note.—The lists of speakers in the Discussion upon any Paper will be found in the Table of Contents at the beginning of the volume.

A.

B.

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